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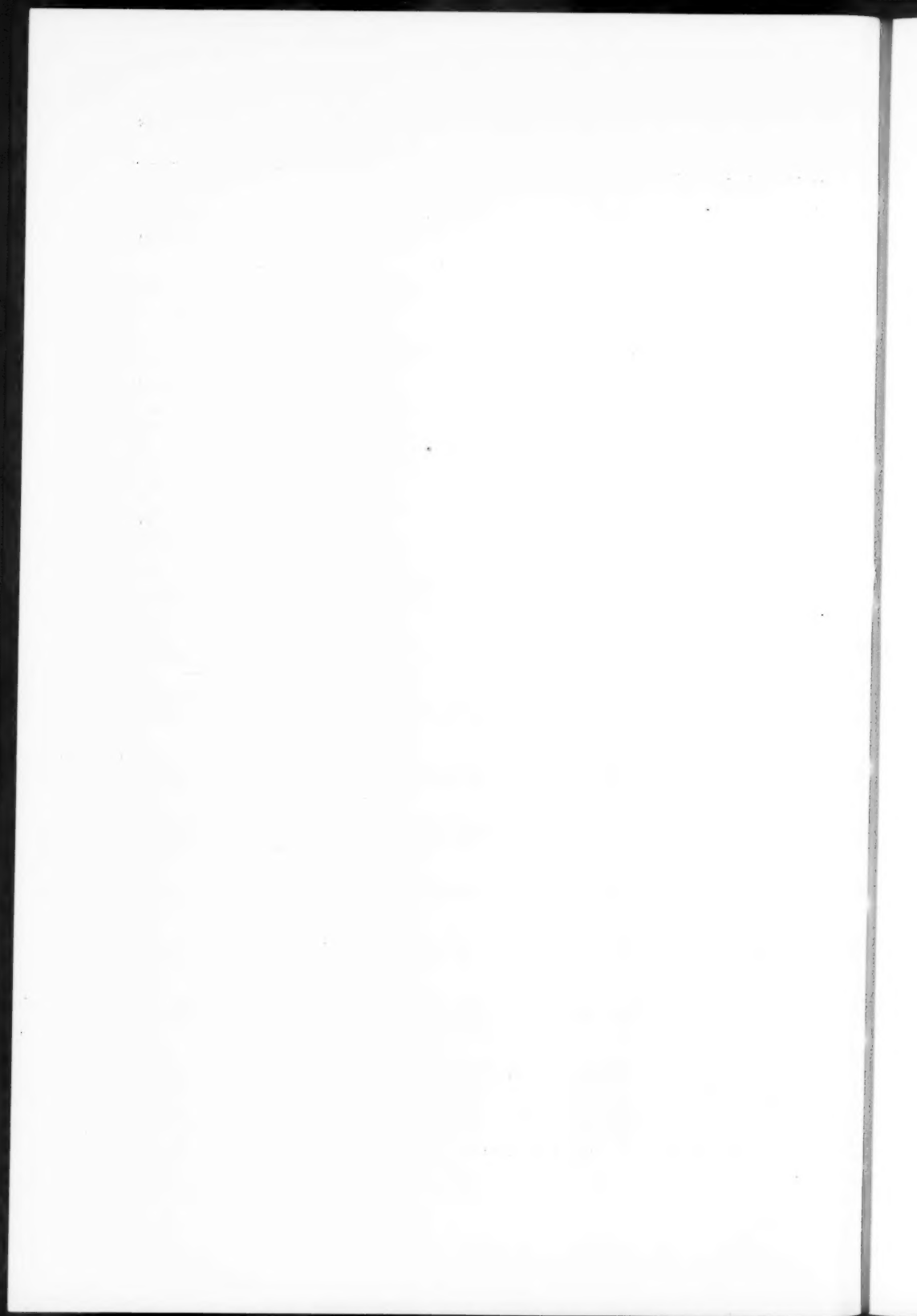
SECTIONS:—

OBSTETRICS AND GYNÆCOLOGY ODONTOLOGY OPHTHALMOLOGY
ORTHOPÆDICS OTOTOLOGY PATHOLOGY PSYCHIATRY
SURGERY, WITH SUB-SECTION: PROCTOLOGY
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Section of Obstetrics and Gynæcology.

President—Dr. H. RUSSELL ANDREWS.

DISCUSSION ON "THE NOTIFICATION OF PUERPERAL SEPSIS."

(With the Section of Epidemiology and State Medicine and the Society of Medical Officers of Health.)

Dr. H. RUSSELL ANDREWS (President; Chairman of the Combined Meeting).

I PRESUME that most of you have read the leaflet which explains the calling of this combined meeting. The Section of Obstetrics and Gynæcology has been asked by the Ministry of Health to give advice as to "the administrative action which should be taken to reduce the incidence of maternal morbidity and particularly of puerperal sepsis, especially with regard to the terminology and definition of puerperal fever as may lead to the prompt notification of the occurrence of this morbid process, so that adequate nursing and treatment may be ensured." The matter was discussed by the Council of the Section, referred to a Sub-committee twice, and discussed again by the Council. Finally, it was decided to hold this combined meeting, so that an expression of opinion could be gained from those interested in every aspect of the subject.

The subject is one of great importance and must be of interest to everyone in this room. We are probably all agreed that the present position of notification of puerperal sepsis is most unsatisfactory. Many poor patients suffering from puerperal sepsis are left without adequate treatment and the cases are not notified unless death seems to be imminent, and not always then. I should like to read a very short quotation from a lecture given this year by Dr. Fothergill, of Manchester. "In 1911-1914 out of 204 areas, in ninety-three the total of deaths from 'puerperal fever' was larger than the total of notifications." Unfortunately there is a widely prevalent idea that if a doctor admits that he has a case of puerperal sepsis under his care he lays himself open to be blamed for the occurrence of the sepsis and so he avoids reporting it if possible. The result of his notification, or reporting that he has a case of puerperal sepsis, ought to be a telephonic communication, "How can we help you? Would you like your patient moved to an institution, or would you like extra nursing provided? Would you like a consultation with someone who is accustomed to treat such cases?" And so on. If this could be done the objection to notification would be done away with. But it cannot be done at present, because, among other things, there is not a sufficient number of beds in institutions into which patients suffering from puerperal sepsis can be admitted. One of the results of this meeting ought to be the provision of beds for septic puerperal women. Meanwhile, cases that are sent to hospitals are often sent too late for treatment

2 Andrews—Fairbairn: *The Notification of Puerperal Sepsis*

(chiefly adequate nursing) to be of value. The suggestion about notification printed on the leaflet, which is certain to be much criticized, was drawn up with the idea of pressing the importance of treatment being begun early. If it were complied with, with an adequate service at the call of the authorities, some patients would have lost all their adverse symptoms before the doctor in charge was communicated with, but, on the other hand, a great many patients would be put in a fair way to recover before they had become dangerously ill. Destructive criticism is easy, in fact every member of the Sub-committees could criticize the suggestion, but what we want is constructive criticism. The subject bristles with difficulties, many of which will be pointed out by speakers to-night, but it *must be tackled* for the sake of the thousands of women whose lives are endangered or whose health is ruined and activity curtailed by puerperal sepsis. It is not likely that one evening's discussion will be enough to provide a final report to the Ministry of Health, but the interchange of ideas ought to give us something to act upon as a foundation for the final reply.

Sir Francis Champneys has written to me expressing his regret that he cannot come to-night. He says, "I would suggest a resolution to be sent to the Ministry of Health that arrangements ought to be made whereby the notification of all fatal cases—or all cases of puerperal fever notifiable (if preferred)—should be accompanied by a statement *who actually delivered the case*. I (Sir F. C.) have urged this for years. Until we know this we cannot be on the track of the cause."

Our discussion must be limited to "administrative action, terminology, definition, and notification"—quite enough for one evening! We cannot discuss causation, prevention, or treatment. Ample opportunity for discussion of these subjects will be provided at the meetings of the British Congress of Obstetrics and Gynaecology in this building in April, 1925.

Dr. J. S. FAIRBAIRN.

To compress what I have to say within the limits allowed, I will confine my remarks almost entirely to two points: (1) What should be notified, and (2) the pros and cons of notification.

There will probably be a divergence of opinion in this meeting regarding the definition of what should be notified equal to that among the members of the Sub-committee, i.e., as many opinions as individuals present.

Support will surely be found, however, for the general principle that notification is useless unless the early and suspicious cases are included as well as those of undoubted puerperal sepsis. The Scottish Departmental Committee considered the matter, but feared to make any definite recommendation. The one suggested by the Sub-committee, to include early and suspect cases, is before you—"Rigor or temperature of 102° or over for twenty-four hours"; it was the shortest and most comprehensive that could be arrived at; if anyone here thinks he can devise a better, let him submit his effort to the meeting to see if it meets with a wider acceptance.

For my own part I have found it helpful, and perhaps others may find it so too, to bear in mind the type of case that we would consider a danger to nurse in a ward with other lying-in patients and the usual hospital procedure in dealing with it. True, the risk of contagion to others is very much less when the patients are already isolated in their own homes, though there is even there the risk of conveyance of infection to others by doctors, midwives and maternity nurses; notification also concerns patients delivered in nursing homes and maternity hospitals of all sorts and kinds. In domiciliary practice, nursing homes and in many of the small maternity homes that have sprung up of late, there are not the facilities for the thorough investigation of suspect cases that a big hospital offers, so that the greater delay in reaching a definite diagnosis may be set off against the less risk of spread.

In other words, we shall not be far out if we take as our guide the conditions that might be regarded as necessitating isolation and special investigation if they occurred in a patient in a lying-in ward. Though many of the cases coming under the suggested definition will turn out to be infections of the urinary tract or other non-genital areas, and in many of the remainder the fever will subside rapidly without further complication, I think most of my obstetric colleagues would agree that they would have all such cases forthwith moved into the observation ward; delay is not worth the risk involved.

In case this method of putting it may not appeal to all, I will set the argument out in another way. Notification only comes in when our first line of defence—prevention—has been breached; the next line to be held is that of catching the disease early and scotching it, and that is only possible if the suspicious cases are included. Treatment is likely to be most effective in the early stages, and if notification is to be preventive of further spread, a trifling and transient pyrexia, which may be the start of an outbreak of more serious infection, should not be excluded.

I take it that I am justified in assuming that I have now put the case for the notification of suspect cases so clearly that the meeting is with me, and that the problem left to it is merely that of framing a better definition than the one suggested.

I will, therefore, proceed to the next point, the discussion of what can be said for and against a notification of puerperal sepsis wide enough to include the suspicious as well as the cases definitely diagnosed as such.

In a characteristic *obiter dictum*, Professor Fothergill has remarked on the curious fascination notification possesses for some minds. The best way to escape being fascinated by it is to look on it as a means to an end, and that end the reduction of the septic death-rate in childbed, and to consider how it may be made to lead up to that end.

The administrative action that must follow the receipt of a notification falls into two categories, according as it concerns the individual case notified and as it concerns the public. In the first category come the measures for determining the nature of the infection, the nursing and treatment of the patient, and in the second those for tracing the cause of the infection, preventing its spread and collating the results obtained.

When the patient can afford such special services as a second medical opinion (which should be one with obstetric experience), bacteriological investigation and adequate nursing, the duty of the health officials will be to satisfy themselves that such facilities have been made use of and to be informed of any other particulars they may require, such as the results of examination and treatment. If, on the other hand, from local conditions or the economic position of the patient, full investigation is not possible then arrangements should be made to provide these services, and, so far as can be, the policy should be to encourage and develop such local provision as is already in existence, both in the case of those able to pay and those who are not. The creation of new officials without regard to the interests of those already trying to provide such services, whether private practitioners or voluntary hospitals, will in the long run be detrimental to the best interests of the maternity service as a whole.

By this means the cases notified as suspect will be narrowed down to those of true genital infection calling for special management and precautions to prevent its being carried to others. The care of the individual patient must, however, be kept in the foreground and the measures for the public health and for obtaining information regarding causation made as little obtrusive as possible. Thus, special medical advice, nursing or removal to hospital, or other provision clearly to the benefit of the patient must be the chief consequence of notification. Though removal to hospital cannot be compelled it must be urged whenever the chances of the patient

will be increased thereby, and if proper provision for the removal and reception of the patient is made, its benefits will soon be generally recognized and little persuasion needed. Provision for those patients requiring, but hitherto unable to obtain, special investigation and treatment and the measures taken to limit conveyance of infection will be acknowledged as all to the good. Also, if notification can be made to lead to an increased knowledge of the disease, it will with equal certainty lead to its lessened incidence, and hence information collected in this way from areas in which success has or has not been attained, and of the factors underlying such success or failure, is bound to be of value. I would emphasize this side as possibly the one that is likely to be most fruitful in the prevention of puerperal sepsis.

But doubt must arise in the minds of many whether the notification of all suspicious cases is a possibility and, even if it is, whether it will make much impression on the incidence of the disease. In vast numbers of confinements temperatures are not taken and recorded properly and whatever standard is adopted there are numbers of mild infections which will escape and yet may develop into serious infections or be a danger to others. Foul venereal warts, vulval ulceration or vaginal discharge may cause unexpectedly little disturbance to the affected patient during her lying-in, but be virulently infective to others, and indeed, septic conditions of all kinds in man, woman or child, may be the starting point of genital infections in childbed, so why single out only the puerperal infections? In all infections it is the carriers, the ambulatory and unrecognized cases, that are the greatest danger, and one has only to recall both the puerperal cases that would escape notice and the causes other than puerperal infections that may be the starting point of an outbreak, to realize the limitations of notification.

Casting of the net widely will certainly make the life of the practitioner who does much midwifery so much more of a nightmare than it now is that only the most thick-skinned will survive, and many will be driven to give up a side of their work that is already a heavy burden. Patients and their households are not altruistic enough to take into account the lessening of the risk to others and the value of increased knowledge, if attained by worry and inconvenience to themselves, and sullen opposition and resentment at official inquisition may more than counterbalance any problematic gain. Resentment will be aggravated if such further administrative action is taken, as that suggested both in the Ministry of Health Report and in the Scottish Committee's Report, namely, that all deaths in childbed should be inquired into, or that post-mortem examinations should be made in all cases, as has been urged in some of the public discussions arising out of the publication of these reports. One form of notification that has also been put forward as much more reasonable as affording useful information in tracing the cause of infection, with no fresh formality, is that of the addition to the birth notification form of the name and qualification of the person actually present at, and responsible for, the delivery of the woman.

Before concluding, I would ask the views of our public health colleagues on the following suggestion. The immediate notification of suspect cases is obviously out of the question; it would probably involve some amendment of the Notification Acts, and certainly some years must elapse before the machinery for carrying out the measures that should follow notification can be set up throughout the country. I would ask them if it is not possible to organize, in some areas at any rate, obstetric consultations, bacteriological examinations, domiciliary nursing and, if possible, a certain amount of hospital accommodation, both for observation and for nursing and treatment. As soon as the most essential parts of the machinery can be put together, the practitioners in the area could be informed and asked to notify voluntarily cases of fever in childbed; the general adoption and success of this form of notification, especially for those patients in need of public assistance, would be proportionate to its efficiency and to the advantages it offered to patient and practitioner being readily recognized. In this way, less time would be lost and much valuable experi-

ence gained, both by the practising and administrative sides of the profession. The Report of the Scottish Departmental Committee makes this suggestion (par. 102) both in connexion with notification and the investigation of deaths within a month of the termination of pregnancy, and states that a form of voluntary co-operation has already been worked in Aberdeen and elsewhere.

The Reports recently published and Circular 517 of the Ministry of Health show the necessity for action—a need long recognized by this Section, as reference to its *Proceedings* will show—and both the Ministry and the local authorities are being urged to take action. Let us hope it will not be precipitate action, but after the possibilities have been explored by local effort in some such way as suggested.

I hope we shall hear from the administrative members who are present whether some way of starting co-operation with the men in practice cannot be evolved.

Definition and Notification of Puerperal Sepsis.

Dr. R. DUDFIELD (Section of Epidemiology).

That the mortality of lying-in women from "puerperal fever" has not decreased during the past seventy years to an extent commensurate with the decreases which have taken place in the majority of the other infectious diseases, is a fact which is beyond dispute. It would be waste of time to adduce statistical evidence in support of that statement on this occasion. The hope entertained that notification of the disease would furnish data for a study of the disease which should enable effective preventive measures to be put into operation has proved illusory. The reasons for that disappointing result were succinctly stated by Berry Hart at the annual meeting of the British Medical Association in 1900 [1]. They are:—

- (1) The difficulty of defining what one means by "puerperal fever."
- (2) The risk of statistics being misleading from errors, unavoidable or otherwise, in diagnosis.
- (3) The supposed unwillingness of practitioners to notify except in cases likely to end fatally.

With the second of the above it is unnecessary to deal here, and first and last will be the texts of these remarks.

The Infectious Diseases (Notification) Act, 1889, and the Public Health (London) Act, 1891, direct that every case of "puerperal fever" shall be notified by the medical practitioner in attendance "forthwith on becoming aware that the patient is suffering" from the disease. In neither Act is any guidance given as to the meaning to be attached to the term. If the evidence afforded by the Reports of the Registrar-General can be safely taken as a guide, the term "puerperal fever" came into general use between 1845 and 1850, it being first used in the annual report for the latter year, as an alternative to the term "metria." The latter designation disappeared from those reports in 1881. The pathological affections which the term was held to connote are indicated in the Note to the term *Puerperal Fever*, given on page 11 of the 1896 edition of the "Nomenclature of Diseases" of the Royal College of Physicians. The Note is in the following words:—

The term "puerperal fever" should no longer be used. Pyæmia, septicæmia or sapræmia, occurring in puerperal women, should be described "puerperal pyæmia," "puerperal septicæmia," "puerperal sapræmia" respectively. The other conditions included under the term "puerperal fever" should be returned under "Affections consequent on Parturition," the word "puerperal" being in all cases prefixed to the word denoting the local process.

On referring to the diseases entered under "Affections consequent on Parturition," the headings will be found to extend to over more than a page. (See p. 215 of the "Nomenclature.")

6 *Dudfield: Definition and Notification of Puerperal Sepsis*

In the Manual issued by the Registrar-General, which was based upon the International List (1911) of Causes of Death, "puerperal fever" is shown to include twenty affections having separate names (p. 45 of the Manual).

It is hardly a matter for surprise that there should be doubts as to what pathological manifestations should be notified as "puerperal fever," or that there should be some divergencies between the practice of the profession generally and the rules of classification adopted by the Registrar-General. The evidence on the incompleteness of notification will be found in an article "The Fatality of Puerperal Fever" [2], where the probability of divergency in describing cases and deaths is referred to. Dame Janet Campbell observes:—

The indefinite character of the term "puerperal fever" is doubtless one of the reasons why notification of puerperal infection has proved unsatisfactory and disappointing [3].

It is evident that to secure complete notification a definite meaning, for the purposes of such notification, must be assigned to the term. The Council of the Section of Obstetrics have suggested that the term "puerperal fever" should be replaced by "puerperal sepsis." On that suggestion the following quotation appears to be pertinent:—

We may in the first place remark that the official use of the term "puerperal fever" is probably advantageous, although the term is abandoned by the profession. The term "puerperal septicæmia" involves a theory, probably right, but yet one that may be superseded, and it is thus better to speak of puerperal fever, and to define it according to our knowledge at the time. Our present definition of puerperal fever is that it is some form of puerperal septicæmia [4].

It was in 1676 that Willis introduced the term "puerperal fever" to include the various pyrexial conditions observed in lying-in women, so that the term has a flavour of antiquity attaching to it. It is one, moreover, which is well known to the general public and for that reason, and because it is still in use in other countries, a suggestion to abandon it should receive anxious consideration. On the other hand great weight attaches to the view expressed by Lea.

The retention of the word (*sc.* puerperal fever), however, is undesirable for many reasons. In the first place it suggests the existence of a specific form of fever in lying-in women comparable to the exanthematous diseases. Fortunately, however, this is not the case, as infection only follows the direct introduction of organisms on the wound surfaces. Moreover, many observers have proposed to restrict the use of the term "puerperal fever" to the more severe types of infection, but any attempt to limit the use of the word is attended by considerable difficulties [5].

While there is little room for doubt as to the advisability of abandoning the use of the term "puerperal fever" in medical writings, for legal and administrative purposes the retention of the term appears to be desirable.

Whether the term "puerperal fever" be retained or either "puerperal sepsis" or "puerperal infection" be substituted therefor, one or other of the earliest symptoms of the "disease" requires to be selected as the indication for notification. For purposes of preventing the spread of infection and securing early hospital treatment (for women who cannot afford specialists' treatment), it is essential that the symptom selected should be that which most commonly is the first.

At the Rotunda Hospital in the early eighties Macan used to insist that there should be no rise in the woman's temperature after an uncomplicated labour. He held that any temperature above 98°8'—no matter of how brief a duration—was an indication of infection. That was undoubtedly a sound rule, but it is not of such universal application as Macan taught. A temporary rise in the woman's temperature may be caused by mental disturbance, constipation, &c., which will yield at once to simple treatment. On the other hand, a continuance of a raised temperature—no matter how slight—is almost certainly an indication of septic infection, and in most instances the earliest danger signal. Lea considered that:—

All cases of fever during the puerperium, unless clearly attributable to some extraneous cause, should be considered as forms of wound infection [6].

The Committee of the British Medical Association on "Puerperal Morbidity" put forward the following recommendations:—[7]

(1) That irregularities in temperature occurring during the twenty-four hours following labour should not be considered for statistical purposes.

(4) That records of the pulse and temperature should be taken twice a day. . . .

(5) That the table of puerperal morbidity should include all fatal cases and all cases in which the temperature reaches 100° F. on any two of the bi-daily readings from the end of the first to the end of the eighth day after delivery.

The Congress of Puerperal Fever held at Strasburg in 1923 adopted the following resolution:—

(4) Excluding the temperature within the twenty-four hours following delivery, every temperature exceeding 38° C. (100·4° F.) on one occasion and persisting more than twelve hours should be considered pathological during the puerperium [8].

It may be added that a midwife is obliged to call in a medical practitioner if a woman delivered under her care develops a temperature of 100·4°.

The Council of the Section of Obstetrics suggest a temperature "of 102° or higher during the first ten days after a confinement or abortion," as an indication of the necessity for notification, presuming the absence of a rigor. Any rise exceeding 100° persisting for twenty-four hours would appear to be more in accord with the practice of the larger maternity hospitals.

Two suggestions may be brought forward at this stage. The first is that the law should be altered to allow a practitioner to notify his suspicion that his patient has puerperal fever, thus obviating any delay while waiting for a certain diagnosis.

At least one distinguished obstetrician has forestalled that suggestion, as is shown by the following quotation:—

It is my practice and one which I teach and recommend, to notify any doubtful case as one of puerperal sepsis simply for the protection of others.

If it be not a case no harm is done by notification, whereas great risk of doing harm and spreading the disease to others is run by not notifying a doubtful case which proves subsequently to be septic [9].

The second suggestion is that the Ministry of Health, or the Royal Colleges, should set up a small committee of experts to collect and digest information relating to notified cases. A precedent for such suggestion is afforded by the inquiries made by the Board of Trade and the Ministry of Health with reference to deaths due to poisoning by coal gas. The committee suggested should draw up two questionnaires, one to be completed by the medical attendant (who should receive a fee for so doing) and the other by the medical officer of health of the district. It is highly probable that such committee would not require to function for many years, if the profession generally co-operate heartily.

The following estimate of the number of cases of puerperal pelvic infections during 1920 has been made by Fothergill [10]. From an examination of the reports published by maternity hospitals during a series of years, he concluded that "out of 1,000 confinements we may expect sixty or eighty infections and two or three deaths." During 1920, 957,782 births were registered in England and Wales.

Suppose only 7 per cent. of the mothers showed clinical signs of infection, that would mean over 67,000 cases. Again there were 2,087 deaths from puerperal sepsis registered in that year. There would be about 100 cases for every three of these 2,087 deaths, which would mean over 69,500 cases. In addition to the births there are abortions, about two to every seven confinements, and infection is said to be commoner after abortions than after

labour at term. Thus we should add at least another 10,000 cases of infection after abortion to get an idea of the total number of cases of puerperal pelvic infection. It would appear that there must have been 70,000 to 80,000 cases in England and Wales during the year 1920.¹

Whatever standard be adopted as the indication for notification and whatever changes be made in the Law governing notification, the results will remain as unsatisfactory and (practically) useless as they are at present, unless medical practitioners discharge the obligations imposed on them by the law promptly and regularly. On this point the words of Macan may be fitly quoted [11].

Public notification seems to me quite impossible until it has become the rule of the profession to tell the truth as to what is the matter with the patient to the patient's husband and relatives. That, as a rule, this is not done at present is well known both to general practitioners and consultants.

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Dr. ROBERT A. LYSTER (President of the Society of Medical Officers of Health, who attended as representative of that Society),

said that investigations into the possible causes of maternal mortality always led to one result, namely, that the chief and almost sole cause was bad midwifery practice. Experience in the supervision of midwives for the past twenty years had convinced him that the level of midwifery practice could be substantially raised by efficient supervision and inspection. The Midwives Act was obviously intended to create a body of midwives practising under close supervision and inspection, but that intention had never been carried out. Midwives exercised considerable influence through their societies, and they controlled to some extent the Central Midwives Board. By means of this influence they had successfully prevented the organization of schemes whereby their work could be properly checked and supervised. The unfortunate result was that twenty years' working of the Midwives Act had resulted in little or no improvement in mortality connected with midwifery, or mortality among newly-born infants.

One of the suggestions to deal with this important matter was that notification should be made. As an illustration of the manner in which notification could be made entirely ineffective, and also as an illustration of the extraordinary position with regard to the evasion of supervision and control by midwives at the present time, he (Dr. Lyster) referred to the working of the Notification of Births Act, 1907. The object of the Act was to procure notifications of every birth, to be sent within thirty-six hours of their occurrence to the medical officer of health, who, as a result of such notification, was supposed to organize schemes by which the progress of these infants could be kept under supervision. With the support of the Central Midwives Board, midwives had secured the position that, although they had to notify the birth within thirty-six hours in accordance with the Notification of Births Act, yet no official visits were paid by health visitors to that house during the first ten days. It was a striking fact that it was the mortality among mothers and infants during those ten days that had shown so small a diminution during the

¹ The cases notified during the year numbered 2,898, rather more than 4 per cent. of the lower estimate (70,000).

past twenty years. He had no doubt that merely by the introduction of regular visits by health visitors immediately after the notification of the birth had been received a very striking improvement would result. At the present time the obvious intentions of the Notification of Births Act were being defied and the Act was being set at naught by the organized opposition of the midwives, supported by the Central Midwives Board.

It might be useful to compare the unreduced mortality connected with midwifery with the enormous reduction in infant mortality which had taken place during the past twenty years. As soon as the care of the infant became more or less public and largely in the hands of the health departments of the country there was an immediate improvement in infant mortality.

In the document issued by the Ministry of Health early this year (1924) the statement was made that there should be "a sufficient service of competent midwives," and it went on to suggest that the adequacy of such a service would be secured by large public payments either direct to midwives or to nursing associations. His (Dr. Lyster's) experience was that a sufficient service of competent midwives could be secured at once in most districts by increasing the supervision of their work. In practice, the payment of subsidies to midwives in sparsely populated districts was found to be unreasonably costly when the total cost was compared with the number of confinements attended. It was far cheaper and more satisfactory to provide maternity homes. Maternal mortality would be substantially and immediately reduced by the provision of maternity beds for complicated cases, and for those patients whose home conditions were unsuitable. To these he would add the cases occurring in sparsely populated districts.

NOTIFICATION.

When diseases more or less infectious were being discussed, there was always a pathetic faith exhibited by some people in the result of notification. Professor Fothergill, in his excellent paper in the *British Medical Journal* in May last, said: "What a curious fascination 'notification' possesses for some minds." The country had had an exhibition of this kind of mistaken faith when measles was made notifiable a few years ago, only to be withdrawn from the notification list after some years' trial. Notification, unless it could be accompanied by substantial schemes of practical utility, was worthless from a preventive point of view.

Much of the present system of notification was completely unreliable. He (Dr. Lyster) said that in his area the notified cases of puerperal fever fell short of the deaths from that disease, the figures for the past few years giving a mortality of 106 per cent. In other words, only one case in thirty or forty was being notified.

Notification should be a serious piece of work and should be adequately paid for. If a suitable payment was made, then it would be reasonable to expect it to be done well. The practical way to bring the practitioner into prevention was to begin by paying him a fair fee for a full report on these cases.

The proposals that were now being made in the way of amendments of definition, and regulations for notification, were quite sound, and would be useful, but only if a definite scheme of dealing practically with these notifications could be arranged. The document issued by the Ministry of Health made the following statement: "There should be investigation by the medical officer of health of all maternal deaths due to childbirth, and to all cases of puerperal infection whether fatal or not." It was very significant that this statement was made without further comment, and it was apparently left to the medical officer of health to get into all kinds of trouble and difficulty with the medical profession in making inquiries concerning the work of private practitioners. It was to be hoped that no medical officer of health would undertake such investigations unless and until the medical profession assented to the proposal. There was much talk nowadays as to the place of the practitioner in

preventive medicine, and the way in which this proposition was received would serve as a useful test or indication as to the views of the general practitioner upon the subject of preventive measures. He (Dr. Lyster) therefore suggested that this proposal on the part of the Ministry of Health should at once be considered by the British Medical Association which, in its turn could advise its members as to the attitude that they should adopt towards it.

Notification was useless unless it was followed by a practical scheme, and the next point to consider was what practical scheme could be evolved to deal with the notification. The more obvious course was for the notification to be followed by an offer of assistance of various kinds. If the case could be nursed at home then an offer of skilled nursing assistance should be the immediate result of the notification. If operative treatment was necessary then an institution must be at once available for the case, or in cases that might be treated at home the services of a consultant should be forthcoming on the request of the practitioner concerned. Last, and not least, there was the question of the supply of nourishment to such patients, altogether apart from the general question of such provision to expectant and nursing mothers. It would be quite useless to send a nurse to many of these cases without powers of providing nourishment.

DR. T. WATTS EDEN

said that reference had been made by two or three speakers to the fact that the notification of puerperal fever was not at the present time as satisfactory as it should be. Dame Janet Campbell said, with characteristic official moderation, that it was unsatisfactory and incomplete. Dr. Fothergill had said that not 4 per cent. of the cases of puerperal fever were notified, and that in some districts the number of deaths exceeded the number of cases notified; and he was supported by Dr. Lyster. The salient fact in the whole situation was that the present system of notification had completely broken down. It was therefore worth asking themselves why it was so.

He believed there were two reasons for it. Those who were in the habit of seeing cases of puerperal fever in consultation were always asked, first, was it puerperal fever? and if the answer was in the affirmative, the next question was: "Must I notify it?" Those questions furnished the clue to the difficulties. There was much honest doubt, especially in the early stages, as to whether a case was or was not one of puerperal fever; and even if the case were of that nature, the practitioner would like it to be called by some other name, because the idea had become fixed in the public mind that if puerperal infection occurred, the doctor was to blame. In notifying it, he would, therefore, be notifying something which reflected upon himself adversely. The practitioner's attitude towards puerperal fever was different from that towards other notifiable diseases. He would notify tuberculosis and the eruptive fevers, but not, if he could help it, puerperal fever.

Without the willing co-operation of the doctors, puerperal fever would not be efficiently notified, no matter how it might be defined. The first difficulty which had to be faced was, that this support was not forthcoming now, and until it was obtained there would not be much progress made.

He therefore contended it was a mistake to approach the subject from the point of view of notification. All would agree with the objects stated by Dr. Fairbairn that evening, as to the importance of getting cases in the earliest possible stages, so that treatment might have a better chance; but as long as one was up against the hostility of the practitioner, this necessary step would not be taken. What was required was not so much to concentrate upon puerperal sepsis, as to review the whole field of puerperal morbidity. This would get round the difficulty of reluctant practitioners in regard to notification. How often did it happen that a case of mild febrile

disturbance in the puerperium was converted into a virulent sepsis by injudicious management, or by some operative interference? The cases should be got before that stage occurred, i.e., before they were notifiable as puerperal fever at all.

It would be possible to deal with the whole field of puerperal morbidity if there were proper provision for the reception of midwifery cases in hospitals. In the meantime, the hospital provision for cases of puerperal fever was scandalously inadequate. If it were possible to organize a system of puerperal observation wards in connexion with existing hospitals, to which practitioners could be encouraged to send all their cases which went wrong, such as those with lacerations which would not heal, as well as those which became febrile, he was sure the women would go gladly, and doctors would be only too glad to get such cases off their hands. Then all cases eventually diagnosable as septicæmia would be notified, not by the doctor, but by the institution, and in that way the doctor would escape all the odium of it.

The scheme would cost a good deal of money, and it could only be undertaken if liberal State assistance were forthcoming, and before that, inquiry was needed. Dr. Williamson, who was unable to be present on this occasion, said he thought the subject was worthy of a Royal Commission. That might be the expression of the enthusiasm of the obstetrician, but he (the speaker) was certain the matter was well worth inquiry by a strong committee, partly lay and partly medical, which could review the whole field of midwifery service, from the training of midwives to that of medical students. The time was overdue for the evolution of some form of national midwifery service for this country. The Ministry of Health had been moving here and there; it had done a number of things which, he thought, were open to criticism, but in any case it was difficult for the Ministry to keep itself in touch with the realities, in a clinical sense, and the needs of the situation, and he thought they would welcome the assistance which a committee of inquiry of this kind could well afford them in laying the foundations of an adequate midwifery service for the whole country, of which observation wards could form a part. Until that was done, he did not think much real progress would be made.

Dr. C. E. S. FLEMMING (Bradford-on-Avon)

said that this was not altogether a new experience to him. As a general practitioner, he had found that when several specialists had dealt with a case there was very little left for the poor practitioner. When he saw the notice, it occurred to him that notification was only a part, and by no means the most important part, of the process of prevention, and it was difficult to deal with one without discussing the other.

He thought that replacing the term "puerperal fever" by "puerperal sepsis" was a valuable substitution, though it provided another exception to Shakespeare's dictum about the uselessness of nomenclature. The old term forced the practitioner either to notify every case which had a temperature, or to look for a definite specific disease, and that without any spots or stigmata to guide him. There were other conditions, not due to sepsis, accompanied by a rise of temperature within ten days of the confinement, and lasting more than twenty-four hours, one of the most frequent being influenza. During an epidemic of influenza it was not uncommon to see such cases during the puerperium, and they caused great anxiety. The practitioner generally would be only too grateful if there were available a second opinion, if enough specialists could be found, to come and tell the practitioner whether a case in question was really one of puerperal sepsis in an early stage.

With regard to procedure, he thought notification at the present time was made to the wrong person, at least so far as county areas were concerned. It often

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happened that a practitioner had to notify a case to a district medical officer who was a rival in practice, and obviously this fact would not stimulate the desire of the former to notify, unless it proved absolutely necessary. He knew the case of a medical man who had a case of puerperal sepsis and could not find out how the sepsis arose, until he discovered that the district midwife, who was looking after the case, was at the same time nursing two other cases, and both the latter were in the same condition, were under the medical officer, and neither had been notified—at least the nurse had not been informed that they were septic.

There was another important reason why notification should be made to the county medical officer, namely, that those who had control of the midwifery service, to a certain extent, should have the earliest possible knowledge of the existence of notification. At present it came to him only in an indirect way. Might it not with advantage be made, not only the moral obligation, but the duty of the medical man to inform the midwife or the nurse in attendance, as soon as he was aware that a case was septic? If prevention was to be effective, there should be a notification not only of puerperal sepsis, but of all septic conditions, such as discharging wounds, burns, &c., as they were equally potent causes of infection and more serious than the cases of chicken-pox, &c., which scared the district nurse. It was agreed that sepsis would be controlled by attention to the patient's body, surroundings, and clothes, and the attendants' hands and instruments; the surroundings themselves might not convey infection, but they made the technique of asepsis very difficult to carry out. In some houses nothing was clean, everything was overcrowded, and sometimes it was difficult even to get sufficient hot water. Notification was necessary to find out the incidence of sepsis and the conditions which caused it, and he strongly supported the suggestion that the certificates of deaths occurring within four weeks of confinement or of miscarriage should in all cases record the fact. A better training of both medical men and midwives was admittedly required; not only would it bring about better midwifery, but, with it, an appreciation of the importance of notification. If notification led to successful treatment, it would help to remove the aversion to admitting that a case was septic, as the opprobrium attaching to a case which recovered was naturally much less than that associated with a fatal case. The doctor could fairly claim that the patient should have received proper ante-natal care and that labour should be conducted amidst proper surroundings. If a medical man, when giving ante-natal treatment, found that the surroundings were not suitable, it should be his duty to notify the local authority of the fact. What was the use of notifying an infectious or septic case unless it could be moved out of harm's way, and to some place where it could receive efficient treatment? It could not properly be transferred to a maternity home or a general hospital; it would not be fair to those institutions. At the present time, in most districts, the isolation hospital seemed to be the only suitable place for such cases, and it was essential there should be there someone able to undertake the necessary treatment; unless this was possible the general practitioner would not be likely to appreciate the value of notification.

Dr. W. BUTLER (Medical Officer, London County Council).

Maternity mortality is one of the gravest incidents with which the practitioner is confronted. At the outset of his career it looms as an impending contingency to which at any moment he may be brought into a relationship of peculiar responsibility, and the pervading sense of this initial experience will always command the sympathetic consideration of such an appeal as that made by Sir George Newman, which is the occasion of to-night's discussion.

The terminology and definition of puerperal fever have an important bearing on the administrative action which, appropriately, may be taken in respect of this

condition, but it will at once be recognized that the difficulties which in the past have arisen in this connexion have not been merely those of nosology. How comes it that a disease after so many years of statutory notification remains so elusive, notwithstanding that it is embraced under so great a variety of nomenclature? Puerperal fever, puerperal pyæmia, puerperal septicæmia, puerperal sapræmia, puerperal septic intoxication suggest at least a broad intent, so inclusive a comprehension, in fact, of a group of morbid incidents of the puerperium that no instance of these morbid processes should escape notification if only the will to disclose were there. Yet the fact remains that puerperal sepsis lurks in dark places and the death returns have disclosed to every experienced medical officer of health the first knowledge of cases which, despite their *camouflage*, he knows should have figured in his notification tables and have been the subject of administrative action. And it is not difficult to appreciate why this is so: on the one hand we have a series of pathological phenomena associated with the puerperal state of so varied a character both in origin and symptomatology as to defy obligatory inclusion under any term or group of terms used to denote such a specific entity as a disease may be conceived to be.

On the other hand we have the tragic history of puerperal sepsis, the sombre background of the past projecting itself and besetting what in the light of modern medicine seems an anachronism, a preventable catastrophe that will suggest a culpable responsibility. The practitioner has a not ill-founded sense that, however innocent of blame he may be, he will not escape censure, practical if not nominal, so soon as acknowledgment is made that his patient is suffering from puerperal sepsis. With the condition of his patient he can deal, her interests are as safe in his hands as in those of another, but let that condition be named and prejudice is introduced; he will not be saved, by any official action that may be taken, from the malicious tongue of the busybody or the ignorant criticism to which he will be subjected. He is not less solicitous than the medical officer that the dangers of childbirth shall be reduced, but he approaches the problem from a different angle and he has personal interests which he naturally thinks have a claim to consideration. Herein, there is good reason to believe, lies the explanation of the failure of notification to yield the results which preventive medicine seeks to achieve by its means. It is its proper function to investigate the causes of disease and to take all suitable steps for its prevention, and the causes of maternal mortality can be no exception to this rule.

Puerperal sepsis, its origin and prevention, its clinical and pathological identity fall within its sphere of action as surely as does small-pox or scarlet-fever. To attain this object, divergent interests must first, as far as possible, be reconciled. The difficulties of the practitioner are real and should be sympathetically apprehended. To disregard professional susceptibilities is to fail in securing that co-operation with the practitioner on which alone successful administration can be conducted. If it be possible to rob notification of any suggestion that it is a confession of constructive or rather misconstructive ineptitude or neglect with its consequential invocation of opprobrium, a step of great value will have been taken and if at the same time it is possible to clear of ambiguity the nature of the condition to be notified, loop-holes of escape will be reduced and the efficiency of notification proportionately increased. This will be attained by broadening and at the same time definitely specifying the clinical phenomena it is sought to embrace under the statutory obligation.

The suggestion of the Sub-committee of the Section of Obstetrics that "any case in which there is a rigor or a temperature of 102° or higher for twenty-four hours during the first ten days after a confinement or abortion, must be notified" is precise and meets these requirements. But, obviously and even fortunately, there is embraced in this definition a variety of conditions which it would be incorrect and misleading to describe as puerperal sepsis. A common cold not infrequently

contracted during labour—perhaps directly induced by it—an attack of influenza, also by no means uncommon during the puerperium, the kindling of a latent tuberculosis, a variety of widely different pathological conditions, would all fall within the terms of the definition. It would be incorrect to call this a definition of puerperal sepsis, nor is this suggested. Call it what it is, a sustained puerperal pyrexia, and you get rid once and for all of a bugbear which neither on practical grounds nor on grounds of clear thinking has anything to say for itself. Sustained puerperal pyrexia is surely a suitable condition for notification to the medical officer of health. It is an obsolete view and one which has never had the sanction of any official or professional conception of his duties that they are limited to investigating diseases of an infectious character only. The problem of disentangling the complexity of conditions brought to his notice under the definition suggested is one which can successfully be accomplished only by intimate and harmonious co-operation with the attending practitioner. Dame Janet Campbell has suggested¹ that there are:—

“substantial grounds . . . for consideration whether the obligation to notify a presumably definite disease might not be replaced by a requirement to report certain morbid conditions arising from childbirth.”

and no morbid condition so fruitful of results, if suitably investigated, suggests itself as that of a sustained rise of temperature during the puerperium. Dame Janet Campbell further suggests that—

“If the doctor were free to indicate on the form of notification that his patient did not require assistance from the Public Health Department his reluctance to report early cases, especially in better class practice, would probably be still further diminished, and the chances of securing reasonably complete information as to the morbidity of the lying-in period correspondingly increased.”

Provision on the notification form for such an intimation should be made, but beyond this the determination of the cause of the formally notified sustained puerperal pyrexia should be the subject of mutual confidential investigation and the information which alone the practitioner can impart, while not limiting the necessary action to be taken, should so far as relating to a particular person be scrupulously regarded as confidential. There is a disposition to regard such confidential information as confidential to a committee of a local authority. In the case of puerperal sepsis, at any rate, I think its confidential character should be regarded more strictly and, as in the case of venereal disease, should partake of the character of the *secret médical*. The claim, on public grounds, to invade the confidences of the sick room, calls for sharp delimitation if it is not to defeat the very end on which the claim is based. The knowledge of the medical administrator need not be shared except in impersonal details by the committee which authorizes his action, and, although it is something of a paradox, it is probably true that more will be learned of puerperal sepsis when the name disappears from the list of diseases officially known to public authorities.

How far it is compatible with the provisions of the Infectious Diseases Notification Act and other Acts dealing with notification of disease to make known only a clinical condition common to a number of diverse diseases, is another matter which presumably does not here concern us. It would certainly be an innovation to make a clinical condition, capable of a varied interpretation, notifiable, but this, and the abolition of specifically notifiable puerperal sepsis, under whatever name it was described, would bring it indirectly but more certainly to official knowledge, while the administrative action taken, involving as it would the closest co-operation of medical officer and practitioner, is the kind of action likely to be most fruitful of results. It is, in fact, the only kind of action appropriate to the circumstances, and if a departure from the stereotyped procedure has to be made and special measures devised accordingly, it will be admitted that the occasion justifies such an innovation.

¹ “Maternal Mortality,” Ministry of Health, 1924.

With the details of the administrative co-operative action to be taken upon receipt of notification others will doubtless deal. The resources of the local sanitary authority are greater than those of the practitioner. To place at his disposal such of these resources as are appropriate to the case indicates broadly the measures of relief which in an increasing degree would be provided.

The provision of consultative and institutional facilities is almost an inevitable outcome of such action. The disinfecting equipment of the local authority is not infrequently looked at askance by the practitioner, but tactfully made available, its usefulness becomes appreciated, and it can rarely be that an equally efficient substitute exists in private hands. It is not, however, so much in what exists, not so much in any developed organization of preventive measures already in being as in the potential mobilization of resources for maternal aid in the contingencies which call for it, and therein lies the best hope of dealing with the causes of maternal mortality. But it is a condition of such a development that notification of the morbidity calling for investigation and relief shall first become efficient. If the main cause of its previous inefficiency be removed there is no reason why it should not share in the success which has attended the notification of other diseases.

Dr. HAROLD SCURFIELD (Late Medical Officer of Health, City of Sheffield),

speaking from his eighteen years' experience as Medical Officer of Health, Sheffield, said that all the speakers, so far, appeared to be agreed that notification of puerperal fever had failed, and some stated that the deaths from the disease frequently outnumbered the notifications of it. There appeared to be an idea, in many districts, that sepsis occurring after miscarriage did not need to be notified. He agreed with those who considered it was better to start anew and think out a proper administrative scheme, before troubling about the exact condition to be notified. He did not think, with one speaker, that notification of tuberculosis had been properly carried out; there was a great failure in this regard, and this failure seemed to vary with the inefficiency of the measures which followed notification. The disease ophthalmia neonatorum was a parallel case to puerperal sepsis, in that its occurrence seemed to imply a slur on the practitioner, that he had not done everything he ought to have done for the newly-born baby's eyes. The notification of this ophthalmia was, he thought, well carried out, and was improving as hospital accommodation for both mother and child at a moment's notice was increasing. When, in Sheffield, a case of puerperal fever was notified, as a rule there was no hospital accommodation in the Jessop Hospital for Women, and the Corporation Hospitals Committee did not admit puerperal fever cases into the fever hospital, so that there was no accommodation there. If cases of the condition were admitted into the Corporation Fever Hospital, a special staff of nurses would be required, and, he took it, there would have to be gynæcological experts ready to deal with the cases. In Sheffield the only resort was the Poor-law hospital. Throughout the country there were large numbers of beds in efficiently equipped Poor-law hospitals, which were not thoroughly used, although the hospital accommodation outside the Poor-law was insufficient to meet the demands of the community. He suggested that the proposal for an alteration in the definition of the disease to be notified should be accompanied by a strong recommendation that a well-thought-out administrative scheme should be prepared to apply to all populous districts, and even for sparsely-populated areas, for the motor ambulance made it possible to deal with these latter cases far better than formerly.

Dr. MARY KIDD

said that she thought that a previous speaker was not altogether fair to the midwives. She considered that, as a whole, they were an efficient and hard-working body of women who were doing a trying but necessary work for very inadequate

remuneration. She thought from her knowledge of the working-class mother that she would find it disturbing, during the puerperium, to have a health visitor coming in to inspect the work of the midwife in attendance, especially if differences arose between the two as to the conduct of the puerperium. Some health visitors had had very little, if any, experience of midwifery, and it would be hard on the midwife who knew her job to have an official who might not be qualified to do so coming in to inspect her work. Surely a more practical way for a local authority to carry out preventive measures against puerperal sepsis would be to provide the district midwives with sterilized dressings and rubber gloves, free of charge. The risk of puerperal sepsis would be distinctly lessened by such measures, at a negligible cost to the local authority. She was glad that Dr. Scurfield had drawn attention to the large number of maternity beds which were lying empty in many of the Poor-law infirmaries, and which ought to be made available for respectable working-class mothers who were living in overcrowded homes in which it was almost impossible to conduct a confinement with ordinary cleanliness, let alone surgical cleanliness or decency. From her experience at one of the largest ante-natal clinics in London during the last five years she was convinced that the chief reason why such mothers hesitated to avail themselves of the excellent provision for confinements in the Poor-law infirmaries was, that they hated the idea of having to apply for admission to the relieving officers. Surely it should be possible to devise some means by which an expectant mother who could pay for her confinement in one of these institutions might be able to apply to the medical officer of health in her neighbourhood for admission to one of them through the agency of the health visitor or doctor at the ante-natal clinic, and have her payments assessed by the Maternity and Child Welfare Committee rather than by the relieving officer.

Sir EWEN MACLEAN (Cardiff)

said he had been very much impressed by the contributions to this discussion, particularly those of Dr. Eden and Dr. Lyster, who had stressed two points: (1) That notification, as such, had broken down; (2) that if a discussion, such as this, and any executive action which was to arise out of it, was to be of service, the matter must be approached on a broader basis. And such a discussion would be incomplete without a contribution from a representative of Wales, because those acquainted with the subject were aware that Wales presented an "awful example" in the matter of maternal mortality, and in particular the mortality from puerperal sepsis. The mortality from childbirth for the country generally was 4.11, while that for Wales was 5.41 per thousand births; and the single factor most prominent in this figure was puerperal sepsis. The Consultative Council of the Welsh Board of Health, of which he was Chairman, in the course of some of its work was so impressed with the high puerperal mortality in Wales that it had obtained permission for the Chairman and several of its members to investigate the conditions in various parts of Wales. The urgency of the problem was readily recognized by all the workers, and the facts which emerged would corroborate what had been said, and in due course would be embodied in a report by the Ministry. Wales presented, in common with other parts of the country, the problem of the large failure to notify this disease, and there were several disabilities affecting notification to which reference had already been made. But the cause to which he wished to draw attention was one which went deeper than the suggestions which had been mentioned in the discussion. Directly or by implication, blame had been laid on the practitioner, and upon the midwife as well; but the fact which emerged in the inquiry to which he had alluded was, that the real reason why notification was not more successful lay in the extraordinary complacency of the patients themselves; they did not mind whether the condition was notified or no, and in

some instances they did not want it notified. So far, the result of notification had done no good to the patient. The suggestion that notification should be followed by an improved nursing service for parturient women was a good one, and if in the future it should become known that notification resulted in benefiting the individual, it would become popular.

Lastly, he did not think that any real good would result even if helpful executive action in the matter were taken until it was brought home to the people themselves: (1) That puerperal sepsis need not occur, and (2) that if it need not, it must not occur, but (3) that if it did, prompt and helpful measures to combat it would be taken.

Professor A. LOUISE McILROY

said she thought time was being wasted in the discussion of notification because it would be always a failure under present conditions, for three reasons.

(1) It required a considerable amount of courage in some cases for a medical attendant to suggest to the patient and her relations that she was suffering from a notifiable disease, and there was therefore a great temptation to avoid making a definite diagnosis of puerperal sepsis. The conscientious practitioner was more liable to have black marks against him at the Ministry of Health than one who had comparatively few cases of sepsis.

(2) The general reason of failure was the great difficulty experienced by obstetricians in defining what was puerperal sepsis.

In the case of scarlet fever or diphtheria it was an easy matter. The rise of temperature alone was surely very little guide nowadays to the diagnosis of sepsis. Would anyone be prepared to say that a case with streptococci in the blood was the same as one in which there was slight infection from a laceration of the cervix, both running a temperature for two days? Should these cases be notified alike? It was necessary to clear up what was meant by the term puerperal sepsis.

(3) The third reason for the failure was that when a patient was notified there was not always expert treatment available for her disease. When she was sent to a fever hospital the services of a skilled obstetrician were not always available and treatment might be carried out by officials with no special knowledge of the disease.

She (the speaker) agreed with Dr. Williamson's suggestion that a Royal Commission should be appointed not only for sepsis but for the reorganization of the whole obstetric practice of the country.

One of the great reforms which the Ministry of Health might bring about was the establishment of a large hospital in the London area which would be devoted exclusively to the treatment of puerperal sepsis and its results—the staff to consist of skilled obstetricians. She fully agreed with those who regarded this question of notification as merely a small part of a large subject, and she said that one of the aims of those who moved in the matter should be to improve the education of the medical practitioner and give opportunities for special study. At present more training was devoted to midwives than to helping practitioners to acquire a knowledge of modern methods in obstetrics.

Dr. T. H. C. STEVENSON (General Register Office)

said that when listening to Dr. Lyster's remarks he was almost tempted to believe that 100 per cent. of the births in this country were attended by midwives, but the remarks of other speakers forced him to draw a different conclusion, for they referred to the difficulties of general practitioners in notifying puerperal sepsis, because of the possible implication regarding their professional skill. It would be a point of general utility to know in whose practice these deaths occurred, whether it was in the practice of doctors or of midwives; it could be easily determined by a tabulation

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of available records. About four years ago he noticed that at that time, when the proportion of cases attended by midwives could be held to have declined, owing to the return of practitioners from military service to their civilian duties, there was a sudden and very appreciable rise in the mortality from puerperal fever. He would be content with merely stating the fact, and suggesting that it constituted a further reason for making the tabulation he urged.

Dr. Dudfield had stated in this discussion that the Registrar-General included some twenty morbid states under the heading of puerperal sepsis. The latter was the term now in use, but he himself preferred the term puerperal fever, as it indicated nothing more than the occurrence of fever during the puerperium, and, as was suggested in the Sub-committee's report, that was the simplest and most straightforward test. The number of conditions tabulated under the heading "puerperal sepsis" merely represented the number of different forms of certification received and which were shown under that heading, and so varied with the degree of diversity of certification. In this case the importance of the subject had been held to justify minute detail in tabulation.

Dr. E. W. GOODALL (Section of Epidemiology)

said that he wished to correct a statement he understood one of the speakers to make, namely that the cases of puerperal fever which were sent to the fever hospitals in London were placed under the care of junior medical officers who were quite inexperienced in these diseases and that the advice of specialists was not available. These cases were not put under the sole charge of inexperienced medical officers. As puerperal fever cases had been admitted into the Metropolitan Asylums Board's hospitals for the last twelve years, the senior medical staff in the service had acquired a considerable experience of the disease, and the patients were under either their direct care or their supervision. Moreover, if additional advice or assistance was considered necessary, it was obtainable, and as a matter of fact was not infrequently procured. Unfortunately a large proportion of the cases were sent to hospital in a hopeless condition, too late for effective treatment.

Dr. JOHN ROBERTSON (Medical Officer of Health of Birmingham)

said that in that city a hospital ward at the Birmingham and Midland Hospital for Women had been set aside for the treatment of puerperal sepsis and that the local authority with the sanction of the Ministry of Health paid for the treatment of the cases. The fact that such accommodation was available meant that there was no difficulty in getting large numbers of cases notified. It had been stated by other speakers that the number of notifications was smaller than the number of deaths in many districts. Last year in the City of Birmingham there were 186 new cases of puerperal fever notified, with thirty-four deaths, i.e., about five and a half cases to one death. In the preceding year there were 137 cases notified and twenty-five deaths, i.e., about five and a half to one. The provision of the best possible facilities for the treatment of cases meant that doctors would notify their cases and send them into hospital.

The amount of accommodation provided in Birmingham had always been sufficient, and no case had ever been refused admission. The local authority had informed practitioners that at any hour of the day or night an ambulance would be available to convey any patient suffering from puerperal sepsis to the hospital for free treatment.

Unfortunately, the results obtained were not as good as might be anticipated. This might have been due to the fact that the cases were admitted too late, but the practical point was that the best facilities and the best specialist advice that could be obtained did not effect such an improvement as to reduce the number of deaths

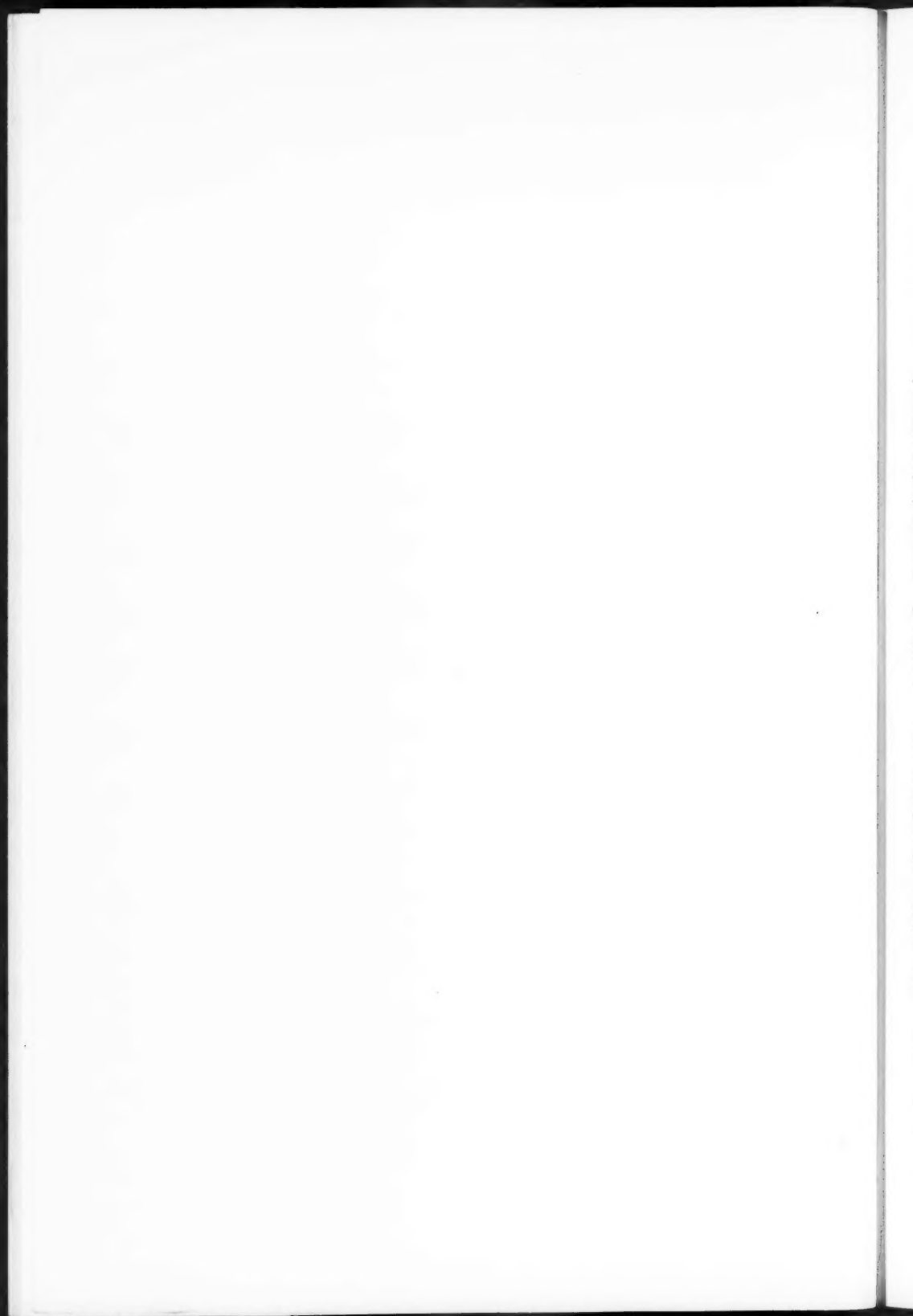
materially, and therefore he thought that something more was required than the provision of hospital treatment. Obviously, what was required was an improvement in the midwifery practice with a view to preventing the occurrence of these cases; but, unfortunately, there was no uniformly agreed method that would prevent the occurrence of puerperal sepsis. Until some scheme could be worked out to achieve this there could be no question that the best chance a woman had was to go into a properly equipped hospital where she could receive adequate medical attention and nursing. There was a great need for some scheme for the prevention of this obviously preventable disease.

Dr. REMINGTON HOBBS

stated that Sir George Newman asked for a report which might lead to the prompt notification of puerperal sepsis, as soon as symptoms of septic infection became manifest, so that adequate treatment would be ensured. He therefore suggested to the Sub-committee that a temperature of 102° F. was too high, and that more attention should be paid to the condition of the patient and also to physical signs.

Dr. H. RUSSELL ANDREWS (Chairman)

said it would be agreed that the discussion had been a very useful one, and what now remained was for the Sub-committee to meet and, with the help of the co-opted members of the other two societies, to consider what reply should be sent to the Ministry of Health. It would simplify the work of the Sub-committee if all speakers would send an abstract of their remarks that night to the senior Honorary Secretary. It could be relied upon that, after this full discussion, the reply to the Ministry would be on broad lines and not be on any small points such as many speakers had deprecated. It could not yet be said how far the statement would go; it might ask for an inquiry such as had been suggested.



Section of Obstetrics and Gynaecology.

President—Dr. H. RUSSELL ANDREWS.

Case of Fibro-cystic Tumour of the Perineum and Buttock.

By Professor BECKWITH WHITEHOUSE, M.S.

R. O., a 3-para, aged 51, was referred to me on September 18, 1924, by Dr. Stewart, of Rugby, with the following history. Until the age of 39 her health had been normal. About that time she fell downstairs and struck the right buttock severely. There was much contusion, and she remained in bed for a couple of weeks. A few months later a small swelling appeared in the right buttock and slowly increased in size. At the end of twelve months it was causing her some local inconvenience and the swelling was explored and removed by Dr. Clement Dukes, at Rugby Hospital. The patient was informed that the tumour was a "fibrous growth" and that it would not recur. It is interesting to note, however, that although the tumour was removed locally by an incision over the buttock, laparotomy was also performed on this occasion. The original notes are not obtainable, having been destroyed with other old hospital records.

About a year later, a swelling again appeared in the right buttock beneath the scar, and the tumour has slowly increased in size during the past twelve years. Although causing great inconvenience by reason of its position and large size, the patient did not complain of any pain. Also she noted that the tumour varied somewhat in size and consistence, sometimes being hard and sometimes soft. Any exertion in the erect posture, or coughing, caused the swelling to become hard.

On examination, a large pendulous bilobed tumour was found occupying the right labium majus and extending backwards to the perineum and outwards into the right buttock. In the recumbent position it was lax and soft, presenting a "knotty" consistence, somewhat resembling a large varicocele. In the erect posture the swelling became tense and firm especially after the patient had been on her feet for a little time. There was a small apparent impulse, on coughing, over the right sacro-sciatic notch, and there appeared to be a communication with the pelvic cavity through the latter. The anterior and smaller lobe of the tumour extended from the anterior part of the right labium majus to the posterior margin of the corresponding labium minus. It was separated from the posterior and larger lobe by a cicatricial band of skin resulting from the previous operation. The posterior lobe extended from the right labium minus to the anal orifice, involving the right ischio-rectal fossa and perineum and extending outward into the buttock. Examination per vaginam and per rectum did not reveal any definite extension of the tumour in the para-rectal or para-vaginal regions, and no swelling could be detected in the pelvic cavity. A radiograph of the pelvis showed a normal condition of the pelvic bones with no evidence of old fracture.

The patient is shown as a case for diagnosis, the condition being either one of labio-perineo-gluteal hernia, soft perineal fibroma or myxo-fibroma, cystic hygroma, sacral teratoma or chordoma.

Operation Note.—At operation it was found that the tumour consisted of a congeries of smooth thin-walled cysts containing clear straw-coloured fluid, in a matrix of fibrous tissue. It was removed with considerable difficulty for the growth had infiltrated the paravaginal and pararectal tissues to a very extensive degree,

22 Andrews: *Fibro-cystic Tumour; Sarcoma causing Hæmorrhage*

and appeared to have originated in the pre-sacral region. The epithelium lining the cysts was flattened and endothelial in type. The tumour was allied to a cystic hygroma.

DISCUSSION.

Dr. H. RUSSELL ANDREWS (President) said that the Section was grateful to Professor Beckwith Whitehouse for having pointed out that there was a room for the exhibition of patients adjacent to that in which the meetings were held. Most of the members had not known that there was such a room and had not realized that patients could be shown at meetings of the Section. He considered that the tumour was partly soft fibroma and partly fat coming down through the hole in the pelvic floor which resulted from the previous operation. He did not think that it was a sacro-coccygeal tumour, and advised its removal.

Mr. CLIFFORD WHITE said he thought it probable that the case shown by Mr. Whitehouse was similar to the two cases recorded before the Section in October, 1911, by Mr. Malcolm and Dr. McCann.¹ In each of these a soft fibroma, possibly originating from the pelvic fascia, was removed by an incision through the buttock combined with laparotomy. In Mr. Whitehouse's case, the deficiency palpable in the pelvic floor corresponded with the position of the neck of the tumour referred to in the above-mentioned cases.

Sarcoma Ulcerating through the Vagina and causing Severe Hæmorrhage in a Girl aged 19.

Specimen shown by HENRY RUSSELL ANDREWS, M.D., for
W. B. COSENS, L.R.C.P., M.R.C.S., (Dorchester).

A SINGLE girl of 19, who had had no previous trouble of any sort, was seized at 7 a.m. on May 1, 1924, with vaginal hæmorrhage of alarming severity. Dr. Broadway, of Dorchester, was sent for and, together with his partner Dr. Bond, examined the patient under an anæsthetic. They found a large tumour occupying the vagina. The bleeding was so free that they plugged the vagina with gauze and sent her into the Dorchester Hospital, where Mr. Cosens saw her later in the day. The pulse-rate was then 90, the patient was rather pallid and a fair amount of blood was trickling out of the vagina. On vaginal examination under an anæsthetic the uterus could not be felt. A large tumour was found, pushing down the anterior vaginal wall and extending up to the umbilicus. Mr. Cosens opened the abdomen and found that the body of the uterus was free from the tumour. He made a transverse incision through the peritoneum and fibrous covering of the tumour, which could be shelled out except over an area of about 2 sq. in., where it was adherent to the anterior vaginal wall. In the centre of this area was a perforation into the vagina and a freely bleeding artery. The disorganization of the vagina was so great—a mere channel about $\frac{1}{2}$ in. in diameter extending from the cervix to an abscess cavity below the adherent portion of the tumour—that Mr. Cosens deemed it expedient to remove the uterus, leaving the ovaries intact. The tumour was said, by the first pathologist who examined it, to be a lipoma weighing 1 lb. 1 oz. Two holes can be seen in its fibrous capsule. The larger one opened into an abscess cavity; the smaller one was at the site from which the bleeding came.

I thought it almost incredible that a lipoma could have behaved as this tumour did, so I had further investigation made. One pathologist called it an endothelioma.

Professor Turnbull has kindly supplied me with the following report:—

REPORT TO DR. ANDREWS ON MR. COSENS' VAGINAL TUMOUR, S.D. 987, 1924.

Macroscopic Examination.

There are two specimens, both hardened in a fixative: (1) a solid tumour, and (2) an opened cyst labelled "capsule of tumour."

¹ See *Proceedings*, 1911-12, v (Obst. and Gyn Sect.) pp. 38, 42.

(1) The solid tumour is a slightly flattened mass measuring 12 by 12 by 7 cm. It is of firm rubbery consistence. Its outer surface is covered by a transparent capsule, and is smooth, except for very delicate fibrous tags. The cut surface has a lobular pattern, the lobules averaging 1 cm. in diameter and being separated by fine grey or opaque yellow anastomosing strands. The tissue composing the lobules has a moderately uniform appearance throughout; it is for the most part smooth, without pattern, shining and greyish white.

(2) The other specimen, the capsule of the tumour, is a cyst with a large ragged opening in its wall, and in its present shrunken fixed state it measures 7.5 cm. in diameter. Its inner surface is covered, over an area of a diameter of 6.5 cm., by a wrinkled white membrane resembling the lining of the vagina; in the centre of this is a black area evidently due to drying. The rest of the outer surface shows tags of torn tissue. The wall is from 0.2 to 1 cm. thick.

Microscopic Examination.

Two portions of the solid tumour were embedded in paraffin; one was cut upon the freezing microtome, and sections were stained with Scharlach R. A thick and a thin portion of the capsule were embedded in paraffin.

(1) (2) (3) The tumour consists for the most part of short spindle cells with oval nuclei. These usually lie parallel to form bundles, which interlace. They are also grouped as rings round vessels. In places the cells occupy a lightly stained homogeneous matrix, are scanty and lie chiefly round vessels. In this matrix there are a few swollen, ill-defined, stout collagenous fibres. The matrix is evidently œdematous. In the frozen section this matrix is present, and in it and in the cells there is a considerable quantity of droplets of fat. Other bundles are formed by longer spindle cells with elongated nuclei, and these merge with cells which in the staining of their cytoplasm and in their rod-shaped nuclei are characteristic, smooth muscle fibres. The tumour is, therefore, a leiomyoma. The preponderance of small, undifferentiated cells implies rapid growth, a conclusion which is supported by the presence of a considerable number of karyokineses. But the small cells are very uniform in appearance, so that malignancy is not indicated histologically. The tumour appears to be a cellular, rapidly growing, leiomyoma.

(4) and (5) The capsule consists of vaginal mucosa and muscle. The epidermis is absent from the greater part of the surface. Beneath this ulceration the mucosa is infiltrated with neutrophil leucocytes and the musculature is fibrosed. There are hæmorrhages in the mucosa and the muscle.

I thought that the tumour was of sufficient interest to be exhibited to the Section. Possibly the Pathology Committee would like to investigate it further.

The specimen was discussed by Dr. EDEN and Dr. FAIRBAIRN. It was referred to the Pathology Committee.

REPORT FROM PATHOLOGY COMMITTEE.

"We have examined this specimen and the microscopic sections thereof, and although large areas of it are degenerate we consider that in the main it has the characters of a spindle-celled sarcoma."

Simple Ovarian Embryoma with Spread into Contiguous Structures.

By JOHN S. FAIRBAIRN, M.B., and J. F. TAYLOR, M.D.

THIS specimen is brought before the Section because it appears to show the invasion of extracystic structures by the tissues of what seems to be a simple and unruptured ovarian dermoid.

It was removed (March 8, 1923) from a nulliparous married woman of 49, who was seen by one of us (J. S. F.) because her doctor had discovered the presence of an abdomino-pelvic growth. Her complaint was of pain, low in the abdomen, with flatulence and epigastric discomfort. The pain was occasional and not incapacitating; it occurred usually in attacks, the most recent one having lasted about fourteen days; the preceding pain had occurred some eighteen months before. Menstruation was unaffected; there had been no disturbance of micturition and the general health was good.

On abdominal examination a swelling was found arising from the pelvis in the mid-line and extending some two fingers' breadth above the pubes. Bimanual examination showed the uterus in front, slightly enlarged and freely movable, pushed forward by a large swelling filling up the pelvic cavity and extending into the abdomen. The swelling had an inelastic, somewhat putty-like consistence and was thought to be an ovarian dermoid or possibly a subperitoneal myoma undergoing degenerative change. Its removal was advised.

At the operation the tumour was found to be a dermoid cyst of the right ovary, occupying the sacral hollow. The round and ovario-pelvic ligaments and mesosalpinx were stiff from infiltration, so that there was unusual difficulty in freeing the cyst so as to secure the pedicle. The infiltration of its attachments suggested the possibility of malignancy, and therefore it was decided to remove the uterus and appendages of both sides.

The patient had a normal convalescence, and Dr. Edwards, of Forest Hill, her regular medical attendant, reports that she remains perfectly well up to the present.

The chief interest in the case lies in the study of the specimen after removal (report by J. F. T.).

DESCRIPTION OF THE SPECIMEN.

The specimen consists of a uterus with its appendages. In the position of the right ovary is a flattened ovoid tumour measuring about 5 in. by 3 in. by 1½ in. in thickness.

The tumour, which shows a few loculi in its walls, was found, upon opening, to be cystic, and to contain a mass of sebaceous material with many fine, light-coloured hairs. There was no sign of rupture or leakage from its cavity.

An X-ray examination, kindly made by Dr. G. Fildes, shows a dense shadow, irregularly triangular in shape, situated in one corner of the sebaceous mass. Dr. Fildes considers the shadow too dense for bone and suggests the presence of dentine.

The Fallopian tube, round ligament and mesosalpinx on the same side, showing infiltration by material having the appearance of fat. This infiltration also extends into the lateral wall of the uterus. (There is also a fibro-myoma in the wall in its lower part.) The left appendages appear normal.

Microscopical sections were made of the tumour, the Fallopian tube and the infiltrated wall of the uterus.

(1) The section of the tumour shows a structure of considerable complexity, and detailed description is not possible within reasonable limits. The bulk of the tumour shows an irregular areolar arrangement with varying amounts of fibrous stroma. The areolæ contain a material which stains well with Scharlach R, but is unstained by ordinary methods, and therefore consists of a fatty substance. The stroma shows different degrees of inflammation, with areas of round-celled infiltration, endothelial proliferation and numbers of giant cells. These giant cells are of the "foreign body" or "irritation" type, as are seen round old ligatures and in inflamed fatty tissue, as in the omentum. There are a few small spaces containing blood-cells and a few ill-formed vessels. Small masses of unstriped muscle are scattered throughout, and this muscle shows fine vacuolation as if by fatty or lipid degeneration. No skin, glands or other definitely formed structures were seen.

(2) The section through the Fallopian tube shows infiltration by a similar type of tissue. There are also a few small areas of densely staining amorphous material, apparently calcareous. In places, the canal is practically obliterated.

(3) The section into the wall of the uterus shows infiltration by fatty tissue with numerous giant-cells at the growing edge. Vacuolation is present in the muscle fibres.

The condition appears to be one of a dermoid cyst with infiltration of the surrounding structures by the cells of the tumour. The point for discussion is whether this infiltration should be regarded as malignant or inflammatory.

In the first place, there appears to be no substantial evidence that a simple dermoid cyst ever produces malignant metastases. In the words of Bland-Sutton, "those cases which from time to time have crept into the literature have never been confirmed by subsequent exploration or post-mortem examination." In the present case, although local infiltration was well advanced, no secondary deposits were seen on the peritoneum or in organs other than those immediately contiguous to the tumour. On the other hand, Aschoff states that rupture of the wall of an ovarian dermoid may lead to escape of the sebaceous material into surrounding tissues with formation of "xanthoma-like tissue with foreign-body giant-cells." He also suggests the possibility of diffusion from the interior of the cyst by perforation of its walls by hairs. It seems that this description might well be applied to the present specimen and that the condition may be diagnosed as inflammatory infiltration by fatty granulation tissue.

DISCUSSION.

Dr. H. RUSSELL ANDREWS (President) said that the case of Dr. Fairbairn and Dr. Taylor and the specimen shown seemed to be most unusual. Malignant change in ovarian dermoids causing infiltration of surrounding tissues was not uncommon. Microscopical examination would be carried out in most of these cases, and, if a fatty infiltration and not a malignant infiltration was found, it was probable that the case would be recorded.

The specimen was also discussed by Mr. T. G. STEVENS.

A Short Communication on a Retroperitoneal Lipo-fibro-sarcoma.

By HENRY RUSSELL ANDREWS, M.D.

A. W. was put on my operating list at the London Hospital on February 5, 1924, soon after admission to the hospital, as she was in a great deal of pain. She was aged 42, married nineteen years, had had eight children, the last in December, 1920, and one miscarriage in 1914. She had had pain in the right side of the abdomen for three years. For the last six weeks the pain had been continuous, sharp, and of a shooting character, going down to the lower part of the back. She had not found that anything made it either better or worse. She had noticed abdominal enlargement for three years and a definite swelling for a month or more. She said that she felt ill in herself. The monthly periods were fairly regular; the loss was small. On abdominal examination an irregular, mobile mass, more or less cystic, but with some harder lumps in it, filled the abdomen, reaching up half-way between the umbilicus and the ensiform cartilage. On vaginal examination the uterus was felt in the right side of the pelvis and free from the tumour, the lower pole of which dipped into the pelvis. The diagnosis which had been made was left-sided ovarian tumour.

As she evidently had a large abdominal tumour I did not examine her myself, as I had prepared for operation before I realized that I had not seen her before. If I had examined her I should very likely have thought that the tumour was ovarian.

If I had diagnosed it as a retroperitoneal tumour I should have lost an interesting operation and specimen, as I should have felt it my duty to transfer the patient to one of my general surgical colleagues.

As soon as the abdomen was opened it was obvious that the tumour was retroperitoneal, with the ascending colon running over it. The tumour filled the whole of the right side of the abdomen, reached across the middle line for some inches, and extended down into the pelvic cavity. It was of a dull yellow colour, which showed through the peritoneum and seemed to be composed, at least superficially, of fat. My practice is to close the abdomen without an attempt at removal of a retroperitoneal tumour if it appears to be a very vascular sarcoma, but as this tumour seemed to be composed largely of fat I proceeded to remove it. Its removal was on the whole easy. At one time I intended if possible to enucleate the right kidney from the tumour and to leave it if it looked healthy, but while I was separating the upper part of the tumour from close proximity to the liver a necrotic-looking patch gave way and about two pints of dark, greenish-brown, odourless fluid escaped. This was so suggestive of sarcoma that I thought it best to remove the tumour whole, tying the renal vessels, a few other smaller vessels, and the ureter. When I sent the tumour down to the pathologists I asked that when it was investigated they would try to enucleate the kidney. They reported that although the kidney was not diseased it would have been impossible for me to have removed the tumour and left the kidney behind. The cavity left, after removal of the tumour, was large and rather moist, so I inserted a drainage tube, which was left in for forty-eight hours. During the first few hours a good deal of serum escaped, but after this the dressing remained dry. The left kidney, healthy-looking and rather larger than the normal, lay at the pelvic brim, rather more of it above than below the brim. It is interesting to note that the low position of the kidney had caused no trouble during any of her eight labours. The uterus was situated in the right side of the pelvis. It was unicorn, its left side being smooth and free except that a fold of peritoneum, about four inches long, ran from its lower part, about the level of the internal os, across the pelvis and a little forward to meet the left ovary, which was of normal size, tucked partly into the internal abdominal ring. The inguinal canal was dilated and admitted the tip of the forefinger easily. No trace of the left Fallopian tube or left horn of the uterus could be seen or felt. There seemed to be a structure representing the left round ligament holding the ovary up against the internal abdominal ring.

The patient made an uninterrupted recovery and was sent to a convalescent home on the fifteenth day after the operation.

Three months after the operation I examined her. Her general health was much better, she had put on weight and had no discomfort or pain, but in the right loin could be felt a firm swelling about 3 in. long by 1 in., which I was afraid was a recurrence. At the present time she has a large mass in the right side of the abdomen.

I am indebted to Professor Hubert M. Turnbull for the following

REPORT ON THE SPECIMEN.

"The specimen measured $29 \times 21 \times 12$ cm., and weighed 7 lb. 13 oz. The outer surface was raised slightly into lobules and was covered by a smooth glistening membrane. This membrane was in a few places opaque and silvery; elsewhere it was very thin and transparent, allowing the pale yellow subjacent tissue to be seen. It was traversed by conspicuous vessels. The lower pole of the left kidney and its ligatured ureter projected from the lower and inner extremity of the specimen. On section the kidney was found to be embedded in a mass of tissue, which was adherent to its capsule but did not infiltrate its substance. In the lower and outer part of the mass, external to the kidney, was an oval area measuring 18 by 12 cm. The centre of this area was occupied by a cystic cavity with ragged, irregular boundary. Round the cavity was a zone of tissue, from 0.5 to 5 cm. in width. Towards the cyst this tissue was nodular, ragged, shredded, friable or cheesy, opaque and creamy yellow; outside

this it became at first gelatinous and dark grey, flecked with red, and finally tough, elastic, homogeneous and light grey or yellowish. The remainder of the cut surface of the mass was occupied by rounded or oval, lobular areas, which measured from 1 to 7 cm. in diameter, and were separated by thin silvery membranes. One lobule, measuring 7 by 6 cm., showed a firm, elastic, homogeneous, opaque buff surface. The cut surfaces of the remaining lobules were softer. They were moist, glistening and very finely granular, and abundant oil could be scraped from them with the knife. They resembled in appearance and colour the cut surface of ordinary adipose tissue, but were, in general, slightly firmer and more homogeneous."

Microscopic Examination.

(1) *A Portion of the Tough Tissue surrounding the Ragged Cavity.*—The tissue is composed of large, interlacing bundles of closely packed, spindle cells with elongated, oval nuclei. Between the cells are delicate, collagenous fibrils. There is very little variation in the size and shape of the cells, but there are a few exceptionally large, hyperchromatic nuclei. Karyokinetic figures are numerous. The tissue is traversed by vessels which are bounded by endothelial cells lying upon a narrow collagenous membrane. Externally the tissue is covered with a capsule of stout collagenous fibres; internally it becomes cedematous, hæmorrhagic and necrosed.

(2) *A Portion of the Gelatinous Tissue surrounding the Ragged Cavity.*—In this tissue short spindle and rounded cells, with short oval and round nuclei, lie in a matrix of delicate, wavy, collagenous fibrils. These fibrils are very abundant, but are separated by a variable distance from one another. The matrix is everywhere in excess of the cells. In many places the cells are very scanty. In some places not only are the cells very scanty, but many are necrosed.

This tissue appears to be a tissue very similar to that in the first section but rarefied by great cedema and undergoing necrosis.

(3) *A Portion of one of the Pale Yellow Lobules.*—The tissue consists of stout trabeculae of fibrous tissue, within which lie arterioles and venules, many irregularly shaped and unevenly distributed strands of a slightly less dense fibrous tissue, and between these a widely meshed net of collagenous membranes enclosing large, round, clear spaces. A thin layer of cytoplasm can occasionally be seen upon the inner surface of the membranes which confine the spaces.

The appearances are those of an adipose tissue in which there is more fibrous tissue than usually.

(4) *A Portion of the Firm, Buff Lobule.*—The lobule is bounded by a capsule of fibrous tissue which is composed of stout, collagenous fibres and contains arterioles and venules. Within the lobule are irregularly distributed, stout, collagenous fibres, and between these is a net-work of delicate fibrils. The meshes of the net are occupied by large, round cells with abundant, very pale, foam-like cytoplasm and small nuclei. These "foam-cells" vary considerably in size; some of the largest examples are multinuclear. In a few places they are round and spindle cells with homogeneous cytoplasm. There are a few large, sharply defined, clear spaces, such as are seen in ordinary adipose tissue. The lobule is traversed by capillaries and by a few arterioles and venules. Round the larger vessels the stout collagenous fibres are relatively abundant. There is a considerable infiltration with lymphocytes and plasma cells. The infiltration is chiefly perivascular.

The lobule is occupied by an areolar tissue which contains numerous "foam" or "fat-granule cells," and is the seat of a chronic inflammation. Similar "foam-cells" replace fat-cells in inflamed adipose tissue. This lobule is probably, therefore, a lobule of adipose tissue similar to that in the previous section, but altered by chronic inflammation.

Summary of Pathological Report.—The tumour was encapsulated and lay behind the peritoneum. It enveloped, but did not invade, the right kidney. It consists for the most part of lobules of an adipose tissue, which is somewhat more fibrous than ordinary adipose tissue, but is fully differentiated and "typical." One such

lobule is altered by chronic inflammation. The tumour contains, however, a large lobule of spindle-celled, fibrous tissue, which towards the centre of the lobule becomes very œdematous, hæmorrhagic, necrotic, and finally cystic. This tissue is much more cellular than any fully differentiated tissue in the body, and karyokinesis are very abundant. In these respects it has the characters of a fibro-sarcoma. But the cells are arranged in well-defined bundles, and they are, with few exceptions, remarkably uniform in size and shape. Further, the lobule is limited by a fibrous capsule. The fibro-sarcoma appears histologically, therefore, to be relatively benign. The tumour is a retroperitoneal fibro-lipoma with a focal area of fibro-sarcoma of histologically low malignancy.

Tumours of this description are rare, though not quite so rare as I thought before I looked up the subject. There is no reference to such tumours in the *Transactions of the Obstetrical Society of London*, and reference to only one paper on the subject in the "Review of Current Literature" in the *Journal of Obstetrics and Gynæcology of the British Empire* in its first thirteen years. Göbell in the *Deutsche Zeitschrift für Chirurgie*, 1901, lxi, p. 1, tabulated 101 cases of lateral retroperitoneal tumours growing from tissues above and below the kidney and extending into the pelvis. Of these, nine were lipomatous, four fibro-lipomatous and ten myxo-lipomatous. Since Göbell's paper was published a few such tumours as mine have been described, chiefly in America, as "lipo-sarcomata" or "lipo-fibromata with sarcomatous degeneration."

DISCUSSION.

Dr. FAIRBAIRN reminded the President that Mr. Alban Doran read a paper on retroperitoneal lipoma at the June meeting of the Obstetrical Society in 1902.

Dr. LOUISA MARTINDALE referred to a case of retroperitoneal sarcoma in a married woman, aged 52, who had been under her care for the last two years. Abdominal section had shown an extensive inoperable retroperitoneal growth as long ago as September, 1922. An intensive X-ray dose (92 per cent. unit skin dose) was given in November and repeated in January, with the result that the patient improved enormously and was able to get about, do all her own work and gained in weight. Three months ago, however, the patient began to go downhill, and she was now in the same condition she was before the treatment after a year's apparent freedom from symptoms.

The Plasma Proteins in Normal and Abnormal Pregnancy.

By L. J. COETZEE, M.D.

(Communicated by J. MARRACK, D.S.O., M.C., M.D.)

(From the Hale Clinical Laboratory, London Hospital.)

ONE of the most striking changes occurring in pregnancy is the alteration of the plasma proteins, especially fibrinogen. With the hope of throwing some light on the disturbances occurring in pregnancy I have compared the alterations of plasma proteins in normal and abnormal pregnancy with the alterations occurring in other pathological conditions.

I have estimated plasma proteins throughout by the method of Wu (1922); the amount of potassium oxalate which was added to the blood to prevent clotting was the same in all cases.¹ The figures found for normal non-pregnant women were very uniform, varying by less than 10 per cent. from the average: fibrinogen 0.32, albumin 5.27, globulin 2.46, ratio of globulin to albumin 0.467. These figures differ slightly from those found for normal men; the fibrinogen is slightly higher (0.29 in men), a difference similar to that found by Gram (1921) in fifty subjects; the albumin is

¹ The "Comparative Tables of Plasma Proteins" communicated with this paper are not printed in the *Proceedings*, but are kept for reference in the Library of the Society.

slightly lower and the globulin higher than in men, so that the globulin : albumin ratio is definitely increased (0.467 instead of 0.340). The figures for women are more uniform than those for men, a fact which is useful when comparisons are made between various conditions occurring in women.

A possible cause of changes of plasma proteins and of pathological conditions in pregnancy is absorption from the foetus of proteins foreign to the mother. I have studied the changes produced by injection of foreign proteins in three cases. Following the first injection and intensified by repeated injections there is a rise of fibrinogen (up to 0.73) and of globulin (up to 4.08), accompanied by a slight fall of albumin, so that the globulin : albumin ratio rises greatly (up to 1.11).

Similar changes were found by Lohr (1922), and for fibrinogen by Gram (1922).

In men with enlarged prostates there occurs a condition of back pressure on the kidneys with more or less renal damage such as may be found in some cases of pregnancy. In seven cases we find moderate increase of fibrinogen and globulin, with slight decrease of albumin. These changes are similar to those found in frank cases of nephritis. There does not appear to be any change peculiar to back pressure.

In nephritis of non-pregnant women we find increase of fibrinogen, diminution of albumin, globulin unchanged in the average, although in two cases much increased. In most of the cases the ratio globulin : albumin is increased, but only in exceptional cases is the globulin so increased that it compensates for the reduction of the albumin; this forms a distinction between nephritis and the effects of foreign protein injections.

In Case XII, as the patient got worse the amount of albumin and total protein became even more reduced; while in Case XIII, as the patient got better, fibrinogen fell, albumin and globulin rose, while the ratio globulin : albumin fell.

In thirty-five cases of normal pregnancy from the sixth month onwards the fibrinogen increased steadily from 0.37 to 0.52. This is not in agreement with Gram, who found a maximum in the eighth month (0.58 per cent.) with a subsequent drop to 0.53 per cent. This increase of fibrinogen is the most striking change; at the same time we find a slight reduction of albumin and slightly greater reduction of globulin, so that the ratio globulin : albumin is below that in normal women (0.37 in the last month instead of 0.467).

In one case studied after delivery the reversal of these changes occurs. These changes differ strikingly from those caused by injection of foreign protein in that the globulin is reduced instead of being increased.

Comparing the proteins of the cases of *albuminuria in pregnancy* with those of normal pregnancy, we find a difference similar to that between the less extreme cases of nephritis and normal cases. That is to say, slight increase of fibrin, moderate decrease of albumin and increase of globulin. In the two cases of *pregnancy in patients with chronic nephritis*, these changes occur to a much greater degree.

Three cases are given of albuminuria first discovered at term; of these No. 3 appears to be a frankly infective condition with high fibrin and globulin : albumin ratio. In No. 2 the more extreme changes characteristic of nephritis are seen. The changes in No. 1 resemble more closely those found in eclampsia.

It is impossible to differentiate sharply the changes in *toxæmia of pregnancy* from those that occur in nephritis. Out of seven cases the fibrinogen was moderately raised in three, and in one it was low; the globulin was rather low (under 1.73) in four and not raised in any; the albumin was low in all, but not much more so than the globulin, so that in only two was the globulin : albumin ratio over 0.50. After delivery the globulin always increased more rapidly than the albumin.

In seven cases of eclampsia, however, the difference was more striking; in only one was the globulin normal, in the other six it was reduced absolutely (in one as

low as 1'27) and relatively to the albumin, so that the globulin : albumin ratio was low. The fibrinogen was low or normal in five out of seven cases.

In four cases examined about a week after delivery, the globulin had risen to normal or above (2'40 to 3'19), and in two the fibrinogen had risen to high figures (0'82); the albumin also increased, but not so rapidly as globulin, as shown by the rise in the globulin : albumin ratio. Of two cases two months after delivery, one showed normal figures, the other slightly raised fibrinogen and globulin.

The changes in protein appeared to bear no relation to the severity of the condition.

Reviewing the whole of this series we find that in the pathological conditions of pregnancy changes corresponding to the degree of nephritis present are found, except that the increase of the globulin : albumin ratio is less than might be expected—this is especially noticeable in eclampsia; the increase of fibrinogen, diminution of albumin and total protein, that we find in nephritis, occur in these cases also. If, however, we compare the changes in plasma protein in pregnant women, both normal and abnormal, with those produced by injections of foreign proteins, we find a resemblance in the increase of fibrinogen, but a striking difference in the absence of any great increase of globulin. In the plasmata of normal pregnant women, in the majority of those with pathological conditions, and especially in those with eclampsia, the globulin is lower than in non-pregnant women. This cannot be taken as showing that absorption of foreign protein does not occur in pregnancy, nor that it is not the cause of these pathological conditions, but it does show that if such absorption does occur, it is not accompanied by the normal response—increased globulin formation. As antibodies to injected foreign substances are found in the globulin fraction, one has to consider the possibility that absorption of foreign protein occurs in pregnancy, but that the formation of antibodies fails, more especially in eclampsia, and that eclampsia is caused not by an abnormal absorption of foreign protein, but by a failure of the normal protective mechanism.

On the other hand, these low figures for fibrinogen and globulin may be the result of liver damage. Gram found plasma fibrinogen low in patients with liver damage, and it is possible that globulin is similarly affected. With slighter degrees of liver injury, such as may occur in less severe toxæmia, this reduction does not necessarily take place—for example in three cases of cirrhosis of the liver I found the average fibrinogen to be 0'50 per cent.

SUMMARY AND CONCLUSION.

(1) The plasma proteins of women, normal and pathological, have been compared with those of normal men, non-pregnant women and those of patients with conditions which affect the plasma proteins.

(2) The changes in plasma proteins in normal and abnormal pregnancy differ from those produced by the injection of foreign proteins.

(3) It is suggested that absorption of foreign proteins may occur in pregnancy, but that the normal protective response fails, especially in eclampsia.

(4) Changes similar to those found in nephritis occur in cases of pregnancy in which there is evidence of kidney damage alone, but in toxæmia of pregnancy and eclampsia the changes characteristic of renal damage are much modified.

My thanks are due to the physicians of the antenatal clinic and the lying-in wards of the London Hospital, for the assistance they have afforded for this investigation; and to the director of the Hale Clinical Laboratory for the facilities for laboratory work.

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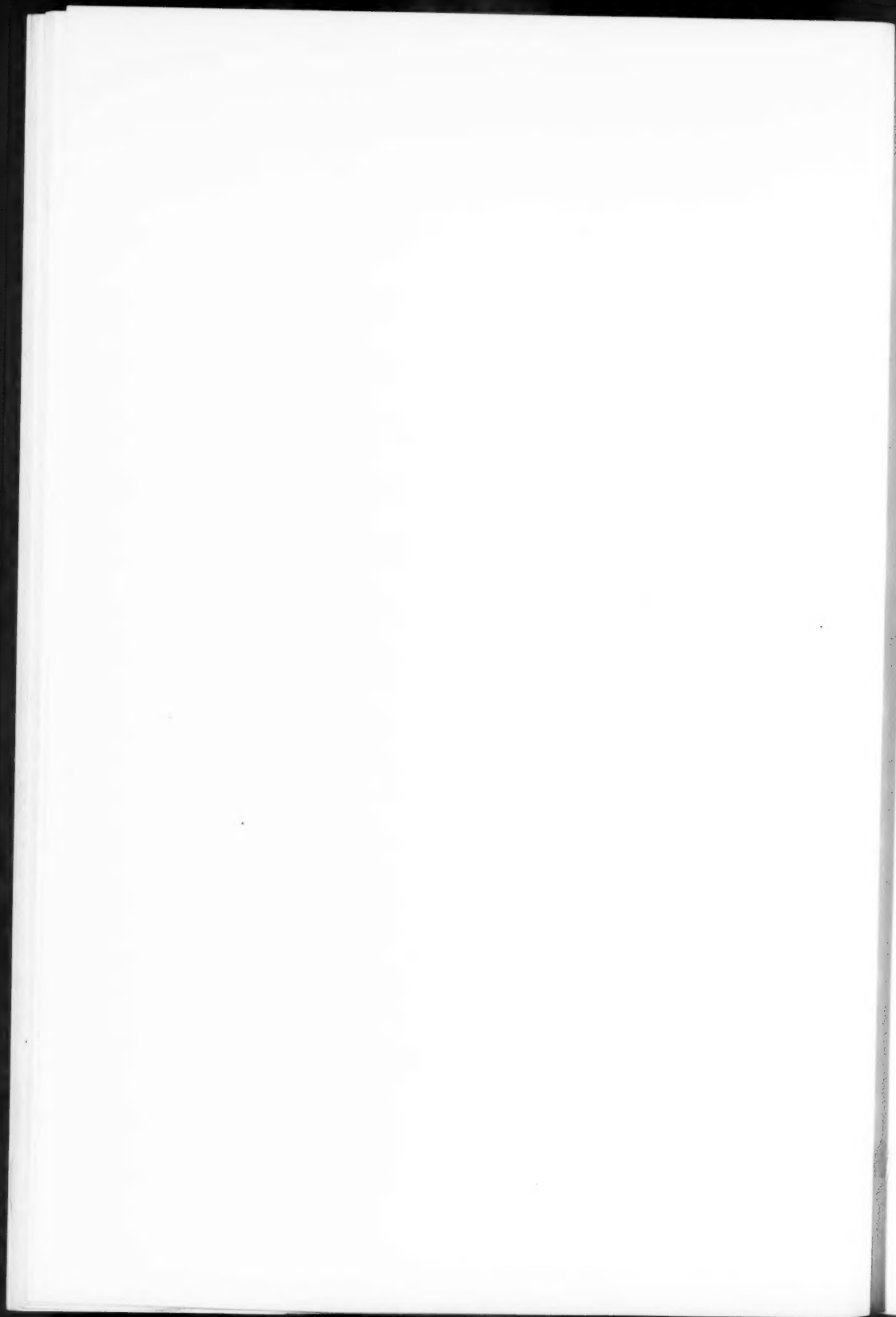
DISCUSSION.

Dr. EVERARD WILLIAMS asked Dr. Marrack whether any estimations had been made of the proteins in the urine. Some observers had found a high globulin content in the urine of pregnancy toxæmias, and if this had been the case in the series under investigation, it might possibly afford the explanation of the low blood globulin fractions which were here recorded. In any case it would make the analysis more complete, if a correction were made for the urinary proteins.

He further asked whether Dr. Marrack had made any estimations of the plasma chlorides, in the course of this investigation, and if so, whether the findings indicated any relationship between plasma chlorides and blood proteins.

The paper was also discussed by Dr. VICTOR LACK.

Dr. MARRACK (in reply) said that estimations of proteins in urine had not been made. He did not think the changes in plasma proteins in nephritis were due to loss of protein in the urine, as he had seen a patient with normal plasma proteins after three years of severe albuminuria. The plasma chlorides in normal and most abnormal pregnancies lay within normal limits.



Section of Obstetrics and Gynaecology.

President—Dr. H. RUSSELL ANDREWS.

Vesical Calculus formed around a Foreign Body.

Shown by SIDNEY FORSDIKE, F.R.C.S.

A PHOSPHATIC stone, oval in shape, weight in the recent state 3 oz., $4\frac{1}{2}$ in. long, $5\frac{1}{4}$ in. in circumference, and enclosing a bone penholder minus the metal fitting.

A. R., single, aged 17, missed two periods in May and June. Soon afterwards, in July, a penholder was passed into the private parts. The penholder disappeared and was not recovered; the patient, of dull intelligence, thought nothing more of it. In August she began to have frequency of micturition, which rapidly increased until September 8, when she was unable to hold her water at all, and was brought to the out-patient department.

A hard lump was felt above the pubes, which, on bimanual examination, proved to be situated in the bladder. The urine contained a large quantity of pus, but no blood. X-ray examination showed a large calculus surrounding a long foreign body.

Suprapubic cystotomy was performed, and upon an attempt being made to lift the stone out it was found to be fixed. Close examination showed that the head of the holder was buried in the right wall of the bladder, and removal intact would have entailed tearing the bladder wall or perforating it. The head of the holder was then cut off with a bone forceps; it was left *in situ*, thus freeing the stone and allowing it to be easily removed. The head was then removed with some little difficulty, a small round ulcer which had almost perforated being left. The bladder was drained and the patient recovered after a prolonged illness.

Incidence of Glycosuria during Pregnancy.

By ARTHUR CROOK, L.R.C.P., M.R.C.S. (Norwich).

THESE notes are based on the result of the examination of the urine of 548 patients during their pregnancy. The routine has been generally a monthly examination, supplemented (in those cases in which the presence of sugar or albumin was found) by more frequent tests. The total number of urine examinations was 1,346, which gives a rough average of between two and three per patient. The specimen of urine examined has not been always the early morning, fasting urine, for this would, as pointed out by Joslin, lead to some cases with sugar not being detected at all, sugar being present in the urine only after a meal, in some patients.

The records date from 1908 to this year, therefore the majority of the cases were under observation before the modern test of the blood-sugar index was in use. Owing to the exigencies of general practice, these records are, in many of the cases, very imperfect.

As to the nature of the clinical material, most of the patients belonged to the rather better working-classes of a manufacturing town, whose diet would be rather more on the generous scale in the use of meat, than in those met with in the purely agricultural districts.

The cases included some with complications, as shown by the following table:—

TABLE SHOWING INCIDENCE OF GLYCOSURIA IN CASES OF PREGNANCY WITH COMPLICATIONS.

Complication	Total number of cases	Number with Glycosuria	Number without Glycosuria	Remarks
Eclampsia...	1	—	1	Severe, forty fits, recovered
Acute alcoholism	1	—	1	Fatal, nearly full time gestation
Morbus cordis	7	3	4	—
Syphilis	1	—	1	Second stage (untreated)
Chorea	1	—	1	—
Tuberculous diseases; hip	1	—	1	Chronic discharging sinuses
Mediastinal tumour	1	—	1	Secondary growths of suprarenals
Simple fibrous goitre	2	1	1	—
Insanity	3	1	2	Morbus cordis in the case with glycosuria
Uterine fibroids	4	3	1	—
Epilepsy	1	—	1	—
Pertussis	1	1	—	—
Tuberculous meningitis	1	1	—	Fatal at third month

Method of Testing.—Boiling the freshly made Fehling solution in the test tube and adding the urine, a few drops at a time, and keeping the mixture at boiling point until nearly as much urine as test solution had been used. Later, the reaction has been carried out by boiling the test solution and urine separately, and mixing equal volumes, then boiling the mixture for five to ten seconds, and allowing the tube to stand and cool for about a minute. Positive results have been checked by the fermentation method, noting the diminished specific gravity, and again testing the filtered, fermented urine for sugar by the same method and finding it absent.

Incidence of Sugar.—Of the 548 patients, 166 had, at some time or other during pregnancy, sugar in the urine, that is, a little over 30 per cent.: twenty-three patients had sugar in the urine in more than one pregnancy, but not in all, that is, about 4 per cent.; eight patients had it in all their recorded pregnancies, that is nearly 15 per cent.

Of the 166 cases of detection of sugar, forty were first noted before the twenty-first week of pregnancy, while 126 were first observed between the twenty-first and fortieth weeks. The week showing the largest number was the twenty-eighth; the next two highest weeks being the twenty-second and thirtieth weeks.

With regard to the cases that were showing sugar at a period of pregnancy not later than the twentieth week, namely, forty cases—if we exclude one case in which the sugar was accompanied by pernicious vomiting, albumin and abortion, spontaneous once, and induced twice, in all three pregnancies (quite an exceptional case)—in twenty-four cases, that is 59 per cent., sugar was definitely recorded to have disappeared before the end of the pregnancy, while of the 126 cases in which it was first detected at the twenty-first week and afterwards, only thirty-four cases, that is 32 per cent., are recorded as not showing sugar in the urine again.

Of the forty early cases, if we exclude the exceptional case before mentioned, no note is recorded of toxæmia at all; while in the 126 later cases, there were four with albumin as well, two had albumin in the next pregnancy, one had sugar still thirteen years later, one showed sugar still ten years afterwards, and ten had marked signs of toxæmia, and three had tachycardia of doubtful origin.

It appears from these figures that about 30 per cent. of these pregnant women passed sugar in the urine, at some time during their pregnancy, and in about one-third of all the cases and in quite one-half of those in which the sugar was first detected before the fifth month it was of short duration, and not associated with other serious signs of disturbance of metabolism. In those cases in which it appeared at about the twenty-eighth to thirtieth week, it was more often associated with signs of toxæmia,

and was more allied perhaps to the so-called "renal diabetes." In about 10 per cent. of these latter cases it lasted till labour, and afterwards.

In the small proportion in which the sugar appeared in every pregnancy (1.5 per cent.) the proportion of definite toxæmias is higher still, 50 per cent. at least; though the total number of these cases is too small to allow of any conclusion to be drawn being reliable.

In cases only first detected at a very late date, that is after the thirty-fourth week, just under 3 per cent. of them had sugar till labour, and afterwards; some of these cases were of the intermittent type.

No special incidence of septic infection was noted in these patients, though it was the rule to record all vaginal discharges, or any septic condition which a complete examination revealed in them.

The average weight of the children at birth of the mothers who had glycosuria at any time during the pregnancy was 7.2 lb.

The sugar was absent in the fasting urine in most of my cases. As to the effect of diet it caused the disappearance of the sugar, I found, if strictly adhered to, thus differing from what is generally stated to occur in "renal diabetes," and from what happened in most of the cases related by Dr. Mackenzie Wallis, in his paper read before this Section, in November, 1921.¹ His statement, however, that the sugar is mostly detected about the sixth month corresponds with the occurrence in my cases, and also the frequent event of severe vomiting at an earlier period of the pregnancy has been the experience of both of us.

In one particular case (No. 2) the result of dieting was disquieting; the urine when sugar-free became loaded with acetone, and the patient manifested symptoms of severe weakness and faintness. This appearance of acetone in the urine where there had been none before the dieting was begun was noticed in the case described by Dr. Graham in his paper read before this Section in May, 1924,² but that was when the patient was at an early age, and before pregnancy had occurred.

Case I.—(2-para—aged 30—her second pregnancy.) No history of first pregnancy; sugar now present at thirty-third week up to 5 gr. per oz. Blood-pressure, 155: œdema of legs, and troublesome pruritus; normal labour—hydramnios. *Third pregnancy:* Now 37 years old—thirty-one weeks pregnant. Urine containing sugar 5 gr. per oz.; sugar persisted till fourteen days before the labour, which was normal, except for hydramnios. This patient suffered from amenorrhœa for four months with enlargement of thyroid and nervous symptoms just before the commencement of this last pregnancy: the thyroid enlargement and nervous signs disappeared with the onset of pregnancy; now, ten years later, she has no glycosuria nor thyroid enlargement, is in good health and has regular periods.

Case II.—(1-para—aged 29; twenty-three weeks pregnant.) Sugar in urine (after meals only) 10 gr. per oz.; no thirst nor polyuria, but excessive frequency of micturition, which occurs, she says, sometimes half-hourly. Blood-sugar: fasting, 0.083, after 100 gm. of glucose, 0.136 after one hour, 0.104 after two hours, 0.079 after three hours. Record taken when on ordinary diet at two hours after the midday meal, 0.166. First two months had excessive vomiting and "air hunger"; these symptoms have now disappeared, but she has pain under right shoulder and in breasts if exposed to cold (it was mid-winter): these symptoms relieved by large doses of sodium bicarbonate. Under strict non-carbohydrate diet, urine became sugar-free, but there was complaint of symptoms of giddiness and weakness and "drunken feeling," and a large quantity of acetone appeared in the urine. Sugar reappeared directly carbohydrates were again taken, even in small quantity, and eventually there was some present in the fasting urine as well; total amount being up to 400 gr. per diem. After premature labour (induced) sugar persisted for six weeks, then disappeared; three months after labour administration of 50 grm. glucose produced blood-sugar 0.18 three hours afterwards, and in urine there was 9 gr. per oz. sugar.

¹ *Proceedings*, 1921-22, xv (Sect. Obst. and Gyn.), p. 31.

² *Proceedings*, 1924, xvii (Sect. Obst. and Gyn.), p. 99.

Case III.—Three successive pregnancies. *First pregnancy:* Aged 32—eleven weeks pregnant—seen because of intractable vomiting for fourteen days. Pulse 100 (persistent), blood-pressure 150-160, scanty urine—high colour, excessive acid reaction; sugar—and trace of albumin; ferric perchloride test positive. Vomiting ceased after one passage of stomach tube and gastric lavage—urine increased in volume and contained more sugar. A fortnight later, aborted spontaneously. *Second pregnancy:* About eight weeks pregnant; vomiting began a week ago and said to be continued. Pulse, 108; temperature, normal. Urine as before, but no sugar—volume very scanty. Improved under gastric lavage and care of trained nurse for a fortnight; sugar appeared in urine; patient then relapsed. Blood-pressure, 120; pulse up again; delirium at night; total urine, 16 oz. per day only. Uterus emptied under anæsthetic and sugar not in urine by the fourteenth day. *Third pregnancy:* Now aged 33; seen at eighth week—vomiting severely; no sugar, but albumin present; seven days later sugar present. Still vomiting often. After twelve days, vomiting less, but patient certified for acute mania and removed to mental hospital; discharged in seven weeks recovered from the mania. Vomiting still severe; sugar still present. Pulse persistently over 100—some night delirium; uterus emptied under anæsthetic—large quantity liq. amnii of dark green colour; no sugar in urine three days later. Now, nine years afterwards, has had no more pregnancies; has become very fat; has no sugar in urine; periods regular.

It has been suggested that these cases were really cases of "renal diabetes" such as has been described, apart from pregnancy; that is, sugar in the urine, with no hyperglycæmia or other diabetic signs. It appeared in two of my cases to be so—the sugar being still present ten and thirteen years afterwards; and yet the patients are still in fair health, certainly not diabetic, and with others of the same family having glycosuria.

In considering this question of renal diabetes and its importance, I mention the work of Barrenger and Roper (see *Amer. Journ. Med. Sciences*, 1907, cxxxiii, pp. 842-55) who, analysing insurance statistics, found that after five years 20 per cent. of their supposed "renal diabetics" had developed true diabetes; 25 per cent. were "suspicious" and 55 per cent. remained free. They also noted that if the glycosuria was recurrent, that is, detected more than once, eight out of eleven cases became diabetic: these figures make one hesitate to say the matter is not important.

Perhaps there are at least two distinct types: (1) A benign type, which is generally first noticed before the fifth month, but may also be only detected very late in the pregnancy, viz., after the thirty-fourth week. This may be transient or intermittent, or it may be a condition which was present before the pregnancy, and will persist after it, unaccompanied by serious symptoms. (2) A type in which the sugar generally commences at about the twenty-first to twenty-third week, is often accompanied by symptoms usually associated with some toxæmia of pregnancy, which continue till the termination of the pregnancy, and in some cases will recur with succeeding pregnancies. It is specially associated with cases in which there has been severe early vomiting, which has often subsided later. In the later months it is also associated with cedema (not necessarily accompanied by albumin), this being a link perhaps with the chloride metabolism. And, again, it is sometimes associated with hydramnios.

In connexion with the supposed comparative innocency of glycosuria before the fifth month, which I suggest as the result of my experience, it may be of interest to recall that in the early months of pregnancy the detection of sugar after a glucose meal has been used as a test for pregnancy (Frank and Nothmann). These writers assert that the test is useless after the fourth month, and if that is so, it seems that the normal pregnant woman can meet most demands after that period on her carbohydrate metabolism. Though the glycosuria met with in pregnancy after the fifth month may bear some relation to "renal diabetes" it does not seem to explain all the other cases (excluding of course true diabetes).

The two patients in my series in whose urine sugar is still present thirteen and

ten years after the pregnancy—though they are still in good health and show no signs of true diabetes—did not, when pregnant, present the symptoms complained of by those patients who rapidly ceased to pass sugar after labour and the puerperium—symptoms such as excessive early vomiting, pruritus, considerable œdema of legs (without albuminuria) or signs of hydramnios.

My only excuse for recording these crude figures and the deduction from them I have suggested, must be the present position of our knowledge of pregnancy and so-called "toxic" pregnancy.

Recent work on the glands of internal secretion seems, at any rate for a time, to have increased the fog between our eyes and the clear picture we all desire to have of the relationship (if any) between pregnancy, "toxic" pregnancy, acidosis and metabolism, carbohydrate and otherwise, to each other. In placing this fragment, as one may figuratively describe it, on the table where lie the parts of an, as yet, unarranged puzzle, I hope that it may lead some others to fill one of the gaps, and so help to its ultimate solution.

Since finishing these notes, I find that I have overlooked the fact that Dr. Herbert French,¹ quoting Saleim in the "Goulstonian Lectures," 1908, gives the incidence of glycosuria in pregnancy as high as 66 per cent., and observes on the frequency of hydramnios in this condition. I have also to mention a case recorded by Dr. R. Wise and published in the *Proceedings* of this Section in 1923,² in which death of the fœtus occurred at the eighth month of the first pregnancy, while in the second pregnancy the symptoms specially noted in addition to polyuria and thirst, were pruritus and faintness, the sugar disappearing after a normal labour with living child.

With reference to this tendency to death of the nearly mature fœtus before birth, I give in conclusion very briefly, notes of a case which occurred within the last few weeks.

A primipara, aged 35, sugar 2 gr. per oz., found at the twenty-fourth week. Hydramnios detected at the twenty-eighth week with great œdema of legs. Sudden death of child at thirty-fifth week, almost complete subsidence of the œdema and sugar before the birth of a 7½-lb. macerated fœtus at the thirty-eighth week.

Discussion.—Dr. H. RUSSELL ANDREWS (President), in congratulating Dr. Crook on his record of clinical observations carried on over a period of sixteen years, said that there was at present a good deal of uncertainty and vagueness about the proper class to which to assign some cases of glycosuria in pregnancy, in spite of much recent work on the subject. The after-histories recorded by Dr. Crook were of value in helping to show the relation of continued glycosuria to diabetes. His own experience was that pregnancy in real diabetes was rare, and that in most cases of glycosuria in pregnancy the patients' health was not affected and no special treatment was necessary.

Dr. KNYVETT GORDON, speaking from the point of view of the pathologist, said that very many pregnant women suffered from glycosuria at one time or another during pregnancy, the proportion being in his experience somewhat over 50 per cent. The great majority of these had no other symptoms and required no specific dietary or medicinal treatment. The same thing happened in regard to the output of urea. If the urea content of the blood in normal pregnancy was estimated it was found as a rule to be lower than the normal, and the same phenomenon occurred with regard to the glucose content of the blood. So also, both in regard to protein and carbohydrate, these occurred less in the blood and more in the urine than in non-pregnant women, or, as the physiologist would put it, the kidney threshold for both urea and glucose was lowered. The explanation in his view lay in the hypothesis that owing to the increased stimulation of the kidneys, probably of an endocrine nature, the kidneys became more active during pregnancy. If this stimulus failed the patient was in danger of toxæmia.

¹ *Lancet*, 1908, i, p. 1399.

² *Proceedings*, 1923, xvi (Sect. Obst. and Gyn.), p. 35.

or even eclampsia. In the majority of the cases cited by Dr. Crook, the blood-sugar had not been estimated, so it was difficult to say whether they were toxæmic or not. The most useful remedy for metabolic disturbance in pregnancy, whether in regard to protein or carbohydrate, was thyroid extract.

Dr. EVERARD WILLIAMS raised the question of the innocence of cases of glycosuria without hyperglycæmia, in pregnancy. He said that further knowledge and experience of cases of renal glycosuria or diabetes innocens, had shown that some of these subsequently developed into true diabetes. It would be well to bear in mind the possibility that at any rate some of these cases might be early cases of renal glycosuria, made manifest by the strain of pregnancy, as had been suggested by another speaker, in cases of so-called alimentary glycosuria.

Section of Obstetrics and Gynaecology.

President—Dr. H. RUSSELL ANDREWS.

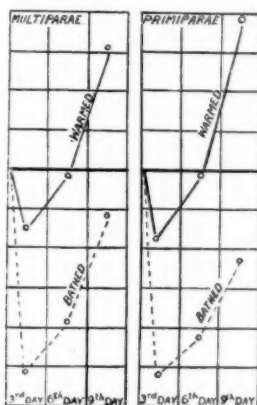
The Relative Loss of Heat and Loss of Weight, and the Treatment of Shock in the New-born.

By A. LOUISE McILROY, M.D., D.Sc.

(I) THE RELATIVE LOSS OF HEAT AND LOSS OF WEIGHT IN THE NEW-BORN.

THE initial drop in the weight of the new-born has hitherto been regarded as inevitable. As a rule the chief fall occurs in the first three days. The average loss is about seven ounces and some authorities state that as much as half a pound is within normal physiological limits. Various causes are assigned, such as adaptation of ante-natal and post-natal metabolism, loss of urine and meconium, and deficient nutriment until the supply of the mother's milk is fully established.

There is no doubt that some loss of weight must occur in the majority of cases but the problem is whether the loss cannot be diminished. Weight cannot be regarded as an absolute indication of the infant's welfare, but is one of the best signs of progress.



A

Some time ago Professor Pembrey pointed out to me that the treatment of the normal new-born was irrational as compared with that of the premature, and he deprecated the process of bathing, as it entails loss of heat. Acting on this suggestion some nine months ago I began to try novel methods of treating new-born infants. A series of normal babies at term were cleaned at birth with olive oil in substitution for bathing.

The results have been satisfactory and now oiling has been instituted as the routine method in the Obstetric Unit at the Royal Free Hospital. The babies are oiled after birth, and daily until the day before they leave the hospital, when the mothers are instructed how to bathe them. They lose less weight than bathed babies, and in most cases regain their birth weights, or reach even higher weights, before leaving hospital on the ninth or tenth day. The cleansing properties of oil are not well understood in this country, and babies treated in the manner described are just

as sweet and clean as if they had been bathed in the ordinary way. The oil is of course wiped off again after application.

After treating a large number of infants I felt that still further improvement could be effected by reducing exposure to the air after birth, thus avoiding loss of heat. Consequently, immediately after delivery the infants in one ward were wrapped in warmed blankets. On the cord being cut the child was at once transferred to a cot with warmed blankets and hot-water bottles and kept there until oiled and dressed. The results in these cases were still further improved.

Appended are two charts—A—displaying results of 160 normal cases selected at random. For purposes of comparison the charts deal with forty oiled and warmed babies of multiparæ; forty bathed (controls) babies of multiparæ; forty oiled and warmed babies of primiparæ; forty bathed (controls) babies of primiparæ.

The oiled and warmed classes on the one hand and the bathed on the other correspond as nearly as possible in their birth months. Birth weights correspond as nearly as possible with a maximum divergence of four ounces.

It will be seen that the records of the oiled and warmed class show a marked improvement, proving that loss of heat means loss of weight in the new-born. Contrary to expectation there appears to be little difference between first and subsequent babies, but in the oiled and warmed class first-born babies show a slight advantage. With the object of estimating the actual heat loss after birth, records of temperatures were kept in 100 oiled and warmed cases.

The temperatures were taken at birth, one or two hours later before oiling and again after. Records of forty of these cases selected at random are appended. Almost invariably they show a fall at the first and second intervals. Children born under abnormal conditions (forceps, &c.) are of course likely to lose more weight than those born normally. Strain during labour, due either to disproportion between the birth canal and fetus, or to prematurity, causes shock and consequent interference with the heat-regulating centres.

Appended (p. 41) are charts B, C, D, E, F, containing individual records of typical cases of various classes.

B—Normal bathed.

C—Normal oiled and warmed (a and b).

D—Premature, oiled and warmed, shows marked loss.

E—Forceps cases; (a) bathed; (b) warmed and oiled.

F—Induction case; considerable loss.

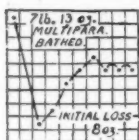
G—Twins, showing loss and gain of both almost the same (a and b).

H—Toxæmia, showing considerable drop.

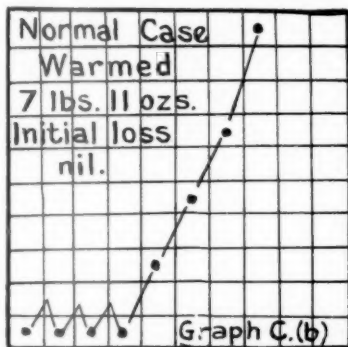
I—Cæsarean section (a and b).

Feeding methods are not involved in the inquiry but we have found that three-hourly feeds produce better weight results than those at four hours. The children were all breast-fed but when the milk supply was deficient supplementary feeds were given at intervals. Early artificial feeding in Cæsarean section cases gives satisfactory results, as the mother's milk is usually deficient during the first few days of the puerperium.

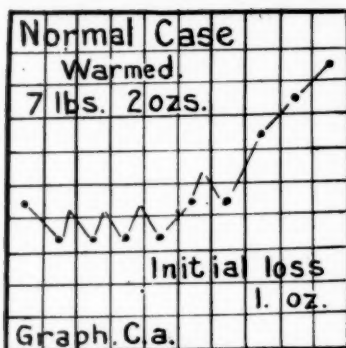
The decrease in infantile mortality during the first year has aroused much attention, but few realize that the decrease during the first month is comparatively small. To put the matter in another way the improvement has been chiefly during the last eleven months of the infantile year. For instance in 1905 the mortality per 1,000 for the first year was 128—between 2-11 months, 86; and that during the first month, 42. In 1923 the corresponding figures were 69, 37·5 and 31·5. According to the Registrar-General's records, deaths during the first month are mainly due to birth injuries and prematurity. Probably much can be done to reduce the mortality by improved methods at and after birth, but ante-natal treatment



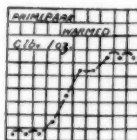
B



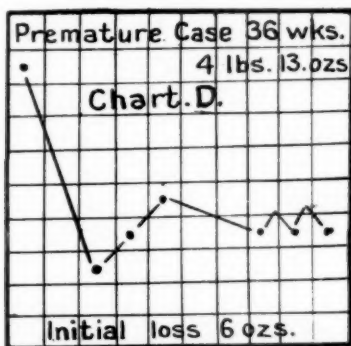
C (b)



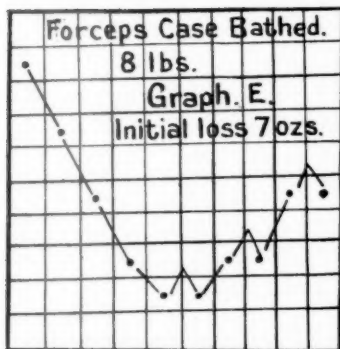
C (a)



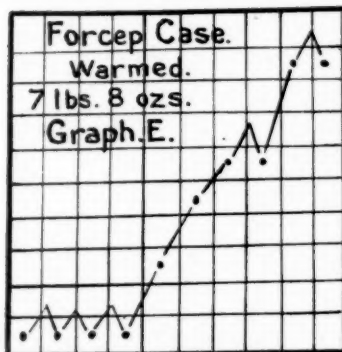
C (c)



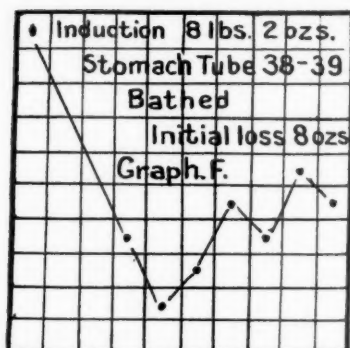
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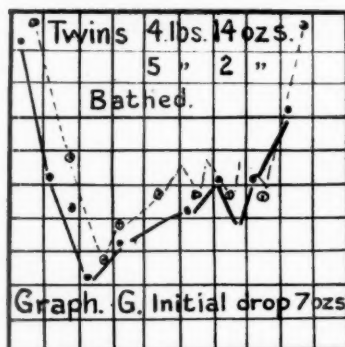
E (a)



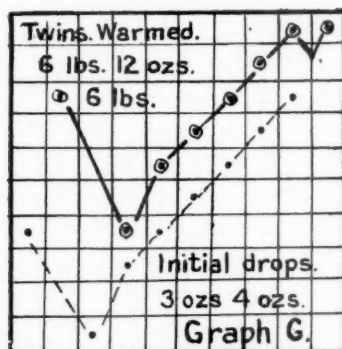
E (b)



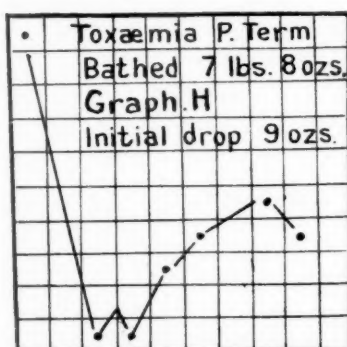
F



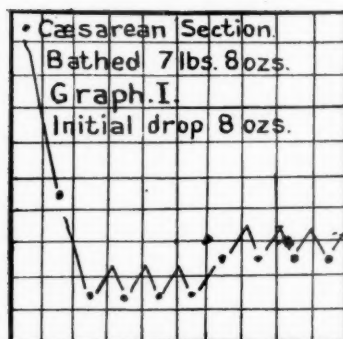
G (a)



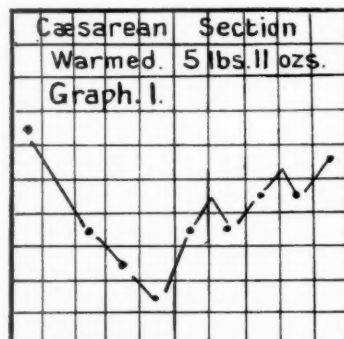
G (b)



H



I (a)



I (b)

is the real remedy. The future of successful obstetric practice lies in skilled supervision during the ante-natal period.

It is obvious that an infant whose mother has suffered during pregnancy from toxæmias or any constitutional disease has a reduced vital capacity. Any form of ante-natal treatment, therefore, which improves the mother's general condition and reduces the possible complications of labour will have a marked effect upon the new-born child.

Ante-natal correction of mal-presentation, the treatment of cardiac complications by rest, the reduction of pain by the administration of sedatives during labour, and the stimulation of a failing foetal heart by strychnine, strophanthus, &c., must have their effect upon the welfare of the infant.

VERTEX PRESENTATION—AT TERM. (OILED AND WARMED.)
Fall of temperature after birth. (Taken in rectum.) Forty cases.

No.	Parity	Temperature at birth ° F.	Temperature after 1 to 2 hours ° F.	Temperature after oiling ° F.	Loss of weight of infant, first three days	Remarks
1	... P. ...	99	98.2—1 hour	98	2 oz.	
2	... P. ...	99.6	98.1—1 "	97	Gain	
3	... P. ...	100.2	99.2—1 "	99	6 oz.	
4	... M. ...	100.4	99.2—1 "	99.2	4 "	
5	... M. ...	100	99 —1 "	98.6	Gain	
6	... P. ...	100	— — 3 "	98.4	8 oz.	Forceps. Post-mature
7	... P. ...	100	99.2— 3 "	99	1 "	
8	... P. ...	100	97 —1 "	—	2 "	Forceps. Foetal distress. Blue, very collapsed irregular heart, pit., oxygen
9	... M. ...	99.8	98.6— 3 "	98.6	3 "	
10	... P. ...	98	98 —1 "	98	2 "	
11	... P. ...	99	97.6—1 1/2 "	97.6	Gain	
12	... P. ...	99.8	99.8—1 1/2 "	98.4	10 1/2 oz.	Forceps. Foetal distress
13	... P. ...	98.2	96.6—2 "	96.4	No loss	
14	... M. ...	98	97.2—1 1/2 "	96.8	Gain	
15	... M. ...	100.4	98.2—1 "	98	Gain	
16	... P. ...	100.8	100 —1 "	99.6	5 oz.	
17	... M. ...	100.4	99 —1 "	98.4	Gain	
18	... P. ...	99	— — 3 "	98.6	4 1/2 oz.	Post-mature. Induction
19	... M. ...	98.2	97.8—1 1/2 "	98	No loss	
20	... M. ...	100	98.6—1 1/2 "	98.2	1 1/2 oz.	
21	... M. ...	98.8	98.6—1 1/2 "	97	3 "	
22	... P. ...	99	97.4—1 1/2 "	97.2	2 1/2 "	
23	... M. ...	100	97.8—1 1/2 "	97.2	No loss	
24	... P. ...	100	97.2— 1/2 "	97	3 oz.	L.O.P., rotated
25	... P. ...	99	97 —1 1/2 "	96	2 "	
26	... M. ...	97.4	97.4—1 1/2 "	97.2	Gain	
27	... M. ...	99.4	99.2—1 1/2 "	99	3 1/2 oz.	
28	... M. ...	99	98 —1 "	97.2	3 "	
29	... M. ...	99	98 —1 1/2 "	97.6	Gain	
30	... P. ...	99.6	98 — 3/4 "	97.8	4 oz.	
31	... M. ...	98	98 —1 1/2 "	97.6	Gain	
32	... P. ...	100.4	98.4—1 1/2 "	97.8	1 oz.	
33	... P. ...	98.2	97.4—1 "	97	6 "	
34	... P. ...	99.8	98.8—2 "	98.6	2 "	
35	... P. ...	100.2	98.6— 1/2 "	98.4	No loss	
36	... P. ...	98	97 — 1/2 "	96	"	
37	... M. ...	—	97.4— 1/2 "	97.2	5 oz.	Forceps, shock, very limp. Mother had tuberculous hip
38	... M. ...	98	97.4— 1/2 "	97.2	6 "	
39	... P. ...	100.2	98.6—1 "	98.4	No loss	
40	... P. ...	98	97 — 1/2 "	96	1 oz.	

(II) SHOCK IN THE NEW-BORN.

The treatment of white asphyxia demands fresh consideration. As a rule blue asphyxia can be treated by clearing out the air passages and stimulating the skin reflexes.

The usual method in cases of white asphyxia is to clear the air passages and

immerse the infant in a warm bath, sometimes with the application of cold water to the head, followed by artificial respiration of varying and progressive degrees of severity, finally culminating in the Schultz method. This arduous effort probably satisfies the obstetrician that if he has failed to restore the infant he cannot be blamed for failing to exercise his energies to their utmost capacity.

The late Mr. Gordon Ley pointed out that in such cases attention should be directed not to establishing respiratory movements but to relieving cardiac failure due to shock, usually caused by cranial compression, or in some cases by pressure upon the umbilical circulation. This, I think, is the correct view. The greater the strain upon the mother during labour the greater the shock to the infant. Respiration cannot be induced until the heart has adapted itself. What is required is cardiac stimulation and not inhibition. Much depends upon the amount of carbon dioxide in the blood for the stimulation of the respiratory centres. It is possible that the reserves for this purpose may be wasted in useless muscular movements carried out upon an infant with an enfeebled heart.

As soon as the child is born it should be wrapped in a warm blanket and its air passages cleared. The mucus should be sucked out of the trachea by a rubber catheter with a glass window, enabling the mucus to be observed before it reaches the operator's mouth. The infant should then be left at rest in a warmed cot. Gentle massage of the heart can be carried out under the blanket, and stimulants such as pituitrin, or camphor, given hypodermically. No movements should be permitted which involve the head or spine. It is difficult at first to abstain from artificial respiration, but experience has proved the efficiency of the alternative method and that results are better than when more vigorous means are employed. Time, warmth and rest will usually restore the heart's action if the shock has not been so severe as to be incompatible with life. In cases of blocked air passages artificial respiration may be advocated but the use of the mucus catheter will generally be successful. Heart failure is not usually the cause in such instances.

After the administration of scopolamine or morphine, or even of ether, during labour, the infant is often born in an anæsthetized condition. It must therefore be allowed time to recover.

I am grateful to my Obstetric Resident Physicians—Miss Langston and Miss Salmond, and to the Students and Sisters, for assisting me with these investigations.

A record of several hundred cases has been charted and classified by Miss Pritchard, whose services became available through the generosity of the Medical Research Council.

DISCUSSION.

Dr. H. RUSSELL ANDREWS (President) remarked that papers on subjects concerning the new-born infant were too rarely brought before the Section. He said he thought that some of the charts shown were very striking. He agreed that in cases of white asphyxia the less vigour used in attempts at resuscitation the more likely were they to be successful. He also said he believed that the method of putting cold water on the infant's head had not been taught for many years.

Dr. KNYVETT GORDON asked what was the weight of the olive oil used for inunction; it seemed possible that the diminished loss of weight recorded in the oiled babies might be accounted for by absorption of so much oil. That absorption from the skin of new-born infants readily occurred was shown by a case in which the vagina of the mother had been painted and swabbed freely with violet-green, and shortly after birth the urine of the baby was coloured with the same dye. Oily substances were, as a rule, more easily absorbed than watery solutions. As regarded loss of heat, oil would retard and water accelerate evaporation and consequent lowering of temperature. Some of the oil would possibly be absorbed as a food.

Professor McILROY (in reply to Dr. Knyvett Gordon) said that with regard to the question of absorption of olive oil she had no proof of this taking place, but that she considered it was possible.

Section of Obstetrics and Gynaecology.

President—Dr. H. RUSSELL ANDREWS.

A Case of Carcinoma of the Female Urethra, treated by Partial Resection of the Urethra.

By A. C. PALMER, F.R.C.S.

MY apology for bringing this case before the Section lies in the comparative rarity of carcinoma of the female urethra, only fifteen cases being hitherto recorded in English surgical literature. Fletcher Shaw [1] mentions that the first case in English literature was reported by MacGill [2] in 1890; whereas the first case in surgical literature was that of Mme. Boivin [3] in 1833. Fletcher Shaw brought the total number of reported cases up to 102. I have found two others: one, by Duvergey [4] and Dax; the other, by V. J. O'Connor [5]; these cases and my own bringing the total to 105.

Two surgical operations have been performed for this condition.

(1) *Total Resection of the Urethra, with permanent suprapubic drainage, or permanent vaginal fistula.*—Of the reported cases, 30 were treated in this way, 22 with vaginal fistula, 8 with suprapubic drainage. Control was obtained in only 12. Venot and Parcelier [6] record recurrence in 6 of these cases, but do not say how long after the operation the recurrence was noted. Five of the English cases were treated by total resection; two were free from recurrence at the end of one year; one recurred in four months; the others were lost sight of. Crossen [7], reviewing the results of 25 cases, found only 8 free from recurrence at the end of three years. In view of the frequency of post-operative incontinence and early recurrence of the growth total resection cannot be regarded as satisfactory. The rate of recurrence appears to be very little better than that obtained by partial resection.

(2) *Partial Resection of the Urethra with retention of the Internal Sphincter.*—Venot and Parcelier record recurrence in 9 cases out of 28 treated by partial resection, but do not say how long after the operation recurrence was observed. Control was obtained in 25 cases. Melchiori [8] has reported one case free six years, and Von Winckel [9] one case free three years after operation. Four of the English cases were treated by partial resection: Cuthbert Lockyer's [10] case, free five years after operation; Mrs. Stanley Boyd's [11] case, free four years after operation; another, well at the end of three months; another, with recurrence at the end of three months. Cuthbert Lockyer's case had control, the other three had permanent incontinence.

Three other cases were so advanced that operation was not attempted. Fletcher Shaw treated his two cases with radium. One of these was free from local recurrence at the end of three years, but the inguinal glands on both sides exhibited carcinoma one year after treatment, and again, three years after treatment, a small carcinomatous gland was removed from the right groin. V. J. O'Connor's case was very advanced, with enlarged glands in each groin when first seen. The urethra was treated by diathermy; following this the local growth completely disappeared, and perfect healing resulted. The urethra contracted to 0.5 cm. in length, but complete control was maintained. The inguinal regions were treated by radium radiation on two occasions; 1,000 mgm. hours on the left side, two days after diathermy, and 500 mgm. hours on both sides, four months after diathermy. Approximately three months later the glands in both groins had become greatly enlarged and painful; the left leg was swollen and oedematous. The patient gradually declined and died eight months after the beginning of treatment.

Whereas the symptoms usually complained of are irritation of the vulva and painful, frequent micturition, it is a point of interest that my patient complained only of bleeding and an inability to hold her water once the need to pass it was recognized. The patient, in O'Connor's case, complained of bleeding and burning pain on micturition, but not of frequency.

Case A. M.—A married woman, aged 56, complained of bleeding from the vulva. Slight bleeding had first been noticed six months before she came for treatment. The loss had been slight and occasional for four months; this had been followed by considerable daily loss for thirty days; since then bleeding had been irregular and variable in amount.

The past history was unimportant. There had been twelve pregnancies, four terminating in miscarriages.

Confinements had been easy, and there was no history of urinary infection, of caruncle, or of vaginal discharge. The menstrual periods had been regular until three years ago, when they ceased. The patient stated that she was quite well apart from the bleeding. She was a plump, well-developed, muscular woman. No bleeding was seen in the vulva at the beginning of the examination; both vagina and cervix felt normal to the finger. Owing to excess of abdominal fat, bimanual examination was impossible, but the uterus felt very light and was mobile. On using the vaginal speculum the cervix was seen to be somewhat atrophic but otherwise normal.

As the speculum was being withdrawn from the vagina, it was noticed that some blood was present on the left labium minus. The blood was found to be oozing gently from the external urethral meatus; the meatus appeared normal, but, on pressure of the floor of the urethra upwards, a red granular nodule, about the size of a pea, protruded from the meatus. A sound easily passed into the substance of the nodule and caused an increase in the bleeding. The anterior vaginal wall for about an inch along the line of the urethra felt harder than the remainder of the mucous membrane.

The patient now stated that she had noticed no change in the number of times she passed urine, but that she had to go at once, when the desire came, otherwise she could not control it well. She was quite definite about having no pain. No glands were felt in the groins. The condition was provisionally diagnosed as carcinoma of the urethra, and the patient was taken into hospital with a view to completing the diagnosis by microscopical section.

When examined under an anæsthetic on October 31, 1924, the growth was found to occupy the floor of the urethra, extending backwards in the form of a ridge for about an inch.

The floor of the urethra was freed from the vaginal mucosa and the growth removed, a very narrow fringe of normal urethral mucosa—not more than 0.3 cm. wide—being removed with the growth.

The portion removed measured 3 cm. long, by 1.3 cm. wide, by 1.2 cm. deep. The growth appeared to have dilated the urethra, rendering possible the suturing together of the cut edges of the urethra along the posterior half of the incision. The anterior half of the incision was repaired by turning up the vaginal mucosa and suturing it transversely to the cut edges of the urethra. A self-retaining catheter was inserted for three days. One expected the wound to break down somewhat, but this did not happen; nor, to my surprise has there been any loss of control since the operation. The urethra in this instance has shown a remarkable capacity for re-forming itself, for two months after operation it is extremely difficult to recognize that there has been any surgical interference.

Dr. E. H. Shaw, Pathologist to the Royal Northern Hospital, has kindly supplied me with the report on the growth as follows:—

"The section shows masses of epithelial cells separated by strands of fibrous tissue, which is more abundant in the deeper part. The cells are elongated in shape, and transition from this variety to an atypical squamous cell very like that of the common variety of squamous-celled carcinoma of the cervix, is seen in some of the masses. The surface of the growth is ulcerated; many small, round inflammatory cells are scattered through the deeper part of the section. An alveolar distribution of the cells is noticed at the spreading margin. The tissue surrounding the growth is composed of fibro-muscular bundles."

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Discussion.—Dr. H. RUSSELL ANDREWS (President) asked Mr. Palmer if he had any reason for not removing the inguinal glands as he would have done if the growth had been just outside the urethra instead of inside it.

Dr. FAIRBAIRN said he had only had one case of urethral carcinoma on which operation had been possible and the result in that case was not fortunate, as in the majority of cases mentioned by Mr. Palmer. In his case the patient was a young woman of 34 and the whole urethra was involved; complete incontinence followed, as part of the neck of the bladder was excised. She died four months later with both local recurrence and diffusion in the pelvis. The case was recorded in the *Journal of Obstetrics and Gynæcology of the British Empire*, xx, p. 306. At the time the case was seen treatment by radium was not available, but now it was clear that better results might be expected from local treatment by radium with clearing out of the glands of the groin by operation.

Dr. T. W. EDEN said that he had only seen two cases of primary urethral cancer; one was inoperable, the other he had operated upon, but as the case had been lost sight of, he could not give the after-history. He thought that this condition could probably be most hopefully dealt with by combining operation with irradiation, radium being used for the region of the primary growth and X-rays for the glands in the inguinal region.

Two Specimens Clinically Simulating Ovarian Cysts:

(1) Carcinoma of the Gall-bladder; (2) Enlargement of the Spleen.

Shown by W. McK. H. McCULLAGH, D.S.O., F.R.C.S.

THE first specimen was removed post mortem from a patient aged 57. Her climacteric occurred, and her symptoms began, three years before admission to hospital.

Her complaint was of a tumour in the right side of the abdomen which had become rapidly larger in the last four months, and subject to attacks of pain and tenderness. She looked acutely ill and anæmic.

Abdominal and vaginal examination both showed a rounded, tender, cystic mass to the right of an enlarged uterus. It seemed to pass under the liver. A twisted or malignant ovarian cyst was diagnosed.

An exploratory laparotomy revealed fibroids of the uterus and a large dark tumour adherent to the intestines, not connected with the pelvic organs, but arising from the edge of the liver. At the post-mortem the specimen was removed; it consists of the liver and a large columnar-celled carcinoma of the gall-bladder arising from its fundus and invading the edge of the liver. It is seven inches in diameter.

The second specimen belongs to an obese single woman of 43. For three years she had complained of severe loss at her periods and a swelling on her left side.

She appeared anæmic, and on examination a rounded tumour was found in the left lower abdomen, of doubtful consistency owing to her obesity. It could also be palpated vaginally to the left of an enlarged uterus, and showed no irregularities in its contour. It was regarded as a pedunculated fibroid or ovarian cyst.

A laparotomy revealed a dark, purple spleen, and a uterus enlarged by fibroids. Owing to the fat infiltration of the omentum and mesentery, the spleen could not be replaced in the splenic area and hung naturally in the pelvis. It was removed after multiple small mass ligatures of its mesentery and re-ligature of the arteries in the stumps. The enlargement was found by microscopic examination and blood-count to be due to splenic anæmia. Chronic leukæmia is a commoner cause of such a mistaken diagnosis.

The main danger in removing an enlarged spleen is hæmorrhage owing to the

slipping of one of the strongly pulsating arteries from a large mass ligature. I have seen one such fatal case post mortem.

These two cases appeared on the operating table consecutively on one day.

Dr. H. RUSSELL ANDREWS (President) said that he and others had been puzzled by the expression "Pseudo-ovarian cysts." He agreed that the diagnosis of a large tumour was very difficult when the pelvic cavity was filled by uterine fibroids. He had, in several cases, felt a spleen on vaginal examination but had only removed one. He opened the abdomen expecting to find an ovarian tumour with a twisted pedicle, but found that the tumour was an enlarged spleen, weighing 11 lb., with acute torsion of the pedicle. The patient did very well.

Osteomalacia in China.¹

By J. PRESTON MAXWELL, M.D., F.R.C.S.

Professor of Obstetrics and Gynæcology, Peking Union Medical College.
and

LEE M. MILES, M.D.

Associate in Obstetrics and Gynæcology, Peking Union Medical College.

[Contribution from the Department of Obstetrics and Gynæcology, Peking Union Medical College.]

HISTORY.

THERE is absence of evidence as to when this disease was first noted in China. We have not come across any trace of it so far in the old obstetrical works, one of which dates back to at least 700 to 500 years B.C.; yet it is a disease which ought to have been recognized, as its consequences are so dramatic.

Of course one has to remember that the parts of China which are most cursed with the disease, namely, Shansi and Shensi, are provinces which, in the early days, were very sparsely populated.

SECTION I.—DISTRIBUTION.

As far as China is concerned, the distribution is as follows: The heaviest incidence is found in the northern three-fourths of the province of Shansi, the southern limits being Luanfu on the east and Pingyangfu on the west. It is not evenly spread over the province, the incidence being especially heavy about the centre of this region, at Pingtingchow, Shouyang, and Yütze. In these places the incidence is surprisingly heavy.

We have taken Pingtingchow as the special place for our study of the disease, partly because of the high incidence and partly because there is in that city a well-equipped Medical Mission Hospital the head of which, Dr. F. J. Wampler [25], has been much interested in the disease and has given us the heartiest help and co-operation. Its severity there will be readily realized when we tell you that in one week one of us saw twenty-two well-marked cases in part of the city and one of the suburbs. The whole city and suburbs contain about 15,000 inhabitants, and it is probable that at least one in fifty of the women of child-bearing age have, or have had, the disease. We know of one family at Pingtingchow where three daughters-in-law died one after the other from obstructed labour due to osteomalacia. The Chinese themselves claim that the incidence may rise to as high as 10 per cent. of the child-bearing women of adult age. The whole affected area is on a high plateau, 2,000 to 3,000 feet above the sea.

In Shensi it is the northern half of the province which is affected. This is very mountainous. There are few cities and the population is a scattered one. The incidence in this region is probably about 1 per cent. or 2 per cent. of the women of

¹The full paper with tables and illustrations will appear in the *British Journal of Obstetrics and Gynæcology* in October, 1925.

child-bearing age, and there are probably 2 per cent. to 3 per cent. of pregnant women who die in labour from this disease.

The area in Shensi does not extend south as far as the Sianfu line and, speaking generally, is much above the sea level.

In the province of Kansu the middle belt, which is a high table-land, is affected. There have been cases both at Lanchow and Pingliang, but there are very few medical men with Western training in the province, and it is not known whether the incidence is really heavy.

A letter from Dr. George E. King, of Lanchow, dated June 23, 1924, tells one of us that there have been six Cæsarean sections there for osteomalacia during the first half of the year. Another case was waiting for operation as he wrote. One of these cases was in a Mohammedan, among the members of which persuasion it undoubtedly occurs.

The whole of Manchuria is affected with the disease, cases having occurred as far south as Newchang, so far north as Ashiho, and in the centre, as at Mukden and Liaoyang. But it is of a distinctly milder type than the Shansi type, and Cæsarean sections for deformity of the pelvis are of much rarer occurrence.

There are also several small patches scattered about the Republic.

There is a tiny patch near Changsha, Hunan, where the major trouble for which the patients came to the Hunan-Yale Hospital was deformity of the chest. There have been no Cæsarean sections at the Hunan-Yale Hospital for pelvic deformity due to this affection.

There is another small patch around Kongmoon in the Canton province, and not only has the disease been seen there in the Chinese, but one Australian woman developed the disease there. She was married to a Chinese and had been living with and as the Chinese, so that she was subject to the same causes which act in their case. With the exception of this one foreign case, we have heard of no other foreign case developing in China.

Another small patch is around Suifu in the province of Szachwan, and there is another small patch around Kweiteh in north Honan. Occasional cases occur in Peking and the province of Chihli, and at one time there were a number of cases in an area on the outskirts of Tientsin, but the disease does not seem to be prevalent there at the present time.

SECTION II.—GENERAL PICTURE OF THE DISEASE.

What is the general picture of the disease as seen in China? Of course, one meets with mild cases as well as severe ones. As known by the patient, it goes under the name of "Yao T'ui T'eng," or "back and thigh pain." In some cases pain in the lumbar region precedes that in the thighs. It is of an aching character, coming and going, better some days than others, and worse during the winter months than in the summer. There may be pain alone and apparently no bone change, although even in these cases, on X-ray examination, there is evidence of the disease.

As a rule, however, the trouble progresses and the bones become distinctly tender, especially over the lower ribs and the pelvic girdle. The bones become soft in spots, and the incidence of the disease on various areas of the bony system is most erratic, and we cannot as yet explain the reason for the differences. One woman may suffer in the pelvis alone, another in the chest, and a third in both. There are also alterations in the long bones, so that a marked coxa vara may be produced, quite apart from the bendings which may appear in the humerus or femur or elsewhere.

The affection of the pelvis takes a characteristic form. The sacrum becomes shortened by reason of the development of a marked convexity backwards, so that the lower part tends, with the coccyx, to come forward and narrow the outlet; the acetabula are carried inward; the symphysis pubis splayed outward; and the descending rami of the pubes and the ascending rami of the ischia tend to approximate so that the vaginal outlet is almost blocked. The tuberosities may actually touch and

tend to be carried a little backward, so that the outlet of the pelvis may be reduced almost to the vanishing point. The upper strait of the pelvis becomes triradiate, and the last lumbar vertebra may slide a little forward, possibly aided by the alteration in the shape of the sacrum.

In the chest a lateral curvature with some rotation of the upper dorsal spine takes place, and the neck seems to sink into the upper portion of the chest, thus reducing the height by several inches. The chest itself tends to become barrel-shaped, the sternum crumples up, curves forward and forms a marked prominence on the front of the chest wall, the distance from the suprasternal notch to the ensiform cartilage being reduced to two inches or under.

The result of this deformity of the spine, combined with a developed weakness in the muscles, is that the patient tends to walk less and less, and sits on the "kang" in a characteristic attitude, bending slightly forward and resting the weight of the head on a pillow. In some cases I have known, this attitude had been maintained for the best part of the year. The patient may come to the hospital complaining of inability to walk and may consider that she is suffering from paralysis.

In the long bones irregular curves take place, but as a rule these are distinctly rarer than the changes in the chest and pelvis. There is generally a little inflammation about the alveolar edge of the gums, not due to pyorrhœa, and in one case we specially studied this made eating difficult. Apparently the teeth are not directly involved, but the jaw is affected, so that the teeth, although perfectly healthy, tend to become loose and some of them may drop out. If they do not drop out, as the patient recovers they again become firm. Tetany is very common. Probably the majority of patients suffer from it at one time or another. Of the last four cases which were specially studied three of the women confessed to having had tetany; the fourth one declared that she had never had it, but she had a typical attack while under study in the hospital. The spasms may last for hours and then leave a numbness in legs and arms; or there may be the development of a numbness, called by the people "ma mu," which is closely allied to tetany and often precedes it.

What is the ordinary history of one of these cases? A typical history would be as follows: First pregnancy, normal; second pregnancy, pain during last few months, normal labour; third pregnancy, pain beginning early in pregnancy, say about the third month, craniotomy or forceps needed at time of labour; fourth pregnancy, pain as already described but more severe, a difficult craniotomy or a Cæsarean section needed, birth of a living child in the ordinary way impossible.

This is the usually accepted text-book picture, and one meets many cases that correspond to it.

But is this the normal course of events? Can one not go further back and find an earlier history? We believe that we can, and in this our opinion is borne out by King, of Kansu, who speaks of the disease there as being one of girls before marriage, becoming worse or *recurring* in the pregnancy period. Wampler, of Pingtingchow, has also remarked to us on the number of cases in which there seem to be symptoms of osteomalacia during the puberty period.

One of the most marked cases we know is that of a male, in whom the symptoms began just before puberty, and we also know of one very well-marked case in a woman in Pingtingchow, in whom symptoms began at the age of 12-13.

But we are fortunately able to go further to-night in this direction. Just before leaving Peking there came into our hands a most interesting paper by Mr. Liu Shih Hao, a student in the Peking Union Medical College, on two cases of tetany occurring in girls from an orphanage near Peking. Nothing had been noticed about the bones save that the X-ray evidence of rickets was absent. Considering, however, the history

¹ The "kang" is a heated platform on which the ordinary person in North China sleeps. It is built of brick or dried earth-brick and is heated by flues running in both a longitudinal and transverse direction. It may be stoked from outside the house or from inside, and the fuel used consists of coal or sorghum stalks.

of tetany, the low blood-calcium, and the age of the girls, it was arguable that here were two cases where there might very possibly be evidence of commencing osteomalacia. We sent back for them, had X-ray pictures taken of the pelvis and the latter measured, and we show you the actual films to-night. The measurements of the pelvis were as follows:—

Case I.—Pai Jui Feng, aged 15 :

Interspinous	21 cm.
Intercristal	22 „
Ext. conjugate	14 „
Interischial	6 „

Case II.—Li Fu Chen, aged 16 ;

Interspinous	22 cm.
Intercristal	24 „
Ext. conjugate	16'5 „
Interischial	6'5 „

The bones were markedly calcium short and gave weak pictures; you will note that in the most serious case a distinct deformity of the pelvis is already present. These cases will be further discussed when we come to the question of aetiology.

The disease may appear in the first pregnancy, and may progress so fast that a Cæsarean section may be necessary at the time of the first labour. Or the disease may appear in several successive pregnancies without causing much difficulty at labour, and then suddenly take on malignant characters, so that it is impossible for the next labour to take place naturally, and the only hope of the woman and child is a Cæsarean section. As a rule the disease clears up after lactation is complete, but may recur again and again. Some of the victims seem to have symptoms of it extending over many years, and in a minority the disease begins and steadily progresses, rendering the patients bed-ridden invalids, till they die of asthenia or some intercurrent affection. Some of them die of pneumonia, but there does not seem to be any absolute course in case-histories of the affection. In a certain number the main symptoms of the disease appear immediately after the cessation of the calcium drainage, or after the strain of labour, and may progressively increase for a time in spite of treatment.

Take for instance the following case. Here is a woman who in each succeeding pregnancy has suffered with increasing severity from the pains which are such a pronounced symptom of the disease, without the bones being markedly affected.

Case No. 3928.—Li Hau Shih, aged 31, native of Chihli (Lanchow region). Family and past history good. Married when she was twenty-five years old. She has four children all born at full term and still alive and well.

In the first pregnancy the pain of osteomalacia began about two weeks after childbirth and consisted of aching in the lumbar regions. It gradually disappeared and was gone after a year.

In the second pregnancy the pain came on at about the same time and lasted three months. It was more severe than in the first pregnancy.

In the third pregnancy the pain came on at about the same time, but its distribution was wider in area, spreading into the thighs. It lasted five months, and was worse than in the second pregnancy. In spite of the pain she was able to get up and be about during these three lactation periods.

In the fourth pregnancy the pain came on three days after childbirth and gradually spread, till after twenty days she had pain in her back, arms and legs. The pain was intermittent and aching, varying in duration from a few minutes to several hours. She was able to walk about with difficulty for about three weeks, and since then she has been lying down owing to pain and weakness.

The muscles seem to be tender; the reflexes are a little increased; but the electrical reaction of the muscles is normal. The grasp of the hand is weak and the patient does not move very readily. Under X-ray examination the bones are normal, except they are a little more transparent than is usual. The blood is normal, except for the blood calcium, which on admission was only 6'8 mgm. per 100 c.c. serum. The urine is normal. The joints appear to be perfectly normal and there is no œdema. The gums are a little inflamed but the teeth are in good condition and there was no pyorrhœa. After three weeks in the hospital under

treatment the blood calcium was 11.3 mgm. per 100 c.c. serum, that is, the normal figure. The pains were gradually diminishing and the patient was sleeping and eating well.

There are cases in which the weakness of the muscles seems to be the symptom of which most complaint is made, and your patients are brought to you because they complain that they are paralysed and cannot walk.

Two such cases have come under our notice lately. Whilst away from Peking one of us was appealed to on behalf of a patient who was said to have gradually become paralysed three months after labour. One of our assistants was sent to look her up, and we speculated on its being an early case of osteomalacia. The report was not full enough to be certain of the diagnosis, but our assistant had put her on large doses of cod-liver oil and calcium lactate, and a fortnight to three weeks after this date she walked into our consulting-room.

In June last we had in the wards a woman who presented a typical picture of osteomalacia; she had come down from Northern Shansi, a day and a half's journey, to the hospital as a case of paralysis.

SECTION III.—THE FÆTUS IN CASES OF MATERNAL OSTEOMALACIA.

Some of the questions which arise in connexion with this disease are the following: What is the condition of the fœtus when the mother has osteomalacia? Does it suffer with the mother, or is the entire brunt of the disease borne by her? If it suffers, considering the close relationship between osteomalacia and rickets, is there any evidence of prenatal rickets?

Having been unable to find any papers dealing with this subject, we first of all took up this question under the impression that the entire brunt of the disease was borne by the mother. Apparently this is by no means the case. Seven fœtuses, taken by Cæsarean section from mothers with obstructed labour due to osteomalacia, have been carefully examined. Five of these were at full term, the sixth at about seven and one-half months, and the seventh at eight months. All these fœtuses came from pure Chinese mothers; and for purposes of comparison bones from eight fœtuses, which were born of normal Chinese mothers and died from accidents of childbirth, seven being full-term fœtuses and the eighth an eight-months fœtus, were also examined.

Röntgenograms show that the proper centres of ossification are present. Two out of three of the fœtuses, however, from mothers with active osteomalacia showed curious changes about the ends of some of the bones, especially the ulna, the main change being an apparent cupping of the end of the bone, which at once suggested the possibility of this cupping being rickety in nature. There is not, however, in the röntgenogram the irregular outline characteristic of a rickety cupping. The mothers of the other four were apparently suffering less from the disease than from the deformity.

The next step in the investigation was to remove specimens of these affected bones, the radius and ulna being selected. It was noted that the bones appeared to be a little smaller and possibly lighter than the normal, and that they could be split with a knife with greater ease than is usual in those coming from a normal fœtus. For comparison the same bones were also removed from a normal fœtus, which had died at full term from an accident of labour. The bones in this case were larger and harder. Even if some of these show certain differences, due to the size of the woman and the tendency of some women to have larger children than others, there is no doubt that the balance is heavily against the bone from the fœtus conceived by the mother with osteomalacia, both in size and hardness, and to a less degree in weight.

Sections of these bones were now prepared and stained in various ways, and the following facts became at once evident:—

(1) The amount of osseous tissue in the bone coming from the normal control was much greater than that in the osteomalacic fœtus. In other words, the bone from the fœtus in osteomalacia was ostein short.

(2) In the specimen from the foetus in osteomalacia the chondrin stained faintly, this being almost certainly due to a shortage of chondrin and a poor quality of chondrin.

(3) In the normal control the zone of calcification was present, whereas in the bone of the foetus in osteomalacia this zone was hardly, if at all, visible. There was no irregularity of the zone of calcification, nor a linking of the cell columns such as one finds in rickets; the appearance of the specimen rather bore out the dictum, that even if the diet is deficient in the proper proportion of its elements, the child will not develop rickets whilst the supply is at a starvation level. The appearance is more that of osteoporosis.

(4) The apparent cupping of the ulna was due to a little advanced calcification on the two sides of the section below the periosteum.

When we turn from these appearances to the analysis of foetal bones carried out by one of us (Miles), we find ample confirmation of the facts which have already been stated. The femur was taken as the most useful; it was carefully cleaned from all extraneous material and then analysed.

When the figures are examined, it at once becomes noticeable that the proportions of the salts to one another as regards calcium and phosphorus are unaffected, but the difference in the ash per thousand is very marked, and one may safely say that there is a definite calcium and phosphorus deficiency as compared with normal bones. It is also clear that although there may be a retention of sulphur and magnesium during the active state of the disease, the bones of the foetus do not show it, as the magnesium count in the osteomalacia cases is distinctly low.

It would seem from the analyses that the foetus fails to obtain enough calcium where the mother's blood is deficient in this mineral.

A foetus removed by Cæsarean section from a markedly active case of osteomalacia, and still living, showed at the age of one and a half months certain changes in the ends of the femora.

The note concerning the child is as follows:—

Baby Chi (mother's No., 2089), No. 2123. Peking Union Medical College Hospital. Cæsarean baby (for osteomalacia); carried very high owing to shape of pelvis. The child was very fond of lying almost prone with its knees drawn up. When one and a half months old an X-ray was taken. There were slight changes in distal ends of femora. Dr. Hodges suggested that the child had rickets or syphilis. The Wassermann reaction was negative and the history of mother clean. Dr. Brackett examined specially for rickets, and confirmed our opinion that there were no clinical signs of this disease; he doubted whether the X-ray findings were those of rickets, though he was not prepared to diagnose the case from the slight changes in the femora. The baby went home fat and well, and was quite well two and a half years later, with no signs of rickets.

It is a question whether some of these children, if placed under favourable circumstances, may not develop some of the röntgenographic appearances of rickets as they recover from the osteomalacic influence. Dunham [4] has published the account of such a case, in which there was no doubt that the condition of the mother had a definite influence upon the early appearance of rickets in the infant.

One notices, when these children are born, that the subcutaneous fat is deficient and the skin tends to be wrinkled, but this condition rapidly improves and otherwise the children appear to be perfectly normal. Another interesting point is that at the operation of Cæsarean section in mothers with osteomalacia the omentum will be found to be shrunk and practically devoid of fat.

It may be asked what is the condition of the blood of these foetuses as regards the proportion of calcium. Here again, one has carefully to distinguish between foetuses born from mothers who are affected with active osteomalacia and those born from mothers who have fully recovered from the disease. Apparently there is little divergence from the normal in the latter cases, and our figures for the calcium in the cord blood average about 10.3 mgm. of calcium per 100 c.c. of serum.

We have not enough data to enable us to speak about the proportion of calcium in the cord blood of foetuses from cases of active osteomalacia.

Hess and Matzner [10] give us a normal figure for the cord blood, 10.75 mgm. per 100 c.c., and Bogert and Plass [1] give 10.9 mgm. per 100 c.c.

SECTION IV.—ÆTIOLOGY.

Let us first try to clear the ground a little. Has the geological formation anything to do with the distribution of the disease?

By the kindness of Dr. Ting, late of the Geological Department of China, we are able to show a map of the Shansi formation. It has already been stated that the whole region is about 2,000 to 3,000 ft. above the sea. Looking at the maps you will see that a large number of the cases occur on loess formation, and a large number on limestone, a further number on the sandstone, and a further number in the north of the province over the Algonkian. There does not seem to be any selection of the areas covered by these formations in the incidence of the disease.

The water mostly falls away to the East and to the West, entering the Yellow River on the one side, and the rivers running into the Gulf of Pechihli on the other. The drinking water is mostly obtained from shallow surface wells, and the people as a rule drink little unboiled water. So that we think we may rule out the actual geological formation as having little to do with the disease, save that the altitude causes the winters to be longer and more severe, and increases, especially in the case of bound-footed women, the confinement to the house.

Next let us consider the question of the ovaries. Although not generally put in this bold way, the ovaries have been considered as the cause of the disease, and their removal advocated on this ground. There is no evidence worth the name which proves the disease to be connected with pathological changes of the ovaries. One of us has carefully examined ovaries taken from active osteomalacia cases at the time of a Cæsarean section, and compared them with the ovary removed from a Cæsarean section case for another cause, and failed to find any histological difference. This only confirms the observations of previous observers as to the absence of histological changes. The mere fact that castration is followed by the clearing up of the main symptoms of the disease proves nothing. In some cases the immediate calcium loss has, after castration, been greater than before. It is probable that castration acts merely by stopping another pregnancy (which can be as easily done by tying the tubes); this of course is a theoretical gain as it stops the monthly drain of calcium by stopping menstruation. Against this has to be placed the premature menopause with its mental influence on bodily health. We do not remove the ovaries themselves if sterilization is needed. We tie the tubes, and we would have no hesitation in allowing a further pregnancy, if it was so desired, with a planned Cæsarean section even in cases of severe deformity, and in assuring the woman that the disease would not again arise under proper treatment. We have put this opinion into practice without any ill effects following.

One of the latest theories as to ætiology is that of McCrudden [13]:—

"Just as the subcutaneous fatty tissue acts as a store of fat and the liver glycogen as a store of carbohydrate, so the skeleton acts as a store of calcium salts, to be called on in time of need. During the later months of pregnancy and during lactation, the need for calcium salts is great, greater than the intake in the food, and it becomes necessary to drain upon the calcium supply in the bones. The result is that the new bony tissue laid down to replace old bone as it disappears is poorer in lime salts than the normal. Ordinarily the quantitative change in the composition of the bones is not enough to produce symptoms. At the end of gestation and lactation, when the extra need for calcium has ceased, normal bone is again laid down."

Hanau [9] has brought forward some proof of this being the case and of there being a true physiological osteomalacia. But this theory only covers part of the

truth. Were it as simple as this, one would only have to supply the calcium deficiency and all would be well.

There are two points which may be brought forward as controverting this simple view of the disease. *The first* is that in practical treatment it is not enough to supply only a sufficient amount of calcium. For example, in one of our cases studied, although it was possible by the administration of calcium with a vegetable oil to increase the blood-calcium, yet this did not suffice to turn the negative balance into a positive one, and the drain went on just as before.

The second is that the theory fails to take into account the fact that a great many cases begin about puberty, when, although there is a calcium demand, it is not nearly so great as during pregnancy and lactation. And there are a few cases which appear to start after the menopause.

It will be recalled that three factors are believed to act in the case of rickets, a disease closely allied to osteomalacia; in fact von Recklinghausen [20] states that they cannot be morphologically distinguished when the histological appearances of the affected bones are alone considered. This statement, however, must be accepted with reserve, as it certainly does not apply to the areas of growth at the ends of the long bones.

These three factors are: (1) A deficiency diet; (2) lack of sunshine; (3) lack of movement.

To these three may possibly be added: (4) A toxic influence due to some change connected with the ingestion of certain forms of protein, as has been noted in experimental work by Mellanby [16]. Reduced to another form this implies that one must have for health:—

(a) A sufficient supply of salts, and suitable protein and carbohydrate: (b) a sufficiency of the necessary vitamins; (c) an activator for calcium metabolism, and this is bound up with the question of sunlight and movement.

Let us consider first the food which these patients get.

What is the ordinary food of the people? In their homes they live on a very meagre diet of cereals with a limited quantity of vegetables, as a rule with no meat, milk, or eggs, and but little animal fat, the oil used for cooking being mainly vegetable. The principal article of diet is millet, which is eaten at least once a day in the form of a thin gruel made of the whole seeds, which have been partially hulled and boiled in water; and to this water are sometimes added a few small pieces of onion or garlic. A small quantity of salted vegetable will be eaten with this gruel as a relish, a variety of turnip or radish, or bean leaves, which have been preserved in brine. About 15 to 20 gm. will be consumed at a meal. Millet may also be prepared by dry steaming, and it produces a dish somewhat like steamed rice. But in the majority of instances it is consumed as gruel, and during the first month after labour this millet gruel in small quantities (not over 4 oz. of millet a day) may be the sole food.

Wheat-flour products are eaten once a day by those who can afford the luxury. Steamed bread is made of wheat flour, or the dough is rolled out thin and is baked in large, flat, unleavened cakes. A third favourite way of eating wheat flour is in the form of noodles, and usually to the noodles a proportion (usually about 25 per cent.) of flour made from green soy beans is added. This wheat flour varies from a white modern-milled flour to a whole-milled flour. Occasionally oatmeal flour is used, made from ground oatmeal which has been partially roasted, but it does not form a main element in the food.

There are but few vegetables. A form of cabbage, marketed in the west as Chinese celery cabbage, is eaten in large quantities throughout North China, but in the regions affected by osteomalacia, which are mostly, as already stated, on a high plateau, the season is short and the cultivation far from extensive. Thus the supply is limited, and the price high; as a consequence poor people and women get

little of it. It is eaten boiled or fried, and the valuable green outer leaves are stripped off and little eaten, whilst the bleached inner leaves form the part which is most esteemed. Onions and garlic are eaten, and salt vegetables prepared from the leaves of the soy bean, the leaves and stalk of the mustard plant, and a gourd which grows well on the Shansi highlands. Small quantities of bean curd are also eaten by those who can afford it. For cooking the fried portion of the diet, sesame oil is mainly used and a small amount of hemp oil.

In order to make matters certain on this point, one of us (Miles) on two occasions spent a month in Pingtingchow working at this subject. On the first occasion (in 1923), although reliable blood estimations were obtained from ten patients, it was found to be impossible to be sure of the accuracy of the work on the urine, faeces, and food. So a second trip was made in 1924, and on this occasion one of us (Dr. Miles) was accompanied by a skilled chemist, Mr. C. T. Feng, of the Union Medical College, Peking, who is also an able Chinese administrator. We had previously learned the quality and quantity of the food which these patients were getting. Four cases, which were active, were chosen for the investigation and admitted to the hospital free of charge. And here the first difficulty was met. These women knew that they were in for investigation and at our expense, and that we could cure them by medicine, and they wanted better food than they got at home. Finally, a satisfactory compromise was attained, and the diet which they received, although a little better than the normal one, was still sufficiently near to make the test a real one. The patients were allowed as much as they liked to eat, and this was measured for a day or two. Then measured quantities were served out to them, so that they would consume all that was given, and the urine and faeces were carefully collected. This went on for fourteen days, and then, after a few days' rest, they were put under treatment, continuing the same diet for another fourteen days, whilst under the influence of the treatment. The results of the treatment and analyses are given below (see p. 58).

First, let us take the results of the first year's work. It was at once evident that there was a marked deviation from the normal calcium content of the serum, this being in some cases less than half the quantity considered as normal, and in the highest determination being only about three-quarters of the normal. The phosphorus in the blood varied greatly, some of the patients showing a marked deficiency while others came within normal limits.

After completion of the blood-analyses on the standard diet, nine of these patients received treatment as follows, while remaining on the same food: Patients Nos. 1 and 4 took cod-liver oil 30 c.c., and calcium lactate 2 grm., per day; patients Nos. 2 and 3 had calcium phosphate .3 grm. per day; Patient No. 5 had calcium lactate 2 grm. per day; Patients Nos. 6 and 8 were placed on a liberal diet, including milk, eggs, and plenty of fresh vegetables; Patients Nos. 9 and 10 took 30 c.c. of cod-liver oil per day. After the patients had received this treatment for twelve days the blood was again analysed for calcium and phosphorus.

The four patients to whom cod-liver oil was given, either alone or with calcium salts added, all showed a net gain in blood-calcium as compared with the results of the first estimation; those receiving calcium salts with no oil also showed an increase in blood-calcium in two cases and practically the same amount in the third case, as in the first estimation; the two patients on liberal diet both showed a decrease in blood-calcium content.

The clinical results were also suggestive in that the four women who were treated with cod-liver oil alone, or combined with calcium lactate, said that they were much improved and that their pains were less, even after such a short treatment. We also observed that these patients moved about the ward and took more interest in life than they formerly did. Patient No. 2, who took only calcium phosphate, was decidedly worse, complained more of pain and could hardly move.

Patients Nos. 6 and 8, on liberal diet, were both clinically worse than at the beginning of the experiment. As these women were both suffering from an early acute attack of the disease, the result is not strange, and in addition Patient No. 8 had spontaneous abortion of a three-months fœtus and did not take her diet well after that occurred.

As has been already said, the first year's work was not entirely satisfactory and so the second year's work was undertaken.

Ten patients were found to be too many to manage satisfactorily, and so four patients were chosen. Full details of these patients are given in Miles's paper [17] and the results are summarized only. They were all marked cases of osteomalacia with deformity.

These four patients were put on the following diet, which, as has already been said, approximates closely to their normal home diet:—

Average Daily Ration.

<i>Breakfast.</i>	183 gm. of wheat flour in the form of steamed bread.	
	50 gm. of millet in gruel.	
	10 gm. of salt turnip.	
<i>Dinner.</i>	150 gm. of wheat flour made into a thick dough, rolled thin and baked.	
	170 gm. of Chinese celery cabbage	} boiled together.
	8 gm. of garlic	
<i>Supper.</i>	100 gm. of wheat flour.	
	30 gm. of green soy bean flour rolled into noodles and boiled.	
	40 gm. of mustard leaves and stalks	} boiled together.
	8 gm. of garlic	

Numerous samples of these foods were selected and carefully analysed.

The amounts consumed were carefully measured and all the patients appeared to be getting a little less of the calcium in their diet than the minimum estimated as necessary for the individual by Shohl and Sato [22], weight of course being borne in mind.

The urine and fæces were also analysed, and of the four patients three of them presented a negative calcium balance whilst the fourth presented a slight positive balance. But about three months before, the latter patient had had a short course of cod-liver oil.

The blood-calcium was estimated in each case and they all presented figures between 5.8 and 7.5 mgm. of calcium per 100 c.c. of serum. Is the negative calcium balance merely the expression of the calcium deficiency in the diet, or is it that the diet lacks some element which acts as an activator in the retention of calcium?

It will be noted that Patient No. 3 had a positive calcium balance. She was an old patient of the hospital and had received two short courses of treatment with cod-liver oil and calcium phosphate, the last of which was in the autumn of 1923, about four or five months before this experiment was begun. She was so much improved that she asked to come into the hospital on the metabolism squad. Physically, she was the most active of the four patients. Tso [23] has observed in rickets that, even after a short course of treatment by cod-liver oil, the X-ray shows bone development to go on for as much as two or more months after cod-liver oil has been discontinued. This probably accounts for the behaviour of this patient in regard to calcium.

Having thus determined the actual condition of affairs, the patients were put on treatment as follows:—

No. 1.	Cod-liver oil 8 c.c. three times a day.	Same diet.
" 2.	Cod-liver oil 8 c.c. three times a day and 1 gm. calcium lactate.	" "
" 3.	Cod-liver oil 8 c.c. three times a day and 1 gm. calcium phosphate.	" "
" 4.	Olive oil 8 c.c. three times a day and 1 gm. calcium lactate.	" "

The same estimation and analysis were made as during the first period of experimentation. The result was that the negative calcium balance was changed into

a positive one except in the last case (No. 4), whilst the blood-calcium increased to a figure between 8.2 and 9.6 mgm. of calcium per 100 c.c. of serum.

For the sake of clearness the cases will be taken up in order, and the results of the two experiments will be compared.

Patient No. 1.—On a diet identical with that used in experiment 1, which was deficient in calcium, but with the addition of cod-liver oil, her calcium balance changed from a negative to a positive one and her blood-calcium from 7.503 mgm., to 8.62 mgm. per 100 c.c. serum. The weekly calcium output in the urine was very high in both experiments, but in the second the output in the urine and feces was less than in the first experiment, which accounts for the change in the balance and for the increase of calcium in the blood. The change in the phosphorus balance was very slight and there was no change in the blood-phosphorus. It would seem from this experiment that there must be in the cod-liver oil some element which in osteomalacia will materially influence the retention of calcium in the system even in the presence of a deficiency in the food. Clinically, she was greatly improved. On admission she would not leave the bed and sat with her head supported on a Chinese pillow, about which she held her arms; but at the end of the second experiment she was voluntarily leaving the bed for a chair, and would walk the length of the ward, though it was slow work.

Patient No. 2.—With the standard diet and cod-liver oil and calcium lactate added, the calcium balance changed from a slightly negative to a marked positive. The calcium in the urine increased in the second experiment, but in the feces there was a decrease as compared with the first experiment. The blood-calcium changed from 6.395 mgm. to 8.97 mgm. The phosphorus balance was more strongly positive than in the first experiment and the blood-phosphorus increased from 1.24 mgm. to 2.10 mgm. She was clinically much improved.

Patient No. 3.—Calcium phosphate and cod-liver oil added to the diet made even more marked changes. The calcium balance changed from plus 0.4402 gm. to plus 3.2870 gm., the phosphorus from plus 2.4453 gm. to plus 6.1040 gm., and the blood-calcium and phosphorus were so profoundly affected. As the calcium phosphate contained more calcium in the molecule than the lactate, she obtained a correspondingly greater amount of calcium, besides, it contains the phosphorus. In this case again, in the second experiment we found that the calcium in the urine increased and in the feces decreased. In these two cases we see a magnification of the effect of some element in the cod-liver oil observed in Case 1. In both cases we now have no deficiency of calcium in the diet, and the activator enables the patients to utilize and retain larger amounts of calcium than in the first case. The retention of the calcium calls for a corresponding retention of phosphorus, which we find in the increase in the balance ratio and also in the blood.

Patient No. 4.—This patient was given, in addition to the regular diet, olive oil 8 c.c. and 1 gm. of calcium lactate per day. The olive oil was given because the other patients were getting oil, and we did not want this patient to feel that she was treated differently from the others; also, from numerous other experiments, we felt that over so short a period the olive oil would probably produce no effect on the calcium metabolism. We found that her calcium balance remained negative, though to a less degree in the second experiment than in the first; the calcium excretion in the urine remained almost stationary with a large increase in the calcium excreted in the feces, a reversal of the condition in the others. The blood-calcium was increased almost in proportion to the others. This must mean that the calcium is absorbed from the intestine and goes to increase the blood-calcium, but unless utilized in the tissues it is returned to the digestive tract and is excreted. The change in the balance of calcium and the increase in blood-calcium are probably due to temporary storage of calcium in the blood and not to metabolized calcium. The blood-phosphorus remained practically stationary with a small change in the total phosphorus balance. Clinically, she was unimproved and complained that while the other women felt like moving a little, she did not.

Taken together, we see that in these patients with osteomalacia there was a deficiency of calcium in their diet. This may have an effect in producing the disease, but there is another deficiency in their diet which is of greater importance than the calcium, namely, the vitamin content, whether "A" or "X," which is found in cod-liver oil and other animal fats. Increasing the calcium in the diet of Patient No. 4 did not clinically improve her condition and did not produce an absolute positive balance. Yet in the case of Patient No. 1, without an increase in the calcium and only the addition of the cod-liver oil, she improved clinically and also retained and

utilized calcium. In the cases of Patients 2 and 3, with the two deficiencies in the diet made up by the addition of calcium salts and the cod-liver oil, the changes were correspondingly marked.

Three facts at once became clear:—

(1) These women are living on the edge of a calcium-starvation diet, and probably, under the circumstances prevailing in their own homes, are at times actually calcium-starved.

(2) Although the administration of vegetable oil with the diet in one case led to a rise in the blood-calcium, it did not appear from the figures to be more than a piling up of calcium in the blood to have it promptly cast off again, for the negative balance was not transformed into a positive balance.

(3) That the addition of an activator in the form of cod-liver oil at once led not merely to a rise in the blood-calcium but to the transformation of a negative balance into a positive one. This showed that the calcium was not merely being carried into the body, but that the body was using the same for anabolic processes.

What is the process which is going on which, in the absence of a sufficient activator, leads to the calcium loss and the appearance of symptoms?

B. E. Read, of Peking, has urged on us the view that for some reason or other, largely due to the character of the food, the acid-base equilibrium of the body is upset. These diets produce an acid ash, and this leads to the washing out, so to speak, of the body-calcium; and instead of the calcium of the food being absorbed, the acids formed by this food combine with the calcium to form salts and are thus excreted. The final result is a calcium starvation plus a calcium drain, and this occurring in pregnancy, proves too much for the stability of the body-supply, and leads to the bones being called on in an abnormal manner to supply the general deficiency. But when we come to consider this theory we find ourselves confronted with difficulties. E. W. H. Cruickshank, of Peking, in a personal communication, says:—

"It is a very difficult thing to alter the acid-base balance, so difficult, in fact, that in acute tetany with alkalosis due to a marked CO_2 deficit, the alteration in pH may vary by only 0.15 to 0.20, and this variation is quite within normal physiological limits.

The idea of the acid-ash denoting an acid diet—acid enough to upset the buffer action of such a physico-chemical system as blood, needs to be viewed with caution. Our own work does not tend to bear out this theory. In the first series of Miles's Shansi experiments, the carbon-dioxide combining power of the plasma does not seem to indicate a change in the acid-base equilibrium and the results do not indicate an acidosis.

The question of what leads to a washing out of body-calcium is a very complicated one. It depends upon the form in which calcium is most easily excreted by the bowel and kidneys. Is that form a salt or an ion? There is a distinct relationship in an *in vitro* mixture between $\text{Ca}^{++}\text{HCO}_3^-$ and H^+ . If the formula of Rona Takahashi holds in blood, then any increase in H-ion concentration will favour the increase in Ca ions, and Ca ions can readily get out by the kidney, and this is favoured by lack of parathyroid or protein poisons. What part, if any, does fat-soluble vitamin play in controlling the amount of Ca ions, which may be liberated from or taken up by protein or salts? H-ion concentration plays a part, we are sure, in controlling the union of Hb with Na and with O_2 . But with a process of metabolism, to say that, is only one step nearer home, in that it gives an inkling to the part a vitamin may play. And, as Mellanby suggests, high protein foods may be playing a part antagonistic to the normal anabolic processes.

As far as the work we have done goes, it tends to show that parathyroid lack alters the balance between ionic and colloidal calcium. Most of the Ca goes to the ionic form and with that change there is a change in pH to acid side of neutrality, and here you are back to the experimental *in vivo* proof of the *in vitro* relationship between H ion, Ca ion, and HCO ion. It may be that parathyroid extract is the correct key for the treatment of the tetany of Ca breakdown; whilst a supply of fat-soluble vitamin is the key for the treatment of the osteomalacic disaster; if so, the pathological change in protein chemistry is different, though not necessarily essentially so."

These ideas naturally lead on to the consideration of two questions, neither of which we are able to answer definitely but both of which deserve careful consideration.

Is the settling up of a process of this kind, if it really occurs, due to the protein of this ill-balanced diet? Mellanby [16] has brought forward evidence to prove that cereals in a diet which is deficient in fat-soluble vitamin tend to produce defective bone calcification, and apparently it is the protein element which is the responsible factor. Moreover, a diet of this kind in puppies tends to produce muscle changes and weakness of locomotion. In the osteomalacia cases in goats, which I shall mention later on, there was undoubtedly overfeeding with beans, which contain a high protein factor.

Is there a definite shortage of fat-soluble vitamin in these diets? There is undoubtedly in the Shansi cases (especially at times) a very poor supply of green vegetables, which is the main source of their fat-soluble vitamin in the absence of milk, butter, eggs and animal fat. That there is not a gross shortage is pretty certain, as, in spite of careful inquiries, we have not been able to trace any cases of xerophthalmia. But the rapid improvement which takes place on the administration of cod-liver oil proves that there must be a shortage in this direction, granting that the activator is really fat-soluble vitamin and not a special substance found *par excellence* in cod-liver oil.

How does this activator act?

There is no doubt that fats play a large part in the transference of mineral salts from the bowel to the blood, but as we have already said vegetable fats will do this to a sufficient extent and yet fail to meet the situation. Vines and Cruickshank have brought forward evidence to prove that the parathyroid gland has a profound influence on calcium metabolism in the body, and Erdheim [6] has reported pathological changes in this gland in five out of six cases of osteomalacia. But it is not clear that these changes may not be the effect of the osteomalacia rather than its cause; and although our experience so far with the administration of the commercial gland products has been small, we have seen no results which would justify our saying that the administration of this gland plus cod-liver oil is more efficacious in the cure of the disease than cod-liver oil alone.

We think there is little doubt that infections added to the disease aid its progress. In our sole case of osteomalacia in the male, the process apparently started at the age of 12 as a sequela to an attack of measles, combined with close confinement to a dark room for many weeks. Afterwards there was an intensification of this treatment, i.e., confinement to a dark room and bed, due to the pain accompanying the onset of the disease and sepsis arising from bed-sores. But we have no evidence which would justify our placing this disease in the category of microbic infection. The work of Montpurgo [18] and others in Italy, and Moussu and Charrin [19] in France, as to a definite infective agent which is transmittable, needs confirmation; and any theory as to infection is doubtful in view of the other facts which we know about the disease.

What influence has the environment factor on the genesis and progress of the disease?

In Shansi, where the disease is most prevalent, the "kangs" are heated by coal, the fumes to a considerable extent escaping into the room; this means pollution of the atmosphere. Cases of carbon monoxide poisoning are not by any means unknown. Next, there is the evil influence of foot-binding, in retarding health and preventing movement. Following this, you have the fact that these cases are mostly occurring on the uplands, where the winters are severe and confinement to the house, especially in the case of women, is almost complete. And in considering the movement factor one has to note the significant fact that osteomalacia is a disease of the middle classes. The poor, who are compelled to get out and work in the fields,

rarely acquire it. The rich, who have plenty of nourishing food with meat and fat, are seldom affected. The victims are what we may almost call the "genteel" members of society: too proud to work, even if their families would allow it, not rich enough to afford the meat and fat which would correct the deficiency in other ways.

As was mentioned earlier on in this paper, after the close of Miles's Shansi experiments there came into our hands the paper on two cases of infantile tetany which had been received into the medical wards of the Peking Union Medical College. Looking at the blood-estimations it was clear that we had here a parallel case to our Shansi work. If this was correct, these girls ought to have signs of commencing osteomalacia. They were fetched back to hospital and re-examined, with the result that both of them showed signs of the disease.

It is interesting to note a few facts about this orphanage. It contains about fifty girls and seventy boys, from 10 to 18 years of age. No boys presented signs of infantile tetany, but of the girls who remained mostly indoors during the winter months there were no fewer than ten who complained of "ma mu"¹ and some had facial phenomena showing that they had either had tetany or were on the verge of it.

The diet at this orphanage is as follows:—

<i>Breakfast.</i>	Millet gruel, corn bread, salted carrot.
<i>Lunch.</i>	Wheat bread, vegetable.
<i>Supper.</i>	Corn bread, vegetable, salted carrot.
	Meat once a month.

Vegetables are as follows, according to the season:—

Cabbage	October to January.
Bean sprouts	February.
Onions and bean curd	March.
Spinach	April.
Egg plant and gourd	May to August.
Pearl melon	August to September.

It also has to be borne in mind that children have their likes and dislikes and will pick and choose their food, thus possibly helping on a diet-deficiency trouble.

Finally, there is the question of sunlight. Some of the small courts get little of this, and if the woman stays on the "kang" (and she tends to stay there more and more after the pains of the disease come on) she is exposed to very little sunshine. So that, taken altogether, the environment certainly aids the disease.

As bearing on the general question of the ætiology of this disease, it must also be borne in mind, *first* that it is extremely rare where an adequate diet is in vogue amongst any people, and *second* that an improvement in diet will lead to its speedy disappearance from places where it has formerly been rife.

Whether there is any racial tendency to the disease is doubtful. We have quoted one case in which an Australian woman living under Chinese conditions contracted the disease. Irving has also personally pointed out to one of us that only three cases of the disease have been diagnosed at the Lying-in-Hospital of the Harvard Medical School during the past ninety years. Strangely, all these cases occurred in Syrians, but it is known that in the case which was most carefully studied the diet was extremely poor, almost a starvation diet.

Our own conclusions as to the ætiology of the disease are as follows:

(1) Osteomalacia is a diet-deficiency disease and is to be placed in the same category as rickets.

(2) The deficiency is principally in the fat-soluble vitamin or calcium-activator

¹ The Chinese term for disturbances of sensation, evidently connected with tetany.

content of the diet, though there may be and probably is an actual or at least a proportional calcium deficiency at the same time.

(3) The result of this deficiency diet is that a disturbance of the normal calcium metabolism is set up and it is possible that in addition to the diet deficiency there arises a toxic process through the agency of an unbalanced protein consumption (cereal).

(4) Intercurrent septic infections will aid in the progress of the disease, but probably only by lowering the general vitality, and there is no evidence of such infection in the majority of cases.

(5) The evidence suggesting diseased processes in the ovaries and parathyroid is insufficient to place the blame for the disease on either organ.

(6) Osteomalacia may be prevented by improving the diet of the population in the direction of a fuller supply of fat-soluble vitamin and salts. It may be cured by providing a sufficiency of food plus a calcium activator such as cod-liver oil and sunlight.

SECTION V.—THE SEQUELÆ OF OSTEOMALACIA.

The sequelæ of osteomalacia may be divided into those which affect labour, and those affecting marital relations.

The sequelæ affecting labour have long been known. In fact, wherever there is a hospital in an osteomalacic region, the measure of the severity of the disease is indicated by the number of Cæsarean sections. The mortality rate for the mother has been very heavy owing to the fact that these cases come in after one, two, three, four, five or six days' labour, often with the vagina seriously damaged and infected.

The native midwives have no idea of how to deal with these patients. They take either a meat hook and conceal it in their hand, binding it round the wrist, or they take a pair of charcoal tongs and bend over one of the points to form a rough hook. The hand with the hook or the modified tongs is then introduced into the vagina, and anything that can be caught and torn away suffers this fate. Naturally they produce the most horrible injuries to bladder and bowel and vagina. I know of one case where the uterine wall was torn and part of the maternal bowel dragged down and torn out; and I know of more than one woman who has entered hospital with the uterus ruptured, whilst vesico-vaginal fistulæ are common in the few cases that survive this sort of manipulation.

What is the best treatment for cases with contraction due to osteomalacia? If the contraction is at all severe, provided the patient is seen before labour or early in labour, Cæsarean section, with tying of the Fallopian tubes to prevent a further pregnancy, is the method of choice unless the patient wishes to have another child. Unless the case is a really severe one, it is quite safe for her under appropriate treatment to risk another pregnancy without fear of the disease becoming again active.

In the very neglected cases there is no doubt that the safest method is a Cæsarean hysterectomy.

Should the contraction be a moderate one and the child be dead, a craniotomy should be done, and a Cæsarean section planned for the next occasion.

As to the anæsthetic to be used, chloroform is very dangerous in these neglected cases. Ether should be used. Straight, open ether with a preliminary injection of atropine $\frac{1}{16}$ gr. (no morphine) is the best form of exhibiting the anæsthetic.

With regard to the question of marital relations, in quite a number of cases the tubera ischii become almost apposed, so that one cannot get a finger between them. Then the inclination of the pubes becomes altered, and the descending rami of the pubes and ascending rami of the ischia also move together so that coition becomes impossible. Where there are several wives this is not a matter of such importance,

but when the patient is the only wife the result may be a most miserable home. The following case may serve as an illustration.

Case No. 3062.—Lo Yin Shih, aged 26, from Shou Yang, Shansi, came into the Peking Medical College Hospital on September 5, 1922. Patient was married at 16 years of age. When she was 20 years old her first child was born naturally; it died when 3 years old. A year later her second child was born naturally, and it died when 3 years old. In the case of both these children the cause of death was unknown.

The third child was born when she was 23 years old and died the day after birth, labour being very difficult. After that time marital relations became more and more difficult till they became impossible. Her husband sold her off to a second man, who finding coition impossible was preparing to sell her to a third. She threatened to commit suicide if nothing could be done for her. Dr. F. J. Wampler, of Pingtingchow, kindly sent her in to one of us (Maxwell).

Patient's height was 4 ft. 10 in.; weight, 92 lb. Pelvic measurements as follows: spines, 21 cm.; crests, 23.25; trochanters, 22.5 cm.; ext. conjugate, 17.5 cm.; inter-ischial, 2 cm.

A finger could be got into the vagina with difficulty before and behind the tubera ischii. Coition was manifestly impossible. The Wassermann reaction was negative and otherwise the patient was in good health.

Two courses were open—(1) to excise the descending ramus of the pubes, ascending ramus of the ischium, and part of the ischial tuberosity, on one or both sides; (2) to excise the coccyx and try to make a vaginal opening posterior to the tubera ischii. In the former operation there was the possibility of disturbing the balance of the pelvis and weakening the adductor muscles of the thigh. In the latter operation, although one might get a larger vaginal opening posterior to the tubera ischii, yet the direction of the vaginal canal would be displaced very seriously. It was decided to try the first course.

On September 14, 1922, the patient was anaesthetized with ether and placed in a lithotomy position.

The vulval aperture and the anus were both closed by fine silk. An incision extending from the pubes to below the ischial tuberosity, and about four inches long, was made, about two inches outside the vulval aperture on the left side. The tissues were divided down to the bone and a subperiosteal resection of the descending ramus of the pubes and the ascending ramus of the ischium and half of the ischial tuberosity was made, bleeding points being carefully secured as cut. There was comparatively little bleeding. The vagina was not pierced. Care was taken not to interfere with muscular attachments more than was absolutely necessary and the wound was sewn up with fine silk, the tissues being brought together in their various layers so as to leave no dead space. After sealing the area round the wound with collodion, on gauze, the vagina and anus were again opened.

The patient made a normal convalescence; on discharge two fingers could be easily inserted into the vagina, and she had no difficulty in walking. She was seen about three months later. Locomotion was perfect and it had been possible to resume marital relations without any pain or inconvenience.

The subsequent history of the case is very interesting. Within two months she became pregnant. Before operating on her, we had explained the situation very clearly and she knew that a Cæsarean section was necessary. But coition had been so easy that her relatives, especially on the mother's side, refused to believe that labour could not be conducted in the ordinary way. In spite of my pleadings and protests they refused to send her to hospital, and it was only after she had been three days in labour that they brought her begging for assistance. She was taken to Pingtingchow Hospital under a heavy dose of morphia, the fetal heart just audible and the uterus already in a state of tonic contraction. Dr. Wampler performed a Cæsarean section. The fœtus, a full-term male, was dead; and the mother was desperately ill for forty-eight hours, but recovered after a smart attack of sepsis. The tubes were tied, but the uterus was not removed.

As to the condition of the parts, the vagina admitted two fingers easily. A hard, irregular crescentic band had formed with the concavity inwards in the situation where the bone had been removed. Locomotion was perfect and there was no instability of the pelvic girdle.

As far, then, as the final result was concerned, the operation had been a complete success and, but for the stupidity of the relatives, there would have been a living and perfect child. In the early months of pregnancy she had been treated with cod-liver oil and calcium lactate, and there were no signs of recurring osteomalacia.

SECTION VI.—TREATMENT OF OSTEOMALACIA.

When we come to the question of treatment we are at once confronted with two difficulties. The first is the lack of an accurate knowledge of the process by means of which calcium is laid down and taken away from the bones; if we were acquainted with this, it might guide us to a rational treatment of the disease. At present one is like a person groping in the dark and seeking empirically for some key which will fit the lock. We know that when given cod-liver oil and good food and when exposed to sunlight, the majority of the patients will slowly recover. We also know that by giving cod-liver oil, and possibly calcium and phosphorus in addition, we can quickly remove the symptoms of tetany accompanying the disease and also remove the worst of the pain. But even here we find that in what is apparently the same degree of severity the recovery rate is very variable in its rapidity and one sometimes seems to succeed up to a certain point, then our efforts apparently fail to press the cure to a full recovery for some time.

One of the complicating features—and this constitutes our second difficulty—is that the great mass of the population of North China are living, so to speak, on the edge of starvation. We can safely say that the food ration of the majority of the people is deficient both as regards quantity and quality, especially as regards the latter. Were we to be able to raise the standard of living in respect of food and exercise, no doubt osteomalacia would become an insignificant problem. But what are we to do, when in one of the best-ruled provinces of China, with strict laws against foot-binding, the people refuse to give up this senseless and cruel custom? How can we advise an improvement in the standard of living when no one, in view of the political unrest, dare invest capital in what would inevitably raise this standard? We want flocks and herds, milk and meat, and there is no reason why, given peace and justice and security of tenure, we should not be able to obtain these necessities. The main treatment for osteomalacia is to secure such security of life and property that a higher standard of feeding and more healthy standard of living may prevent its development.

And in discussing this matter we cannot leave out the question of drought and famine. The whole of North China has been de-forested, moreover Shansi lies on the edge of the dry belt, and flood and drought follow one another with painful frequency. These questions are of national importance and are problems which can only be dealt with by the nation as a whole. If good roads were quickly constructed so as to make the interchange of commodities more rapid and easy, we might get a sufficient improvement in living standards without the larger questions being solved, and probably the best way to tackle the matter would be by road building.

As to the medicinal treatment of the individual case, we have found that cod-liver oil in sufficient doses—and by this we mean at least one and a half ounces daily, combined with calcium lactate, 15 gr. a day—is the most satisfactory method. Calcium phosphate does not seem to be as efficient. We have tried treatment with parathyroid gland as an additional help, but, as we have already said, without being convinced that it really helped in the progress of the case. Fresh air and sunlight should also be urged and the patient made to go and sit out in the sun.

And of course the general diet standard must be improved as far as possible, so that the family understand the importance of the balancing of the various articles of food which they may be able to obtain.

In case of future pregnancies the patient should be placed on cod-liver oil and calcium at the beginning of the pregnancy, and this treatment kept up through pregnancy and lactation, care being also taken that the patient gets an improved and adequate diet.

GENERAL CONCLUSIONS.

- (1) China is at present one of the world-centres for the study of osteomalacia.

(2) In the special area involved the incidence is from 1 to 3 per cent. of child-bearing women.

(3) While its effects are more often shown in pregnancy and the puerperium, it often begins about the time of puberty.

(4) The disease is one due to a deficiency diet, the principal lack being in a shortage of mineral content and the activator for calcium metabolism.

(5) Lack of sunlight and movement are contributing factors in its production.

(6) The bones of full-term fetuses from mothers with osteomalacia do not show the signs of rickets but of osteoporosis. There is, however, a little evidence to suggest that these infants develop rickets more easily than the normal child.

(7) The sequelæ of the disease are twofold—firstly, deformity of the pelvis, which may be counteracted by a planned Cæsarean section; secondly, interference with marital relations, which may be treated by a new operation in which portions of the pubes and ischium are resected.

(8) The ovaries have nothing to do with the disease and it is not necessary either to remove them or even to tie the tubes, provided that the woman is treated with cod-liver oil and calcium and this treatment is kept up during any future pregnancy.

(9) By improving the diet and supplying cod-liver oil it should be possible in due time to stamp out the disease. Prevention is the principal thing and to this end a settled government and improved means of communication are essential.

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Discussion.—Dr. H. RUSSELL ANDREWS (President) said that his (the President's) own knowledge of osteomalacia before reading Dr. Maxwell's paper had been very slight indeed. He had seen three cases in India in 1897, but otherwise his knowledge was entirely derived from text-books and therefore, in this case, incorrect. He congratulated Dr. Maxwell on a splendid piece of research which had entailed an immense amount of work, and thanked him in the name of the Section, also personally, for his extremely interesting communication.

Mr. WYATT asked Dr. Maxwell whether in cases of osteomalacia occurring at puberty the disease progressed at all rapidly if pregnancy did not occur.

Dr. GRACE STAPLETON said that the paper had been of great interest especially to those Members who had come from India. There the disease showed the same marked tendency

as in China, to occur in certain districts and to be absent from others. It was common in Delhi, some cities of the Punjab and the United Provinces and Bombay; but practitioners from Madras said that they rarely saw a case, and it was uncommon in Bengal. As regards symptoms at the onset, she would say that the patients in the neighbourhood of Delhi complained fairly consistently of pain in the lower part of the back and the hips, and only later did it spread to the ribs, where it was felt more particularly on sneezing and coughing. She (Dr. Stapleton) asked Dr. Maxwell if he did not think that the housing conditions and customs of the women regarding exercise were an important factor in the aetiology of the disease, as his slides showed that the houses were close and dark and the people had often to retire to caverns in the ground for considerable periods. In Delhi she had found, in a series of cases which she collected for the late Dr. H. S. Hutchison of Glasgow, that the disease occurred largely among women who observed the custom of purdah. Was late rickets found in the same districts in China as osteomalacia, as both diseases occurred in the same kind of patient in Delhi? Did intermediate cases also occur—an event that rendered it difficult to know which diagnosis to give?

Professor MCILROY said that she would like to know whether sunlight or ultra-violet ray treatment had been applied directly to these patients. She could not understand Dr. Maxwell's statement that the genital organs were not concerned in the aetiology of the disease. Surely a condition almost confined to the female sex, occurring at puberty, aggravated by menstrual function and pregnancy, had some relationship to these productive functions, which were so closely bound up with the calcium metabolism. She had had experience of only one case of osteomalacia, that of a woman in Belgrade who had been a prisoner in Germany and who had lived on brown bread partly made from sawdust.

Section of Obstetrics and Gynæcology.

President—Dr. H. RUSSELL ANDREWS.

Uterus and Vagina excised for Epithelioma of the Vagina.

By BECKWITH WHITEHOUSE, M.S.

THE specimen was removed from a 7-para, aged 49, by the abdomino-perineal route and is shown in order to demonstrate the advantage of spinal anæsthesia in carrying out a prolonged dissection in the pelvis.

Symptoms had been present for ten months before the operation, the most prominent being irregular hæmorrhages, a foul persistent discharge and irritability of the bladder. On examination a large indurated ulcer was exposed on the posterior vaginal wall at the junction of the upper and middle third, and extending into the posterior fornix. It bled freely on contact, and was associated with a septic vulvo-vaginitis. Combined recto-vagino-abdominal examination under anæsthesia showed that the ulcer was not adherent to the bowel. The patient was a spare woman and no lymphatic extension could be detected.

She was in hospital for a fortnight in order that the septic complications might be eliminated.

The radical operation was performed on November 14, 1924, under spinal anæsthesia with tropa-cocaine, reinforced at the end of sixty minutes by gas and oxygen. The vaginal dissection was performed first, the vagina being separated from adherent tissues and its orifice closed by a purse-string suture.

No great difficulty was experienced and there was very little hæmorrhage, this being attributable to the effect of the spinal anæsthesia.

After the vagina had been detached as high as the upper third, the patient was placed in the Trendelenburg position and the ordinary extended abdominal hysterectomy with dissection of the ureters was performed. The uterus and vagina were removed through the abdominal incision and the pelvic peritoneum was closed. The perineal wound was lightly packed with gauze. The operation occupied two hours, and when the patient was returned to the ward her pulse-rate was 92. Convalescence was normal.

Dr. Haswell Wilson (Birmingham) reports that the "vaginal tumour is an epithelioma. It shows many mitotic figures and is apparently proliferating rapidly. The histological appearances have a close resemblance to those seen in epithelioma of the cervix uteri."

Sarcoma of the Vagina.

By BECKWITH WHITEHOUSE, M.S.

THE specimen, obtained post mortem from a child, aged 21 months, consists of the complete pelvic organs dissected to show an extensive malignant neoplasm in relation with the anterior vaginal wall.

The child was admitted to the General Hospital, Birmingham, with a history of severe vaginal hæmorrhage four days previously. Following the hæmorrhage a soft fleshy mass had appeared at the vulva and this was protruding for about one inch at the time of admission. The patient was well nourished but exhibited the signs of hæmorrhage.

Under anæsthesia it was found that the vagina was much ballooned and filled with a soft gelatinous grape-like mass. The vesicles were translucent and varied in size from that of a small pea to that of a marble. About a tea-cupful of this tissue was removed with the finger and ovum forceps. It appeared, clinically, that the

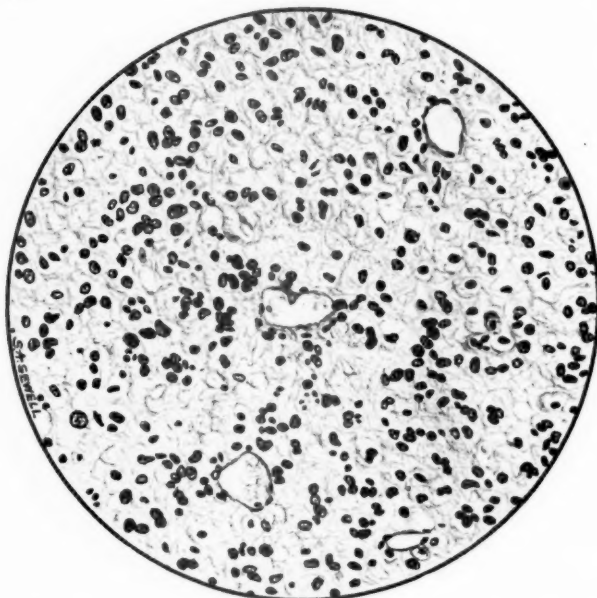
neoplasm arose from the cervix uteri, and that a large metastasis was present on the anterior vaginal wall extending to the vulva and involving the left labium minus.

A diagnosis was made of sarcoma botryoides of the uterus.

In fourteen days hæmorrhage recurred. The vagina was again found ballooned to the original degree and about the same quantity of growth was removed. Three weeks later the tumour reappeared outside the vulva and its extraordinarily rapid development could be watched daily.

The child died about three months after the appearance of the first hæmorrhage in a condition of extreme emaciation and anæmia.

Post mortem it was found that the tumour had no relation to the uterus but that it arose from the anterior vaginal wall. In this respect it agrees with the statistics given by Wilms, who believes that these vaginal sarcomata in children originate in off-shoots of the mesoderm detached during the early growth of the Wolffian duct.



The pathological report states that the present tumour is a mixed-celled myxosarcoma. No muscle fibres, either plain or striated, have been seen in the sections examined, thus excluding relationship with the teratoid group of neoplasms.

Mesenteric Cyst.

By BECKWITH WHITEHOUSE, M.S.

THE specimen appears to be a chylous cyst of the mesentery and not a cystic teratoma as originally thought. It was removed at Hammerwich Cottage Hospital, Cannock Chase, on December 18, 1924, from a 1-para, aged 26.

The patient was referred to me by Dr. Bradford, of Brownhills, to whom I am indebted for the following clinical notes.

She consulted him on December 11, 1924, on account of severe pains in the hypogastric region and frequency of micturition. The pain, she said, "doubled her up," and became more acute and severe when she stooped. Micturition was very frequent both day and night. A similar attack had occurred three months previously, but as the menses were present at the time the patient had disregarded the pain.

On examination, a tense, fixed swelling was palpable, bimanually, in front of the uterus. It could not be displaced. The uterus was completely retroverted and very low in the pelvis. A diagnosis of ovarian cystic teratoma was made, chiefly owing to the consistency of the tumour.

At the operation a cyst was found impacted in the pelvis in front of the uterus and between the layers of the mesentery of the small intestine, which had prolapsed into the pelvic cavity. It was enucleated from the mesentery and proved to be a unilocular cyst with a fibrous wall, containing thick cream-like fluid uniform in consistence.

The patient made a normal recovery and has since remained well.

Pathological Report.—Dr. Haswell Wilson has kindly furnished me with the following report upon the specimen: "The fluid from the mesenteric cyst appears to be chylous in nature. It contains many globules of fat with many degenerated nuclei. The cells present are mostly lymphocytes. No organisms of any kind are found in films or cultures. The wall of the cyst is composed of fibrous tissue, with an imperfect lining of endothelium."

Dr. H. RUSSELL ANDREWS (President) in discussing the first specimen said he thought that separation of the vagina from below, and then abdominal hysterectomy followed by removal of the vagina from above, was the more thorough operation, though removal of both vagina and uterus from below might give very good results. He referred to a patient of his own who was in excellent health eighteen years after the latter operation for carcinoma of the vagina. With regard to the third specimen he said that his own experience of mesenteric cysts was confined to a few which were situated higher up than that shown by Mr. Beckwith Whitehouse. The low position of this cyst, resting on the uterus, made accurate diagnosis impossible.

Retroplacental Hæmatoma from a Case of Toxæmia.

By A. LOUISE MCILROY, M.D.

THE patient, aged 29, 2-para, was sent up by her doctor, complaining of œdema of legs and face for two weeks. There was no headache; vomiting had been present for three months, and pain was experienced in the lower abdomen.

Some dimness of vision was present. Digestion was fairly good, but constipation was complained of. The first pregnancy and labour had been normal but the child only lived one month.

Examination revealed œdema of face, legs and abdominal wall, and the uterus felt about the size of a twenty-weeks pregnancy. Blood-pressure was 130, and the urine contained albumin.

As there was no bed available the patient was sent home, but was admitted to hospital later. On admission: no fetal sounds audible; albumin 1·6 per cent., blood-pressure 210. Labour began on evening of admission; very slight hæmorrhage, uterus very hard and tender and increasing in size. Membranes were artificially ruptured, the cervix being partially dilated. A dead fœtus (not macerated) was expelled an hour later, of about twenty weeks' maturity. A clot of blood the size of a cricket ball was expelled immediately and then the placenta. A large blood-clot was adherent to the placenta, with hæmorrhage into the membranes.

The œdema was more pronounced next day.

Two days later the œdema had disappeared, the urine was increased in quantity and the albumin had become reduced in amount by 0·5 per cent.

Examination of the fundus oculi gave no definite evidence of toxæmia.

A fortnight later the patient was discharged much improved in health, without œdema, headache or eye symptoms; only a trace of albumin remained in the urine.

A Case of Toxæmia in Early Pregnancy with Jaundice, Hyperemesis and Multiple Neuritis. Death three weeks after termination of Pregnancy.

By JOHN S. FAIRBAIRN, M.B., and Z. MENNELL, M.B.

WE have thought this case worth putting on record because of the development of a hepatic type of toxæmia early in pregnancy, the temporary improvement following evacuation of the uterus, and the development of widespread paralysis in its final stages. When the patient was seen by us (October 17, 1924) she was extremely ill, having on the previous day returned from Switzerland, where she had been under medical treatment during six weeks for jaundice and vomiting. She was moved into a nursing home and the following outline of her previous history obtained: Age 27: married three years; previously healthy and physically well-developed. She had been playing tennis and leading an active outdoor life up to August 30, when the menstrual period then due did not appear. Within a few days vomiting, together with slight jaundice, came on; the jaundice increased to a moderate degree and then gradually subsided. A letter from the doctor, under whose care she had been, stated that she had been examined under chloroform anaesthesia, a uterine displacement corrected and a pessary inserted. For the progressive weakness of the pulse camphor had been given hypodermically. Morphine had also been administered in order to secure rest and lessen vomiting. On October 17, examination in the nursing home showed extreme emaciation with hollow cheeks, sunken eyes and scaphoid abdomen. There was little colour in the face and a slight but perceptible icteric tinge of the skin and sclerotics. The tongue and lips were moist and clean and there was no fur or sordes present. Temperature was normal.

She complained greatly of pain and oppression in the chest, and extreme weakness. The most striking feature in her case was the rapid, feeble pulse, the rate being 120 to 130. The blood-pressure was 110 (systolic). The uterus could not be recognized on abdominal examination. No diminution in the area of liver dullness was detected. A pessary of the Hodge type was removed and the uterus noted to be soft, enlarged and slightly retroverted. The urine was of a very dark colour and on boiling showed a faint trace of albumin.

The excessive vomiting seemed to be the only explanation of the emaciation, feeble pulse and extreme exhaustion. That the vomiting was a symptom of a hepatic toxæmia was suggested by the jaundice having been marked early in the illness. But the almost complete disappearance of the jaundice with persistence of vomiting did not accord with this view and the possibility of hysterical vomiting was kept in sight. There was a strong suggestion of this suspicion, as her two elder sisters had both suffered from hyperemesis so severe that abortion had been induced in both, and in one of them on three occasions.

As the patient was still suffering from the effects of the journey from Switzerland, it was reasonable to hope that a few days' rest might effect a decided improvement in her cardiac condition. We also wished to know whether it was possible for biochemical investigation of the blood and urine to throw light on the nature of the case, and to have the opportunity of further observation before deciding on the treatment to be adopted.

On the following morning (October 18) she was seen by Dr. de Wesselow, who drew off a few cubic centimetres of blood and took the urine passed since admission, 5 oz. in all, for examination. She was allowed all the fluid she cared to drink and in what form

she fancied, together with egg, bread and butter and jelly; two rectal saline injections with 10 per cent. glucose were also administered. She was put on six-hourly hypodermic injections of morphia (gr. $\frac{1}{4}$). For the most part the matter vomited was small in quantity, 2 to 4 oz. of greenish fluid, but vomiting occurred frequently, up to nine or ten times in the twenty-four hours.

Dr. de Wesselow reported that the urine contained a small amount of protein and much acetone; the percentage of urea was 1.65; no casts were discovered. The blood-urea was 32 mgm. per 100 c.c. These findings did not suggest the presence of any grave renal damage. The plasma chlorides were 0.580 per cent., not as low as might be expected from the continued vomiting. The Van den Bergh direct reaction was negative, the indirect positive; the direct test is for the presence of bile-pigment in the serum, a positive result being indicative of obstructive jaundice; the indirect, obtained after precipitation of the proteins by alcohol, is considered indicative of a toxic jaundice. Although the value of the reaction is as yet unproven, the result in this case corresponds with the claims made by Van den Bergh.

In view of the history of jaundice and the serious condition of the patient, these biochemical findings were taken as confirming the condition to be one of hepatic toxæmia and as indicating the termination of the pregnancy at the earliest favourable moment. Throughout October 18 the pulse remained between 120 and 130, and even moving the patient in bed caused an increase to 140 or 150.

On the 20th her condition, so far from having shown any improvement, had distinctly deteriorated; no urine had been passed since the early afternoon of the 19th, the pulse-rate had been 136, rising to 140 at 6 p.m., so at 7 p.m. an intravenous infusion of a pint of saline and glucose (2½ oz.) with an ounce of brandy was given. The improvement after the infusion was immediate and striking. A few hours later under light ether-anæsthesia the uterus was rapidly dilated and emptied. Fortunately there was little hæmorrhage during this procedure. The fœtus was broken up during removal but the duration of the pregnancy was estimated to be about ten weeks; the gestation sac did not occupy the whole uterine cavity. At the time of the operation the pulse-rate was 160, and remained at that level all the next day and night.

There was little loss at any time after the evacuation of the uterus. As no urine had been voided for thirty-six hours, a catheter was passed on the morning of the 21st and 12 oz. drawn off. A second intravenous infusion, similar to the first, was given on the evening of October 21, and the only rise of temperature at this time was to 100.6° F. at midnight of the 21st to 22nd.

The improvement in the patient's condition after both infusions and the emptying of the uterus was very marked. Though the pulse-rate remained for about thirty-six hours at between 150 and 160, it fell rapidly after that; on the morning of the 23rd it was 100 and by 2 p.m. 90, and the strength of the beat had proportionately improved. The amount of urine passed had steadily increased, and all vomiting had ceased; the patient was taking large quantities of fluid by the mouth and was on an easily digestible diet,—at first bread and butter with jam or honey, then eggs and jellies, and later stewed fruit and potatoes and vegetables with gravy. On the afternoon, however, of October 23 (i.e., the third day after evacuation of the uterus) vomiting began again. On the first two occasions the vomit differed from the previous type in the quantity being considerably greater (12 and 10 oz.); it continued throughout that night and next day, over 70 oz. being measured in the twenty-four hours. The patient's general condition, nevertheless, remained good. This return of vomiting was thought to be due to a too rapid increase in the diet and to the cutting off of the morphine injections. Food and fluid by mouth were stopped and the rectal glucose infusions and hypodermic injections of morphia (gr. $\frac{1}{4}$) resumed. After thirty-six hours albumin water by mouth and then arrowroot and Benger's food were tried, as the rectal infusions were not well retained. Owing

to the difficulty in finding a suitable vein (the two best having been used for the intravenous infusions) no blood was taken for examination. Bandaging the arm with this object, however, elicited Trousseau's sign, tetany of the hand being produced by compression of the brachial nerves. On Dr. de Wesselow's suggestion, ammonium chloride (gr. 15) was added to the rectal infusions, as the very definite Trousseau's sign was taken to indicate the presence of an alkalosis.

During this recrudescence of vomiting the pulse-rate again rose to between 110 and 120, but the strength of the pulse remained good and the condition of the patient not alarming. A week elapsed before the pulse-rate remained below 100, but after the first two days steady progress was made, the emaciation and faint tinge of jaundice disappeared, a fair quantity of urine was passed and the patient slept well; she took increasing quantities of fluid and nourishment, felt better and was in good spirits. Although vomiting continued for a week it diminished in frequency to two or three times, and latterly to once, in twenty-four hours; the amount vomited was only a few ounces, and it was independent of the taking of food. Indeed at the end of October and early days of November, the striking change in her general appearance, the filling out of the face and body, the disappearance of the extreme weakness and depression made all in attendance on the patient feel that her complete recovery was but a matter of time. There was another set-back on November 1 and 2, when the temperature ran up to 101° and remained in the neighbourhood of 100° for about twenty-four hours with a slight cough and rise of the respiration rate to 24-28 per minute. On November 2 she was seen again by Dr. de Wesselow. The urine then contained no acetone but abundant clumps of pus-cells and a motile bacillus. Examination of the chest failed to reveal any physical signs beyond some impairment of resonance and poor air-entry at both bases. On two occasions there was vomiting of bile-green fluid, 6 oz. and 2 oz. being brought up. On November 3 she was much better; temperature normal, pulse-rate 98 to 110, respiration-rate 22; taking food well (milk puddings, eggs, &c.) and sleeping well. But on the following day signs of laryngeal paralysis appeared (loss of voice and coughing after drinking fluids); later there were those of diaphragmatic palsy together with physical signs of collapse at the bases of both lungs. On November 5 foot-drop in both feet was noted, with loss of knee-jerks; massage and movements of the feet were therefore begun. Coughing became so troublesome that all fluids had to be thickened or jellied, and occasional regurgitation through the nose occurred. There was no expectoration. Cyanosis of the lips and respiratory distress were present, and by November 7 a respiration-rate of over 30 per minute, fever up to 100° to 101° and the rapid increase in the frequency of the pulse, from 104 to 140 in the course of the day, all showed a serious turn in her condition. Some relief was obtained from hypodermic injections of adrenalin, atropine, and strychnine, but the pulse gradually failed, remittent fever up to 103° to 104° continued with respiratory embarrassment and a respiration-rate rising to 40 and over. Incontinence of urine set in; there was delirium at night followed by coma, and death occurred on the afternoon of November 12, over three weeks from the termination of the pregnancy. Permission for a post-mortem examination was not obtained but leave was given to take portions of the liver and kidney for histological examination. In opening the abdomen for this purpose nothing abnormal was seen in the peritoneal cavity.

The portions removed from the two organs were examined by Dr. Bamforth, of the Jenner Laboratory, St. Thomas's Hospital. They were too small to allow of a naked-eye description.

Histological Examination: Liver.—There is well-marked and extensive necrosis, the central portions of the lobule being chiefly affected. In the necrotic areas, the nuclei of the hepatic cells do not take up the stain and the cell-protoplasm is granular. Sections stained for fat show marked fatty degeneration especially in the intermediate zone. Here and there are brown patches of blood-pigment.

Kidney.—A similar necrosis is present in the sections from the kidney, the epithelium of the convoluted tubules being more markedly affected. The glomeruli appear to have escaped except for a slight degree of fatty change. Sections stained for fat show, however, that even in the tubules which appear unaffected when stained by the usual method, there is marked fatty degeneration. Slight interstitial change is also present. The necrosis of the tubular epithelium is widely distributed throughout the section.

There are many points in this case on which we would like to hear the experience of others. In the first place there is the onset of toxæmic symptoms (vomiting and jaundice) within five weeks of the last menstrual period, followed by the rapid development of inanition and cardiac weakness, shown by the administration of camphor by her previous medical attendant within a fortnight of the onset of the illness, and the extreme emaciation and the rapid, feeble pulse when the patient came under our observation.

Secondly, the rapid and striking improvement that followed the emptying of the uterus made us hope that in spite of occasional relapses and recrudescence of vomiting, complete recovery was but a matter of care and time. Thirdly, the onset of paralytic symptoms about a fortnight after the termination of the pregnancy marked the beginning of the end. Various types of toxic neuritis are associated with the intoxications of pregnancy, but one so closely following the diphtheritic type is outside our experience.

Lastly, the histological examination of the liver and kidney corresponds very closely with the usual descriptions in similar cases. The statement that the damage in the liver-cells is chiefly in the centre of the lobule in toxæmia with vomiting, and at the periphery of the lobule in eclamptic toxæmia, is borne out by the findings in this case. Whitridge Williams appears to have been the originator of this statement, which has been copied from one text-book to another, as it is just one of those delightfully simple generalizations that will appeal to the teacher as excellent pabulum for his students. Whitridge Williams's view seems to be accepted by Seitz in Stoeckel's "*Lehrbuch der Geburtshilfe*," and this single case of ours for the most part bears it out, though his further statement that the cells in the periphery of the lobule are intact does not hold good in our case. The central fatty change seems to be characteristic of the acute yellow atrophy type of pregnancy intoxication. Sternberg, in Aschoff's "*Pathologische Anatomie*," says that central fatty degeneration characterizes the liver in the diphtheritic intoxications of children, which appears to be of interest in this case in view of the form of paralysis that appeared in its terminal stages. Hinselmann, in his exhaustive treatise on eclampsia, quotes three cases of toxicosis following diphtheria, with hepatic changes similar to those seen in eclampsia.

Discussion.—Dr. H. RUSSELL ANDREWS (President) said that this was an interesting account of a very interesting case. He thought that the diphtheritic type of paralysis must be an extremely rare occurrence in toxæmia of pregnancy. Accurate accounts of cases of real toxæmic vomiting were of great value, as such cases were not often seen.

Dr. T. W. EDEN said it would be interesting to hear whether Dr. Fairbairn had any explanation to offer of the very late onset of the neuritis and the other lesions which resulted in the fatal termination of the case.

Dr. Z. MENNELL said Dr. Fairbairn had co-operated in giving an accurate and fair description of this interesting case, upon which he (Dr. Menzell) had called Dr. Fairbairn into consultation. The points which impressed him (the speaker) most were: (1) The extreme emaciation—and it was difficult to believe the patient had been a fat, healthy girl, able to play tennis, &c., up to the sudden onset of the illness; (2) the dramatic effect of the infusion which was the equivalent to the first meal she had retained for some time; and (3) the similarity of the early stages of this illness to the condition known as delayed chloroform poisoning. He thought it was curious to choose chloroform as the anæsthetic for the examination and introduction of the pessary in Switzerland, where ether was almost universally used. The paralysis corresponded closely to that usually associated with a post-diphtheritic polyneuritis.

Mr. W. H. F. OXLEY related a case in which toxæmia of pregnancy had been accompanied by peripheral neuritis. A young primipara was admitted at term suffering from headache and paralysis of the facial nerve form. The same evening she was confined and soon afterwards had an epileptic fit, during which the systolic blood-pressure rose to 180 and remained there. A series of fits, about twenty in number, supervened, during the intervals of which she was in a state of coma. Veratrine, hypodermically, brought the pressure down to 90 mm. systolic, and the fits ceased. She had two more on the third day and recovered. The urine contained only a trace of albumin, and no acetone bodies were present at first, although they were found later. Although not a typical case of eclampsia, there was no epileptic or neurotic history, and he thought it must be considered as a case of toxæmia.

Dr. EVERARD WILLIAMS recalled a case of pregnancy toxæmia and jaundice which occurred in the maternity department of St. Bartholomew's Hospital three years ago and in which, after four or five days of slight jaundice, labour set in. The subject was a young primigravida at term. Within a short time of the birth of the child, fits and deep coma developed and the woman was then admitted to hospital. The case was treated on the usual lines, but the jaundice gradually increased and the woman died about seventy-two hours after admission. During this time there was marked oliguria with much albumin. Van den Bergh's test was positive by the indirect method. At the autopsy the liver was found to be small and atrophic, and the sections showed central necroses. Sections stained by methyl-aniline violet showed amyloid deposits in the walls of the arteries, indicating a lesion of considerably longer duration than the present brief illness. The view expressed by Sir Bernard Spilsbury was that some abortifacient drug had probably been taken during pregnancy, and that this had damaged the liver. He (Dr. Everard Williams) asked whether Dr. Fairbairn had had sections similarly examined in his case, in order to exclude such a possibility.

Dr. FAIRBAIRN (in reply) said that although in this case the progress of the disease was not of the acuteness exhibited in the case mentioned by Dr. Everard Williams, it could not be described as "chronic," for within a few days of its onset, jaundice of moderate degree, as well as vomiting, had been present, and, arguing from the resort to camphor injections, serious cardiac weakness as well. The sections were not stained for amyloid disease but this could yet be done. Multiple neuritis had frequently been noted in connexion with the toxæmia of pregnancy, and some twenty years ago Dr. Turney had described a series of cases in the St. Thomas's Hospital reports. It was difficult to understand why the evacuation of the uterus failed in this case, unless it was that the hepatic and renal degeneration had progressed too far for recovery to take place. In conclusion it might be stated that there was no history of infection of the fauces that would suggest recent diphtheria; also, there was no reason to suppose any drugs had been taken to produce abortion, indeed, the patient was extremely anxious to go through with her pregnancy and was so depressed at the thought of its termination that the reason given her for the emptying of the uterus was that the fetus had already perished. The only possible complicating factor was that mentioned by Dr. Mennell, the administration of chloroform for the replacement of the uterus and the insertion of a pessary.

A New Method of Determining the Patency of the Fallopian Tubes by Means of X-rays.

By EVERARD WILLIAMS, M.D., and RUSSELL J. REYNOLDS, M.B.

IN this communication we propose to outline a method of employing radiology as an aid to the investigation of cases of sterility.

It was felt that the present methods of testing the patency of the Fallopian tubes were unsatisfactory, in that they were all open to the objection of causing an increased intra-uterine and intratubal pressure in cases in which, from their very nature, it may be anticipated that organisms are lurking in the uterus or the tubes. If this is so it would be safer to employ some method of investigation which does not depend upon creating a positive intra-uterine or intratubal pressure.

A method occurred to one of us (E. W.), after reading the account of Bond's experiment dealing with the recovery of coloured particles from the peritoneal cavity,

at a laparotomy, these particles having been previously introduced into the vagina, as outlined in Eden's "Manual of Midwifery," p. 9. If opaque particles be substituted in place of coloured particles, an X-ray photograph should indicate their passage through the tube.

After consultation with Dr. Eden and the late Dr. Herbert Williamson, who both felt it could do no possible harm to the patient, the experiment was made. Particles of barium sulphate were insufflated into the cervix uteri and X-ray photographs were

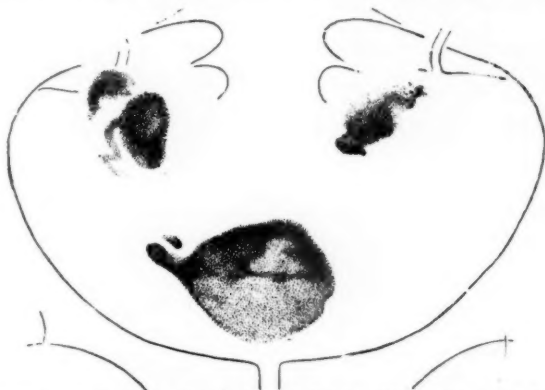


FIG. 1.—Shows shadow in uterine cavity and distal portions of tubes and fimbriated extremities. (A normal case.)



FIG. 2.—Shows shadows similar to fig. 1. A normal case ten hours after barium introduction.

taken twenty-four, forty-eight and seventy-two hours later by Dr. Russell Reynolds. Several cases of normal multiparous women were thus investigated. The photographs did not give a dense enough shadow to be really satisfactory, and at Dr. Russell Reynolds' suggestion an emulsion of barium sulphate was tried. This method gave good results, particularly when general anæsthesia was employed (figs. 1 and 2). Accordingly a simple apparatus was devised for the introduction of bismuth or barium emulsion into the uterine cavity, and this apparatus has been constructed

by Messrs. Allen and Hanburys (fig. 3). The point we wish to emphasize is that the emulsion is merely dropped into the uterine cavity and is not forced in under pressure. An anæsthetic is not necessary.

The question naturally arises as to how the particles find their way into the tubes. The observations (1) that better shadows are obtained if the patient has been under an anæsthetic (with the accompanying post-anæsthetic retching and vomiting), and (2) that Bond placed the particles in the vagina and not in the uterus, suggest the possibility of aspiration being the motive power, though the original conception was that the ciliated epithelium swept the particles up. To settle the point certain experiments were performed upon animals.

In one experiment, a control, Bond's experiment was repeated, and bismuth emulsion was introduced into the vagina of a rabbit. Unfortunately the animal died under the anæsthetic. Vigorous resuscitatory measures, however, were adopted. The body was kept warm and was photographed four hours later. The X-ray shows shadows in both horns of the uterus. In another experiment bismuth emulsion was introduced into the vagina of a rabbit, the abdomen immediately opened and both Fallopian tubes ligatured. The animal was killed twelve hours later and photographed. The photograph shows the bismuth emulsion still in the vagina, at the original site of introduction, and it will be seen that it had not passed on into the uterus (*vide slide*).

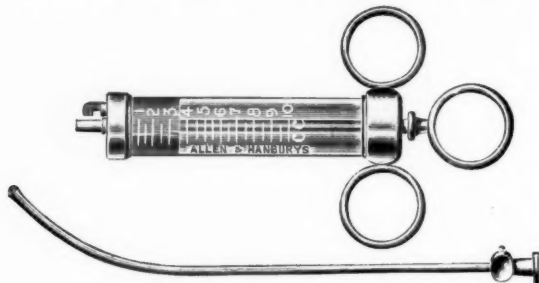


FIG. 3.—Syringe and cannula.

These experiments show that the particles do not find their way up the Fallopian tubes unless there is a free communication between the tube and the peritoneal cavity. They suggest that a purely mechanical factor, such as aspiration, is more likely to be responsible than anything in the nature of ciliary activity.

If the tubal ostia are patent the emulsion travels from the uterus to the tubes, and therefore if the shadow is seen in one tube at all, it indicates patency of that tube. In the course of seventy-two hours the whole of the bismuth or barium has disappeared. In no instance has there been complaint of any after-effects.

Another point worthy of mention is that the shadow in the body of the uterus gives a much more accurate estimation of the size of the uterine cavity, in cases of infantile uterus or other mal-development, than does the bimanual examination, which only reveals the gross size of the uterus, and thus may be masked by the presence of a fibroid or an unduly thick wall (*vide slide*).

We have used the method to determine the patency of a tube after salpingostomy for tubal mole (impossible by the other methods if the opposite tube is patent); also in a case which proved to be that of a double uterus. The value of the method as an aid to diagnosis in other difficult cases is obvious.

We believe also that the demonstration of the passage of particles from the uterine cavity to the fimbriated extremity of the Fallopian tube throws some light on the

origin of ascending infections of the genital tract, and perhaps also on that of endometrioma of the ovary and the deposit of fragments of exfoliating endometrium on the pelvic peritoneum and the surface of the ovary.

In conclusion we express our thanks to Dr. Eden and Dr. Cuthbert Lockyer for permission to investigate their cases, and to Dr. Piney and his staff for assistance in the laboratory experiments.

The Investigation of the Uterus and Fallopian Tubes by Air and Opaque Bodies in Sterility.

By SIDNEY FORSDIKE, M.D., F.R.C.S.

INFLATION of the uterus and Fallopian tubes has been practised for many years as a help to the elucidation of sterility in the female, but hitherto the apparatus necessary has been too cumbersome and too elaborate to allow of the investigation being carried out as a clinical test. I have used a simple apparatus for a considerable time as a routine method in the out-patients' department and venture to describe it before this Section of the Royal Society of Medicine.

Precedent to the application of the test the following conditions are observed:—

- (a) That the semen of the husband is normal; for it is clear that no investigation of the female for sterility should take place until the male is known to be normal.
- (b) The absence of acute or subacute inflammation of the adnexa, lest it be aggravated.
- (c) That the cervix is free from purulent discharge, lest infection be carried upwards to the uterus and tubes.
- (d) That time be well spaced from the menstrual period, for the endometrium is then congested and thickened, resistance being thereby offered to the passage of any fluid except at a much increased pressure. Secondly, as we have lately heard a great deal about the occurrence of air embolism during urethroscopy when slight hæmorrhage was taking place, I conceive that this might equally happen in the uterus during menstruation.

APPARATUS A.

(a) Hollow uterine dilator, which is the third in the series, well tapered and with the opening at the tip.

(b) The bulb and reservoir ordinarily used with a Junker chloroform inhaler. The reservoir has a specially constructed net of strong twine which gives it a fixed support.

Two points about this simple apparatus are to be noted:—

(1) The uterine tube, being tapered, fits the os accurately, and the resilience of the cervical muscle grips it tightly and prevents the escape of air. There is no necessity for the application of the various devices suggested for occluding the os and preventing the back-flow of air.

(2) No manometer is used, nor is it necessary, for my experience is that when either uterus or tube is under tension and air does not pass, obstruction is present, and I proceed to the further investigation by means of opaque substances. For practical purposes, however, I measure the pressure in the reservoir by testing it beforehand against a manometer, and I thus obtain reasonably accurate information of the pressure employed.

Again, it is noted that the apparatus is not burdened with gas-containing cylinders. Various gases have been used, oxygen, carbon dioxide and air; the two former involve the use of cylinders, whereas air requires no additional storage. The question arises as to whether there is any valid objection to the use of air as against oxygen or CO₂, and considering how we expose the peritoneal cavity during operation without ill result, the answer must be, "there is no real objection." The difference in the rate of absorption of small quantities of these gases must be negligible.

METHOD EMPLOYED.

The test is carried out in out-patients. The patient places glycerine plugs against the cervix on the two nights preceding the test. She is then put in the Trendelenburg position on the couch, for two reasons: (1) The suprapubic area forms the dome of the abdomen, so that any air entering the abdomen forms a bubble in that area, whereas an increasing area of tympanites can be demonstrated by percussion as the quantity of air increases. (2) In this position the vagina can be kept filled with saline and any back-flow of air is immediately detected.

The vulva, vagina and cervix are dried with swabs and thoroughly painted with iodine, the lower part of the cervical canal being also cleansed and painted. A vulsellum fixes the cervix, and the two early members of the series of dilators are passed through the internal os, followed by the third, which is the uterine tube. The bulb is now attached to the uterine tube and the vagina filled with sterilized saline or water. Pumping slowly, the reserve bulb is soon full and its net under tension. If the tubes are patent the reservoir slowly deflates, the one fallacy being a leaking valve in the air-bulb, which should of course have been detected beforehand.

EVIDENCE OF THE PASSAGE OF AIR INTO THE PERITONEAL CAVITY.

(1) In the early days of tube-inflation the pneumo-peritoneum was demonstrated by X-rays, and if that were necessary it would seriously diminish the value of the test clinically. Fortunately it is not necessary, for when the tubes are patent the air passes evenly and gradually, as seen by the deflation of the reserve bulb.

(2) On auscultation above the tubes the air can be heard on one or both sides according to whether one or both tubes are patent. When there is a layer of fluid over the fimbriae a succession of bursting bubbles can be heard, at other times a gurgling or hissing noise; and whichever is heard, it can immediately be arrested by pinching the rubber connexion of the reserve bulb and uterine tube, and it is again heard upon freeing the tube.

The sounds produced by the passage of air through the abdominal ostium can be studied in the course of an abdominal operation to much greater advantage and accuracy than upon the post-mortem uterus, for in the absence of coils of living gut in the latter case the conditions are not comparable. The uterine tube is introduced before opening the abdomen, and when the pelvic organs are exposed inflation is carried out.

When the lumen is fairly free and the pressure is raised to 100 mm. Hg a hissing noise is produced; if a layer of fluid be placed over the ostium a succession of bursting bubbles is heard. If the Fallopian tube be buried beneath coils of small intestine a gurgling is produced, as the coil of intestine is raised by the increased pressure and separates from its neighbour to fall back again as the pressure is relieved.

(3) An increasing globular area of tympanitic resonance can be demonstrated suprapubically on percussion, as the quantity of air in the peritoneal cavity is increased.

QUANTITY OF AIR PASSED.

In my earlier cases I passed air into the peritoneal cavity until the tympanites was well marked. This resulted in considerable discomfort to the patient, even in the Trendelenburg position; also in sharp pain in the chest, with embarrassment of the heart's action on regaining the upright position. With improved observation I find it necessary to inject only a few cubic centimetres of air the passage of which can be shown by the above-mentioned methods. With this modification I have no hesitation in carrying out the test in the out-patient department, for few patients make any complaint and at most such complaint is of a slight pain in the shoulders which passes off before she leaves the hospital or house; only one informed me that she had noticed it for three days.

FAILURES.

There are two classes of cases which involve failure unless with anæsthesia—cases of vaginismus or hyperæsthesia, and those hard, resisting cervixes into which it is difficult to pass a probe through the internal os.

THE PRESSURE TO BE USED.

Much discussion has centred around the rate of flow of the gas into the uterus, the measurement of the total pressure, and as to whether obstruction is to be inferred at 100, 150, 200 mm. Hg. To answer these problems it is necessary to see what happens when the inflation is carried out while the abdomen is open.

With the abdomen open and the pelvis filled with saline to a level at which the abdominal ostia are submerged, air passes through some tubes without delay and at a pressure not greater than 50 mm. Hg. In some cases, however, air did not pass until the reserve bulb was under a tension which indicated a pressure of 95-100 mm. Hg, and here the uterus became tense for a fraction of time before the air passed and the viscus resumed its normal appearance. In these cases the Fallopian tube appeared to be flaccid and collapsed, thereby offering resistance to inflation, but upon subsequent inflation the air passed without tension. The resistance in these cases is certainly not due to organic obstruction, but rather to a transient phase in a normal tube. It is clear that in neither class of case is an abdominal operation needed. When obstruction is present distension of the organs takes place according to the site of obstruction; when the block is at the uterine end of the tube distension of the uterus only takes place, it rises up, the peritoneal surface is taut and when grasped it feels like an air cushion and no more air will pass into the viscus. I am unable to confirm Bonney's observation that distension of the uterine cornua takes place, and I do not think that anything in the nature of rupture or even local bulging of the uterus is possible at legitimate pressures.

When the block is in the tube or at the fimbria, air does not pass and the stress falls first upon the relatively thin-walled tube and later upon the uterus. The tube passes into a state of tension and presents the appearance of a solid viscus with the covering peritoneum tense; this happens at a pressure of 95-100 mm. If the pressure be considerably raised dilatation goes on, the wall becoming thinner and almost transparent; I have no doubt that were the pressure maintained rupture would ensue. From these observations we may conclude that, wherever the obstruction, a pressure which is just sufficient to produce distension of an organ is safe; whereas higher pressures place the Fallopian tubes in some danger if they are taking the stress. I find that 150 mm. Hg is the maximum required, and never work beyond that. It is not necessary to push the investigation beyond this limit clinically, for in common with the other hollow viscera the uterus and Fallopian tube when under tension give rise to acute pain and vagal disturbance, which are instantly relieved upon the pressure being lowered. These pass off more slowly, if the pressure is maintained but not increased, as the distended viscus accommodates itself to the pressure. Therefore, as soon as the patient complains of pain I stop the procedure and conclude that there is obstruction.

LOCALIZATION OF THE OBSTRUCTION.

Having concluded that there is obstruction it is of some practical importance to decide upon the site of it; this I proceed to do by the injection of an opaque substance—lipiodol. The patient is sent away for a week to allow of the absorption of the air introduced, otherwise it would be impossible to displace it by the opaque substance.

Lipiodol is a viscous oily compound of iodine (40 per cent.) and oil. The iodine is chemically closely bound to the oil, for it does not give the iodine test for starch, and is only freed when heated, copious purple fumes being evolved. The opacity to

X-rays is due to the iodine content, and Sicard first introduced its use into radiology for localization of tumours of the spinal cord.

The patient undergoes the same preparation as for air inflation, with the addition of an aperient twenty-four hours beforehand. She is then placed in the lithotomy position, and with the same precautions and procedure the uterine tube is introduced into the uterus. The uterine tube is now connected to a Record syringe by a short length of rubber tubing both of which are filled with lipiodol. The vagina is then plugged with gauze to maintain the tube in position and to prevent leakage of the lipiodol on to the X-ray couch. The patient is removed to the X-ray room, and screened while the lipiodol is being injected; when she complains of pain a photograph is taken.

Pain is complained of when there is obstruction owing to the distension of the viscera behind the block; and the pain continues and becomes more acute if the pressure be increased, but is instantly relieved on the pressure being lowered. When tension is produced by lipiodol our object is achieved, for the opaque body has arrived at the seat of obstruction, we can lower the pressure and are ready for the photograph. Secondly, there is a transient acute pain when the first drop of lipiodol touches the pelvic peritoneum; this passes off in a few seconds.

THE EFFECTS OF LIPIODOL.

Lipiodol is completely absorbed from the peritoneum in from seven to ten days, and no shadow can be seen after the latter date. For two or three days following the injection there may be some uterine hæmorrhage but it passes off and no further disturbance has been observed. The question as to whether the lipiodol has any ill effect upon the epithelium of uterus or tubes, or even upon the peritoneum, is of grave importance, and the answer is, "No." I conducted a series of experiments upon cats, injecting lipiodol into the Fallopian tubes and widely into the peritoneum. Examination of the tissues from three days to six weeks showed no evidence of peritoneal irritation nor yet of disturbance of the epithelium lining the uterus. In addition, I opened the abdomen of a patient who had been injected six days before, and although I found a small pool of lipiodol in Douglas's pouch there was no evidence of peritoneal irritation.

The radiogram after lipiodol injection shows us:—

(1) Position, appearance and direction of the Fallopian tube. (2) The exact site of obstruction, whether in isthmus or ampulla. (3) Whether an operation for relief of obstruction is advisable when the fimbriæ are closed, or inadvisable when the block is at the cornual extremity.

These two methods of investigation, when combined, seem to me a considerable step forward in estimating the wisdom of an abdominal operation for sterility, for they not only tell us that the Fallopian tube is blocked, but also show us the point of obstruction. I consider that any operation for restoring the lumen of the Fallopian tube when obstruction exists at the cornual end is doomed to failure, notwithstanding the ingenious operations devised and figured for this purpose. I do not believe that their use is ever attended with success.

On the other hand a more hopeful outlook attends the restoration of the lumen when the fimbrial extremity is occluded, for where the abdominal ostium can be opened by the unfolding and conservation of the fimbriæ, there is some prospect of their remaining open with the possibility of subsequent pregnancy. It is in this class of case that inflation may later be carried out both to maintain the restored ostium and to demonstrate that the operation has been a success.

The advantage of inflation without anæsthesia lies in the larger number of cases that can be done, without encroaching upon the use of hospital beds which are already taxed to the utmost capacity. Where inflation is positive valuable information is gained, but when negative the result affords no indication for

treatment. The injection of lipiodol, however, supplements the negative information and indicates the site of proximal obstruction, but it tells us nothing of the state of the Fallopian tube beyond the obstruction. This is of considerable importance, for I have set out to relieve obstruction at the isthmus of the tube, only to find that the ampullary end of the tube was completely disorganized. Criticism may possibly be directed to the amount of dilatation of the cervix necessary while the patient is conscious; the answer is that only about 10 per cent. require an anæsthetic, whereas the remainder suffer no greater inconvenience than that of fixing a vulsellum in the cervix and passing a sound into the uterus.

In conclusion I wish to thank Dr. Gray for her clinical help, and Dr. Berry's assistants, Miss Gray, Miss James, and Miss Fouldes for the radiographic work.

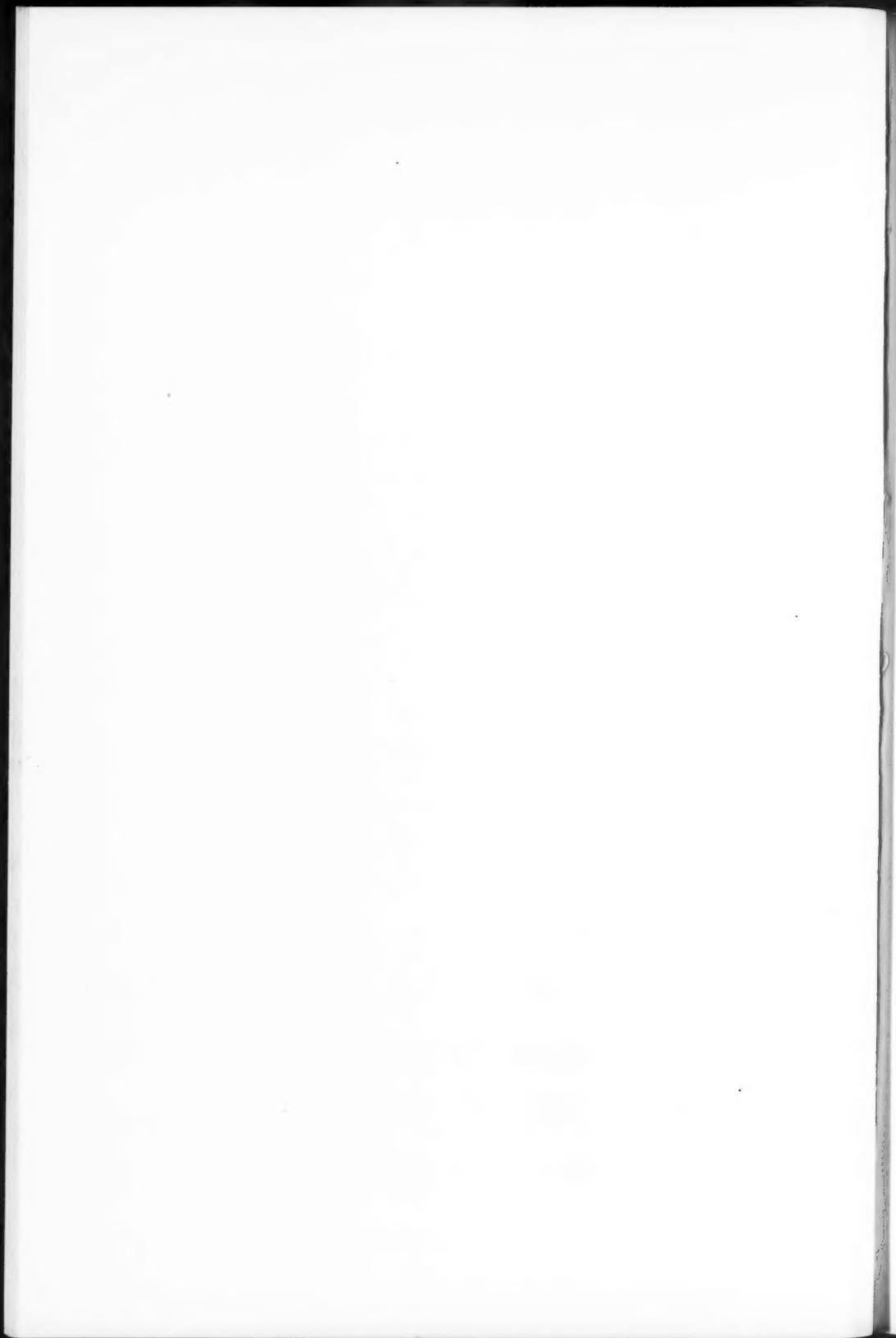
Discussion.—DR. H. RUSSELL ANDREWS (President) said that two ingenious and interesting methods had been described. The chief objection that had been raised to the ordinary method of inflation of the tubes was that although the presence of obstruction could be proved there was no way of telling where the obstruction was situated and whether it could be dealt with without opening the abdomen. These two methods showed the exact spot where the obstruction occurred.

Dr. EDEN said that the authors had done interesting work in connexion with what was a comparatively new subject. He thought the time had not yet arrived to say which was from all points of view the best method of testing the patency of the Fallopian tubes. He was not prepared to accept without question the statement that the introduction of insoluble foreign matter into the peritoneal sac would do no harm, when the passages were not obviously infected. It appeared to him very likely that a pool of lipiodol in Douglas's pouch would lead to trouble sooner or later. There seemed no doubt that the use of X-rays gave valuable information as to the position of the block in the tube which could not be obtained by simple inflation.

Mr. GORDON LUKER said that he had used Rubin's apparatus with carbon dioxide sparklets and manometer in a considerable number of cases without any ill-effect. He thought it quite satisfactory, and simpler than radiological methods. He used an anæsthetic. Occasionally the patient complained of a little abdominal pain on recovering from the anæsthetic, but this soon passed off. He had never seen any symptom or sign of peritonitis. In a fair proportion of cases he had found the pressure rise to perhaps 150 mm. before the gas passed through the tubes, and then the gas passed quite suddenly and the pressure dropped to under 100 mm. He thought these were probably cases in which there were light, filmy adhesions over the ends of the tubes which were thus rendered patent by the passage of the gas. In his experience the Fallopian tubes were nearly always patent in cases of sterility in which there was no history or evidence of salpingitis.

Miss MARTINDALE referred to the methods she found being used in some of the gynæcological clinics she had recently visited in the United States, where Rubin's test was being used very extensively in suitably selected cases. She had been interested to note in William P. Graves' most recent publication, a definite reaction against this test being used as a routine measure on account of its danger in acute, subacute and even chronic pelvic inflammation.

Mr. F. L. PROVIS said that he had had considerable experience with Professor Kurrier's apparatus, and that it had always proved most satisfactory. The essential point was to maintain a steady flow of gas and to be able to see and gauge what one was doing. The time taken, and the expense of carrying out investigations by injection of metals into the tubes and the subsequent X-ray photography, were considerable drawbacks to the usefulness of these methods. He considered that the localization of the focus of obstruction in the tube or tubes was of little or no practical value—and that the results of salpingostomy were most discouraging in the experience of the great majority of gynæcologists. He thought that the tubes might be kinked without actual occlusion and that a retroverted heavy uterus with adhesions might cause occlusion of the tubes by pressure, without there being any organic obstruction.



Section of Obstetrics and Gynaecology.

President—Dr. H. RUSSELL ANDREWS.

Endometriomata of Vulva and Perineum.

By A. C. PALMER, F.R.C.S.

THE tumours about to be described in the following paper are of the type already known as endometriomata [1].

That they are of comparative rarity is shown by the relatively small number recorded in English surgical literature since 1920.

Each of the three tumours was situated in the region of the vulva: one in the subcutis of the perineum; one in the subcutis at the upper part of the left labium majus; and one in the upper part of the right labium majus. Sampson records two tumours found in the abdominal scars after pelvic operations involving incision of the uterus.

Crosslé [2] found a similar growth embedded in the lower segment of the right rectus abdominalis after an operation for fixation of the uterus, but the exact details of the operation were not stated.

Abbott [3] records two such tumours in abdominal scars after Cæsarean section. In one case the lump was noticed three months after operation, and removed six and a half years after operation on account of slight increase in size and some tenderness on pressure. In the second case, it was not noticed until seven and a half years after the operation.

Cullen [4] has described the existence of adenomyoma (endometrioma) in the recto-vaginal septum, pelvic and adjacent organs, and rarely elsewhere, where the structure was identical with the lining of the body of the uterus. He also reports three cases of adenomyoma (endometrioma) found in abdominal scars after operations on the uterus, one after operation for adenomyoma of the uterus, the other two after a single Cæsarean section.

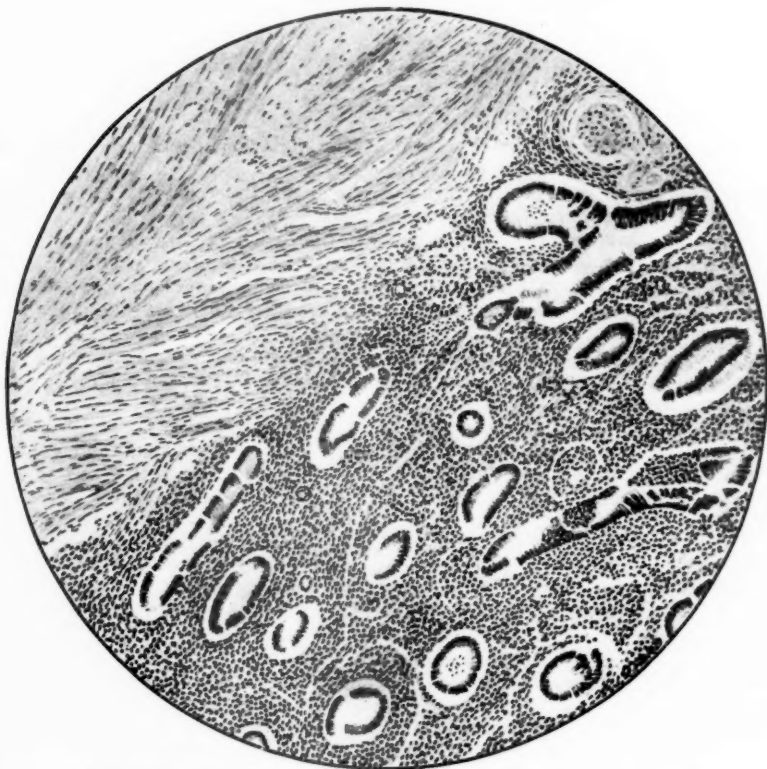
According to the view of Sampson, endometrioma, either benign or malignant, escaping through the patient's Fallopian tubes at the time of operation, or difficult menstruation, may continue to grow on near-by organs, especially the ovary, there tending to assume renewed activity, sometimes with cystic growth.

This view is in keeping with the experimental work of Jacobson [5] on rabbits in which he demonstrated the successful growth of transplanted endometrium. The occurrence of endometrial tumours in abdominal scars after operations involving section of the uterus would seem to offer further evidence of the correctness of Sampson's view.

In one of my cases the endometrioma may have been the result of implantation, in the other two cases there had been no previous operation of any kind.

Case I (see fig. 1).—M. B., aged 41, was seen on June 30, 1924; she had had two children, followed by a miscarriage four years ago. At the first confinement sixteen years ago the perineum had been torn and sutured. Early in 1921 she had been curetted on account of menorrhagia for eighteen months, and six months later in the same year a subtotal hysterotomy was performed, on account of recurrence of excessive bleeding. In June, 1924, the patient complained of dragging pains in the groin, backache, chronic vaginal discharge, bleeding on defecation, and a kind of itching discomfort in the perineum, which came on in attacks. The bleeding had occurred daily for the previous six weeks, and was thought to be due to piles. Examination showed a moderate cystocele, a deficient perineum, and chronic infection of a lacerated cervix. No evidence of piles was discovered, and the patient was not bleeding at

the time of examination. When the patient was examined later under an anæsthetic, during a period when bleeding had occurred, the use of the sigmoidoscope revealed no pathological changes in the rectum. During the examination a small spot of blood was noticed on the skin surface of the scarred and deficient perineum. A minute bleeding orifice was discovered



Müllerian tubules and endometrial stroma from subcutis of perineum. $\times 90$.

in the perineum, and beneath it, within the subcutis, a hard nodule of indefinite outline approximately 1.5 cm. in diameter was discovered.

This nodule was removed during the course of the usual repair operation.

Dr. Ernest Shaw, Pathologist to the Royal Northern Hospital, submitted the nodule to histological examination, and his report is given below.

Microscopic Report.

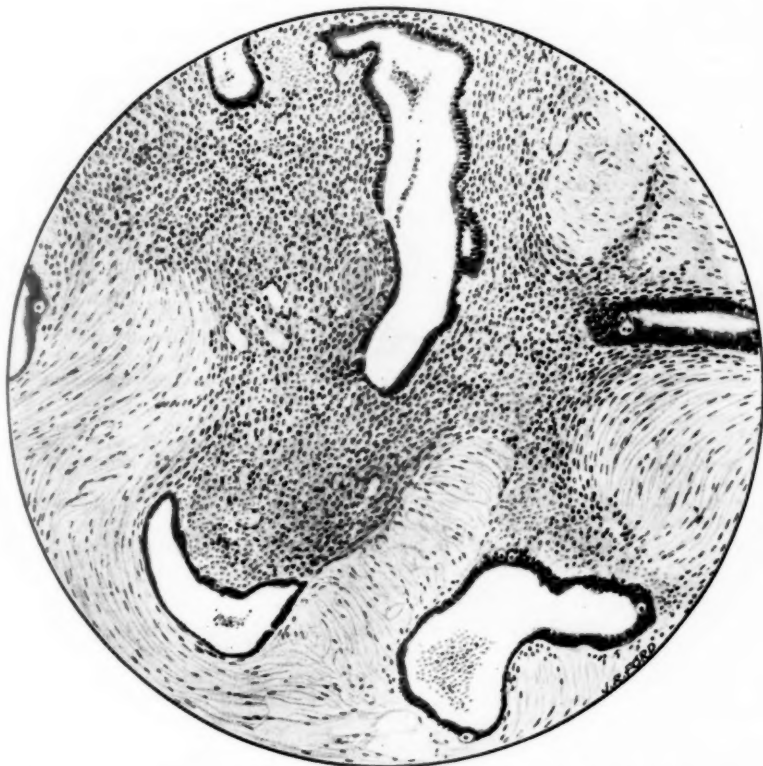
"Many collections of columnar-celled glands are seen scattered throughout the section. The largest group of glands is centrally situated just below the surface epithelium. The columnar cells lining the gland tubes are arranged in a single layer; some of the glands are large and cystic. The tubes are surrounded by a cellular stroma composed of closely-packed, short, spindle, oval and round cells.

"The picture exactly resembles that of the mucous membrane of the body of the uterus. Blood is seen in some of the gland tubes and in places between the stroma cells.

"The tissue between the groups of glands consists of fibrous tissue, unstriped and striped muscle, and there is considerable infiltration by small, round inflammatory cells. The surface of the section is partly covered by squamous epithelium.

"Near the large collection of glands is an irregular cystic space. This space is lined by squamous epithelium for the greater part, but a small part of the lining consists of a single layer of columnar epithelium similar to that lining the gland tubes. The transition from squamous to columnar epithelium is sudden.

"In the deeper part of the section are several tube-like glands lying in a fibrous matrix, without the intervention of any spindle- or round-cell stroma."



Müllerian tubules and altered endometrial stroma in wall of canal of Nück and inguinal hernia. $\times 90$.

The patient was discharged in July, 1924, to be seen again in October, when, in addition to menopause symptoms, she was complaining of itching and irritation in the perineum. No abnormality was discovered in the perineum. In January, 1925, she was still complaining of itching and irritation in the perineum and pain on defæcation.

The irritation and pain came on in attacks at intervals of about seven to ten days in November, December and January; the last attack, now passing off, was much less severe. Examination showed a small, tender nodule, about the size of a small cherry, just beneath the skin in front of and to the left of the anal margin. This nodule was now thought to have arisen owing to the repair of the perineum closing the tube or tubes leading to the skin surface from

an outlying group of glands not felt at the operation. Since the attacks were diminishing in severity, and the patient showed symptoms of the menopause, she was advised to undergo medical treatment only for a few months. She now states that the attacks, though still present occasionally, are much shorter and much less severe.

It would appear, therefore, that the endometrium in the perineum is undergoing regressive changes, similar to those which normally occur in the uterine mucosa.

In the present case, since the origin of the gland tissue is unknown, the following possibilities are put forward for consideration:—

(1) *Can the growth be a secondary carcinoma?*

We think this possibility may be dismissed because of the typical nature of the glands and stroma, and the absence of primary growth in the uterus.

(2) *Is it a developmental abnormality?*

Such an explanation would appear to involve the presence of an accessory Müllerian tubule, possibly developing in the lower part of the round ligament of the uterus.

(3) *Can the growth be due to the implantation of a fragment of decidua at the time of confinement?*

This appears to be a possible explanation, as the perineum was torn and repaired at the first labour; a nidus had thus been prepared to receive any small, loose fragment of decidua.

In view of the recent explanation of the occurrence of endometriomata in ovaries, it would appear that this possibility cannot be entirely excluded.

Case II (see fig. 2).—G. H., aged 34, four children and no miscarriages, complaining of an abscess in the vulva, which had been discharging brownish or yellowish material off and on for the last two years and giving rise to throbbing pain during the menstrual periods.

On examination, a small sinus was found in the upper part of the left labium majus surrounded by an indurated area, the whole being about the size of a cherry. On her being examined a fortnight later, a left inguinal hernia was discovered.

The patient was admitted to hospital and operated upon on January 3, 1925. The hernial sac passed downwards into the left labium majus, and stopped short of the nodule surrounding the sinus.

After herniotomy the subcuticular nodule and sinus were removed from the left labium majus. The nodule was submitted to microscopic examination, and described as follows:

Hæmorrhage in and around Müllerian tubules in the wall of the canal of Nück.

The tubules are lined for the most part by a single layer of columnar cells. The cells are often vacuolated, and in places flattened, from distension by blood in the larger cyst-like spaces.

Surrounding some of the tubules is a hæmorrhagic-celled stroma which resembles endometrial stroma, but does not appear to be typical of endometrium.

In this case the nodule had been giving rise to symptoms for two years. No other abnormality was found in the genital canal and no previous operations had been performed, so that some form of developmental abnormality would appear to offer the most likely explanation of the occurrence of the nodule in the labium majus. The round ligament was extremely well developed and passed down the whole length of the wall of the hernial sac.

Case III.—S. K., aged 29, one child, 10 years old, one miscarriage, five years ago.

She was admitted to the hospital on January 10, 1925, as an emergency case complaining of acute abdominal pain and some bleeding.

The symptoms and signs suggested a ruptured tubal pregnancy, but, in addition, there was found a small hard nodule in the mons Veneris on the right side. The patient stated that the nodule had been tender for three days, and that she had not noticed its presence before.

Laparotomy confirmed the main diagnosis. The right tube was ruptured and approximately two pints of fluid blood were found in the abdominal cavity. After removal of the right tube and performance of the usual abdominal toilet, an incision was made over the nodule in the right side of the mons Veneris. The nodule was found to extend upwards into the inguinal canal beneath the fascia of the external oblique. No hernial sac was discovered in the removal of the nodule.

After hardening, the nodule was approximately 4 cm. in length by 2 cm. thick and 2 cm. at its broadest part. On section it was found to consist of a number of spaces distended by blood-clot and held together by fibrous tissue. Histological examination showed that the hæmorrhage was partly in defined spaces, and partly into the tissues of the wall of the canal of Nück. In two places the spaces showed a lining of spheroidal and large granular polygonal cells, resembling endothelial cells. The tissue outside the spaces was greatly infiltrated with leucocytes, eosinophil leucocytes, pigment granule cells and fibroblasts.

In this case the histological picture does not closely resemble endometrium, but it must be remembered that the patient had also had an ectopic pregnancy and that previous to operation she had been bleeding for three weeks.

It would therefore seem probable that the alteration in structure might be accounted for partly by pressure from hæmorrhage into and around the gland tubes and partly by resulting inflammatory reaction.

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Dr. H. RUSSELL ANDREWS (President) said that since attention had been directed to endometriomata they were being found in unexpected situations. A surgeon had told him earlier in the day that he had seen two cases in which endometrioma had occurred inside the sac of a femoral hernia. He himself had had an interesting case in which a patient, who complained of bleeding from the umbilicus during the periods, was found to have a small mass of endometrioma at the umbilicus and a similar nodule in Douglas's pouch.

Case of Prolonged Pyrexia Apparently Caused by Chronic Myometritis.

By A. W. BOURNE, F.R.C.S.

THE following case is of interest because it illustrates the possibility of pyrexia prolonged for nearly two years, being caused by a uterine infection.

The history of the case is as follows:—

In March, 1921, Mrs. F., aged 45, under the care of Dr. B. H. Stewart, of Barnet, began to be affected by general ill health with slight evening temperature ranging between 99° and 100° F. There were abdominal pains and appendicitis was suspected. The patient had two children; the labours were normal. There was no evidence of venereal infection.

May 19, 1921.—The appendix was removed and a general exploration made of the abdominal cavity—nothing was found.

June, 1921.—A small swelling suddenly appeared in the left breast, and the pyrexia, which had not abated after the removal of the appendix, was maintained and increased, especially before each menstrual period. During the menstrual loss the temperature was normal, and after the cessation it gradually resumed its evening record of between 99° and 100° F.

July 27.—The left breast was removed, and it was found to contain several cysts from which escaped some purulent material. Cultures were made from this material, and it yielded a staphylococcus. After the operation on the breast there was suppuration accompanied by continued fever and the wound healed up slowly.

October.—The teeth were extracted.

November.—Patient was seen by a physician who recommended that the left nipple, which had not been removed with the breast, should be excised, but the surgeon recommended that this should not be done.

Between November and January the ill-health continued; there was a slight loss of flesh, and painful swellings developed in the right breast. I saw her in

January, 1922, examined the pelvis but could not find anything positive beyond a trifling leucorrhœa and retroversion of the uterus, which was not enlarged. I recommended the preparation of a blood-culture, which was carried out by Captain Douglas, of the National Institute for Medical Research at Hampstead. After several days' growth under anaërobic conditions, a staphylococcus was recovered, which gave the same characteristics as the organism found in the breast. A vaccine was made from this microbe and it was injected regularly from January to March, 1922, without any effect on the temperature or general health. The leucocyte count was increased slightly and did not show any reduction during the afebrile period of menstruation.

In March, 1922, a tender spot appeared in the right breast with discharge from the nipple; a culture was again prepared by Captain Douglas from this discharge, and it was found to contain staphylococci.

On March 15 the right breast was removed, together with the left nipple. Microscopical examination of the right breast showed chronic fibrocystic mastitis, and the cysts yielded a pure culture of staphylococci.

For a short period after the right breast was removed, the temperature declined and became subnormal night and morning, but within a few weeks, in June, there was a sudden return of the former evening pyrexia and a quickened pulse. The patient's complexion was now muddy; she was thin and languid, but was not confined to her bed though she spent most of her time sitting about in her garden. Inoculations were still without any result. July and August were spent at the sea, but her general condition was not affected.

In September she was seen by Sir Almroth Wright, who, with his assistants, particularly Dr. Colebrook, made exhaustive examinations of her blood-resistance and gave her transfusion of blood immunized against her own staphylococci. A second transfusion was given in October, but examination made again of her resistance showed no improvement.

After the failure of the blood-therapy she was seen by Mr. Stevens and myself. She complained at that time of increasing pre-menstrual pains and menstrual loss. We found the uterus retroverted and considerably tender; and on January 12, 1923, we curetted the uterus and suspended it by Gilliam's operation. The right tube was also removed because it showed signs of adhesive salpingitis. After this operation there was an immediate fall in the evening temperature which had persisted so long. The general health improved and the patient began to get about again. She was apparently recovering, but in May, 1924—that is fifteen months after the curettage—the evening temperature returned with more pelvic pain and menstrual discomfort. I saw her again in October, 1924, and found the uterus very tender and the patient in a very depressed state of health. As the improvement after the curetting for fifteen months seemed to point to a uterine source of the infection, and as no other operation had been followed by so long a relief of symptoms, I recommended hysterectomy and removal of the remaining tube and ovary. On October 7, 1924, I performed panhysterectomy and removed the remaining appendages. The convalescence was uninterrupted and after a few days the evening temperature again became normal. Her general health rapidly improved and she became fit and active once more. During the last few weeks however, she has developed an acute cystitis, but as the culture yielded a pure growth of *Bacillus coli*, and as she is now nearly free from symptoms, I think that this is an adventitious infection not related to her previous trouble.

The uterus was carefully examined by Dr. Newcomb, of St. Mary's Hospital, and his report is the following:—

REPORT ON UTERUS OF MRS. F., BY W. D. NEWCOMB, M.B.

"Complete uterus with greatly thickened walls ($1\frac{1}{4}$ in.), endometrium about normal thickness ($\frac{1}{4}$ in.).

"The condition is one of subacute endometritis and myometritis.

"The endometrium is about normal thickness, but the stroma is increased at the expense of the glands. Here and there are patches of fluid exudate, and there is a diffuse infiltration by polymorphonuclear leucocytes, plasma cells and lymphocytes. This cellular infiltration is more marked in the stroma than in the glands.

"The muscular wall of the uterus is thickened and shows a diffuse fibrosis. Here and there, especially around the vessels and in the lymphatic spaces, there is an exudation of fluid. A patchy infiltration by polymorphs, plasma cells and lymphocytes is also present here.

"A careful search for organisms in sections stained by Weigert, Gram, and Murray's methods has failed to demonstrate any.

"The cervix is slightly more fibrous than usual, but otherwise normal. A small lutein cyst is present in the ovary.

"From the clinical history of the case and the extraordinary condition of the uterine muscle, I cannot help feeling that we have here a case of sustained pyrexia due to metritis."

Discussion.—Dr. REMINGTON HOBBS said that the histological appearance of endometrial tissues removed from cases of endometritis associated with slight temperature and hæmorrhage was very similar to what could be seen in Mr. Bourne's specimen. The specimen was shown to demonstrate the presence of metritis, but actually there was a very well-marked endometritis. The gland-acini were irregularly dilated and contained pent-up secretion, indicating obvious obstruction of the mouths of the glands. The original lesion, in his opinion, was to be found in the endometrium, the myometrium being involved secondarily.

Mr. A. C. PALMER said that Mr. Bourne had shown patience and perseverance in dealing with this case of chronic metritis.

Case of Chorion Epithelioma.

By A. W. BOURNE, F.R.C.S.

MRS. Q., aged 39, had had two children, the second fifteen years ago. In 1924 she became pregnant, but, owing to hæmorrhage and violent sickness, she was seen by my colleague, Mr. Leonard Phillips, in August, when two and a half months advanced. He examined her and evacuated a hydatidiform mole. Bleeding continued for a month after this operation but then ceased, and menstruation reappeared normally, but only with slight loss. She was brought to me on February 3, 1925, by Dr. Parr, of Willesden Lane, on account of continuous bleeding of slight degree, which lasted for three weeks. There was no other symptom except a trifling abdominal pain. On examination I found that the abdomen was normal. The uterus, liver and spleen could not be felt to be enlarged, nor were there any enlarged glands. *Per vaginam* there was a blood-stained discharge, slightly offensive, and the vulva was pale. The cervix was patulous and a small polypus with small hæmorrhagic patches was attached to one lip. The uterus appeared to be normal in size and was freely mobile.

In view of the history, taking particular account of the continuance of bleeding after evacuation of the mole for a month, together with the anæmia and general appearance of ill-health when I saw her in February, I decided that the polypus was a chorion epithelioma and that the uterus should be removed without any delay whatever. I thereupon performed panhysterectomy and removed the tubes and ovaries. The patient recovered normally from the operation and is still quite well.

Examination of the specimen showed the interior of the uterus to be quite normal, but a polypus was attached to the cervix as seen in the specimen. This was examined by Dr. Newcomb, of St. Mary's Hospital, who found that the polypus was a typical chorion epithelioma and that the malignant cells were in one place invading a blood-vessel. At the time of the operation no evidence could be found of metastasis in the abdomen.

Discussion.—Dr. RUSSELL ANDREWS (President) said that as Mr. Aleck Bourne had invited criticisms he would ask him why he removed the ovaries in the case of chorion epithelioma. If it was with the idea of doing a thorough operation and minimizing the risk of metastases, the President thought that removal of the vagina was probably more important than removal of the ovaries. Patients suffering from chorion epithelioma were usually aged between 20 and 30, and it was a serious matter for them to have both ovaries removed. In a case in which the uterus showed so little growth it was probably unnecessary. He would like to hear the opinion of other Fellows on that point.

Dr. T. W. EDEN said that, while he congratulated Mr. Bourne on the accuracy of his diagnosis, he did not think that any harm would have been done by removal of the polypus for microscopic examination. The question of the extent of the operation which should be performed for chorionic cancer was one which required consideration. On general grounds the vagina should be removed as well as the uterus and tubes, because, after the uterus, the vagina was the organ most prone to be attacked by this neoplasm. It was, however, doubtful whether it was practically expedient to push operations for malignant disease to their theoretical limit.

Mr. A. C. PALMER said he wondered why both ovaries were removed, as he understood that it was considered wise not to remove the ovaries in cases of chorion carcinoma, even when they contained small lutein cysts.

The Treatment of Placenta Prævia by Continuous Weight Traction—a Report of Seven Cases.

By J. A. WILLETT, M.D. Oxon.

EXCLUDING Cæsarean section, the treatment of placenta prævia consists in applying sufficient pressure on the lower uterine segment in order to control the hæmorrhage.

The methods commonly employed are either podalic version and the bringing down of a leg, or the introduction and inflation of de Ribes' bag. Both are equally effective as compressors, but there are certain disadvantages in their use. Collectively, both require a dilatation of the os, equivalent to two fingers, for their easy performance. Individually, version, whether external or bipolar, is not always a simple manœuvre, and I have known the manipulation during bipolar version cause a serious, not to say alarming, hæmorrhage. Again, at the end of the second stage how hard it is not to interfere with the birth of the aftercoming head; and yet I feel fairly sure that many maternal deaths from shock and collapse after delivery of the fœtus are caused by rupture of the lower uterine segment from uncalled-for interference.

De Ribes' bag is not always at hand when wanted; its sterilization, often incomplete, takes time; if the bag is old it is apt to leak after inflation and become a source of danger, and lastly, there may be a highly critical minute or two between the expulsion of the bag into the vagina and the descent of the dislodged presenting part into the os.

The other methods are in less common use—plugging the vagina before the os is sufficiently dilated for more active treatment and rupture of the membranes in mild cases of lateral placenta prævia. Both are useful, especially the latter, in appropriate conditions.

It is unnecessary to quote a long list of statistics, but at a recent meeting of the Edinburgh Obstetrical Society the result of the treatment of 254 patients in the Royal Maternity Hospital was given :—

Maternal death-rate (from all causes) ...	9.8 per cent.
Fœtal and neo-natal death-rate ...	64 „

A closer analysis of the figures showed

179 cases after version or bag.	
Maternal mortality just under ...	13 per cent.
Fœtal mortality ...	81 „
61 cases of { Unaided.	
{ Rupture of membranes unaided.	
{ Rupture of membranes, forceps, delivery.	
Maternal mortality—1 case (eclampsia) ...	1.6 per cent.
Fœtal mortality ...	41 „
14 cases of Cæsarean section.	
Maternal mortality 1 case ...	7.1 per cent.
Fœtal mortality 1 case ...	7.1 „

At the same meeting, Dr. Munro Kerr stated that the figures of the Glasgow Royal Maternity Hospital were approximately the same.

RESULTS OBTAINED AT:

(1) Queen Charlotte's Maternity Hospital, 1921 and 1922.

63 cases. Total maternal mortality ...	5
Cæsarean section—17 cases.	
Maternal mortality ...	0
Fœtal mortality ...	7 = 41 per cent.
Version or bag—23 cases.	
Maternal mortality ...	3 = 13 „
Fœtal mortality ...	14 = 60 „
Other 16 cases.	
Maternal mortality ...	1 = 6 „
Fœtal mortality ...	11 = 68 „
Normal delivery—7 cases.	
Maternal mortality ...	0
Fœtal mortality ...	2 = 28.5

(2) City of London Maternity Hospital, 1920-1924—52 cases.

Cæsarean section—5 cases.	
Maternal mortality ...	0
Fœtal mortality ...	0
Version or bag—34 cases.	
Maternal mortality ...	4 = 11.7 per cent.
Fœtal mortality ...	31 = 62 „
Other 11 cases.	
Maternal mortality ...	0
Fœtal mortality ...	2 = 4 „
Normal delivery—2 cases.	
Maternal mortality ...	0
Fœtal mortality ...	0

(3) St. Mary's Hospital, Manchester, 1923.

Cæsarean section—4 cases.	
Maternal mortality ...	1 = 25 per cent.
Fœtal mortality ...	1 = 25 „
Version (bag not used)—27 cases.	
Maternal mortality ...	2 = 7.4 „
Fœtal mortality ...	26 = 96 „
Other 35 cases.	
Maternal mortality ...	1 = 3 „
Fœtal mortality ...	16 = 44 „
Normal delivery—10 cases.	
Maternal mortality ...	0
Fœtal mortality ...	5 = 50 „

Too much reliance must not be placed on these figures, for there is no means of knowing of the condition of the patient or of the fœtus at the commencement of treatment, nor the reason why any particular form of treatment was adopted. But they do indicate the high maternal and fœtal mortality following version or the introduction of a dilating bag.

Regarding sepsis during the puerperium, I can only quote from the results of the City of London Maternity Hospital figures. They show that of the total number there was fever of a varying degree in sixteen patients, and that thirty-two were afebrile, whereas in the version and bag group there was fever in eleven patients, and an afebrile state in twenty-one.

Traction on the Forecoming Head.—I had for some time thought that, could the requisite pressure be exerted by the forecoming head, the disadvantages of version and of de Ribes' bag might be obviated; and with the possible advantages of earlier treatment, less interference and so diminished risk of sepsis.

Forceps and Method of their Use.—It was first requisite to obtain a satisfactory tractor forceps. The criterion I adopted was that the forceps must sustain a 3 lb. weight for twelve hours continuously when fixed in the scalp of a dead fœtus. The ordinary type of volsellum with sharp teeth failed in this respect. On the suggestion of Messrs. Allen & Hanbury, instrument makers to the hospital, I tried de Martell's surgical scalp forceps, the only modification being an increase of length to 7½ in. This instrument came through the test successfully, and up to the present I have seen no reason to alter it. It is of the T-shaped clip type, the holding bars being ½ in. long, narrow enough to pass where a finger will, and rigid enough to sustain the requisite weight when fixed in the fœtal scalp. The holding teeth are rounded.

The application of the forceps is easy and they can be applied to the scalp as soon as the os will admit a finger, thus ensuring early treatment. Preferably, an anæsthetic is given; the membranes are ruptured with the forceps which, with the blades closed, are passed through the os until the head is reached. The blades are then separated, and pressing on the scalp, closed, when a grip on the scalp will be obtained. The grip is superficial and never bulges below the bars of the forceps and depends somewhat on the amount of dilatation present. A weight varying from 1 lb. to 2 lb., hanging over the end of the bed, is applied to the handles by a tape. Nothing further is done until the head is in the vagina, when the forceps are removed and the patient is allowed to deliver herself without further interference. The scalp stretches rapidly at first, and the forceps descend rather quickly, giving the impression that the head is descending as well, so care should be taken not to remove the forceps too soon.

Results.—I have to thank my colleagues, Mr. Comyns Berkeley and Mr. Eardley Holland, for permission to carry out this treatment on patients under their care.

In every case the placenta was felt on ordinary vaginal examination, either marginal or lateral, and this may be taken as an instance requiring active treatment. In no case was the placenta central. In every instance the mother's general condition was noted as good or fairly good. The only adjuvants to delivery were pituitrin when necessary, and a tight binder; in no case were the obstetric forceps used.

Maternal Mortality, 1.—The patient died from postpartum hæmorrhage due to a morbidly adherent placenta.

Fœtal Mortality, 3.—(1) A premature infant weighing 3 lb. 14 oz. (2) Case of a dead macerated fœtus. (3) A fœtus dying during delivery. The duration of traction varied from forty minutes to fifty-three hours, at which time the forceps pulled off. The average for the seven cases was twelve hours.

Puerperium.—The puerperium was afebrile in five cases, and febrile to a slight degree in two cases. In neither instance was the sepsis severe.

Damage to scalp varies with the duration of traction. With five to six hours' traction very little damage is done and the puncture wounds heal readily. After

prolonged traction the stretching of the scalp may cause the formation of a hæmatoma. In one instance the hæmatoma was opened on the mistaken diagnosis of abscess and took two months to heal. In the instance in which the forceps tore off, the resulting ulcer healed in about a month. The Sister-in-charge of the Infant Welfare Centre tells me that in no instance has any permanent damage been done.

Conclusion.—On such a meagre number as seven cases, no definite conclusions can be drawn, and my chief object in bringing this paper to the notice of the Section is the hope that others may be tempted to try this method. Personally, I am hopeful that it will diminish the foetal mortality, as the statistics quoted suggest, without endangering the mothers.

Dr. Fyans and Dr. Gibberd, the two resident medical officers under whose immediate care these patients have been, both assure me that this method is both easy and quick of application, and that it has never failed to check the bleeding immediately and completely. For these reasons this method seems suitable for extern and domiciliary practice, or even in hospital when, with the hæmorrhage controlled in a cleanly manner, an unhurried decision as to the best method of delivery may be given. If one may substitute version for compression this method fulfils those precepts laid down by the late Dr. Herman for the treatment of placenta prævia, viz., early compression, slow extraction and antiseptics—and I would add one more precept taught me by Sir Francis Champneys, namely—gentleness.

TABLE OF CASES.

		Duration of traction	Result	Remarks
A. K., 2-gravida, 36-37 weeks	Stated to have lost one pint before admission. Condition good. Size of os not stated. Placenta marginal	2 hrs. (2 lb.)	M. F. (G. G.)	Forceps slipped off after two hours. Tight binder applied. Pituitrin. Puerperium afebrile
K., 2-gravida, 35 weeks	Frequent hæmorrhage during pregnancy. Slight bleeding preceding admission. Prolapsed, pulseless cord—head soft. Condition good. Placenta lateral, os = two fingers	4 hrs. (1 lb.)	G. D. B.	Forceps cut through the macerated scalp and were replaced by ring type of forceps. Puerperium afebrile
M. M., 3-gravida, 34-35 weeks	Frequent hæmorrhage during pregnancy. Loss $1\frac{1}{2}$ pints before admission. Condition fair. Pulse 100. Placenta marginal. Os = one finger	40 mins. (1 lb.)	G. D.	Infant 3 lb. 14 oz., died twenty minutes after birth. Slight fever during first four days
E. L. B., 1-gravida, 37 weeks	First hæmorrhage controlled by plugging vagina before admission. Condition good. Placenta central with one-finger os, later marginal. Forceps attached to scalp through placenta	53 hrs. (1 lb.)	M. F. (G. G.)	Poor contractions throughout. Forceps cut out, but head being down were not re-applied. Puerperium afebrile
M. G., 3-gravida, 37 weeks	Slight bleeding on admission. Condition good. Os = one finger. Placenta lateral	12 hrs. (2 lb.)	D. G.	Forceps cut through when head in vagina and were not re-applied. Patient died from P. P. H. = morbidly adherent placenta. Infant's scalp incised for hæmatoma
M.A.M., 7-gravida, 37 weeks	Slight bleeding on three occasions before admission. Sudden severe bleeding while in bed in hospital. Os = one finger. Placenta marginal	10 hrs. (1½ lb.)	G. D.	The hæmorrhage was so severe as to cause symptoms of shock in ten minutes. It ceased immediately. Traction was applied. Puerperium afebrile

94 Willett: *Placenta Prævia*; Fitzwilliams: *Secretion of Milk*

Discussion.—Mr. CLIFFORD WHITE asked if Dr. Willett had ever noticed any injury to the maternal soft parts from the pressure of the forceps as they had no pelvic curve. He had once seen a vesico-vaginal fistula follow prolonged weight-traction in a patient with a contraction ring causing obstructed labour, but in this case the weight used was one of 5 lb.

Dr. H. RUSSELL ANDREWS (President) said that the method described was extremely ingenious and that he was certain that it would be given a trial by others. He did not place much value on the statistics that had been quoted, as cases that required no treatment beyond application of a binder or rupturing the membranes were cases in which the pains were good, and the results would be expected to be satisfactory. He thought Dr. Willett's method of treatment would be welcomed, as it involved the minimum of interference and the minimum of bleeding. The severe bleeding which might occur during the performance of bipolar version and after the expulsion of Champetier de Ribes' bag were serious disadvantages connected with these two methods. He thought that severe hæmorrhage after expulsion of the bag could usually be avoided if the head was pushed down firmly from above as the bag descended.

Mr. A. C. PALMER said that Dr. Willett had made a simple yet ingenious contribution to the treatment of placenta prævia. It appeared that there was a large group of cases in which Dr. Willett's method could be used with great advantage to the mother. One very important point was that weight traction readily applied pressure in the right place without disturbing the presenting part or the placenta.

Dr. WILLETT (in reply) said that his object in quoting statistics was to show the high mortality following treatment by version or bag, otherwise he considered them valueless. As regarded the shape of the forceps, there might be an advantage in making them with a pelvic curve, but the present pattern was easy to apply, and up to the present he had not seen any injurious effects on the maternal soft parts from their prolonged use. A pulley might be an advantage in diminishing friction, but as 2 lb. was the greatest weight found to be necessary he did not think that the pulley was of much importance. He quite agreed that the pull on the fetal head must be very small indeed. There had not been any difficulty in reaching the scalp; the fetal head could always be pushed down by pressure from above. He disliked plugging the vagina and thought that it increased the risk of subsequent sepsis. He quite agreed that version might have been necessary in really bad cases, but at the same time he thought that fetal lives had been lost by this treatment in less severe cases. There had not been any instance of central placenta prævia in the series, but in one case in which the placenta was over the os at the beginning of treatment the forceps had been passed through it and the scalp fixed without difficulty. He believed that the rapid stretching of the scalp exercised the pressure when the os was small. He was quite aware that the number of cases were too few for information in details, but he hoped that he had established his point that traction in the forecoming head was sound in principle.

Anomalies in Connexion with Secretion of Milk.

By DUNCAN C. L. FITZWILLIAMS, C.M.G., F.R.C.S.
(Introduced by CLIFFORD WHITE, F.R.C.S.)

MILK IN THE INFANTILE BREAST.

IT is the experience of many who attend confinements that soon after birth the breasts of the infant swell up and that from them can be expressed a fluid of the quality of milk. This is found probably in about 75 per cent. of the infants and is quite irrespective of the sex of the child. The explanation usually given for this phenomenon is Halban's [2] and is that the hormones which are circulating in the mother's breast at this time, and which stimulate her breasts to secrete milk, are to some extent circulating also in the breasts of the infant, with the same effect. Though this is usually quite a transient condition and passes off in a few days, it sometimes becomes excessive, as in the following case:—

A male infant 4 weeks old was brought to St. Pancras Royal Dispensary, with the presence of milk in both breasts, in such quantity that it required to be got rid of daily by gently pressing the glands towards the nipples. The mother seemed to think it was

increasing instead of diminishing. A small quantity of it was examined by Dr. Gibb [1] who found it to possess all the characters of ordinary milk, fat globules being plentiful, and the sugar abundant. It was less watery than other specimens of infants' milk he had examined.

This condition affects other mammals, for it has been reported in foals, calves and lambs.

INFANTILE PRECOCITY.

Apart from this temporary activity found in the infantile breast some children appear to be almost sexually developed when born. These cases are, of course, very rare, a remark which might be applied to most of the anomalies mentioned in this paper.

Malléat [3] recorded a curious case in which a baby was born with well-developed genitals, large breasts; it began to menstruate when fifteen days old.

Wilson [4] saw a case in which an infant was born with breasts the size of hen's eggs, which increased in the course of five months to those of a girl at puberty, by which time she had begun to menstruate; at the age of six years her breasts resembled those of an adult woman.

Wall [5] reported the case of a female infant who menstruated at the age of 9 months and whose breasts were fully developed at the age of 18 months.

Von Lenhossek [6] noted the precocious development of an infant of 10 months; at the end of two years she had well developed breasts. In the black races this development may be more common. Ramon de la Sagra [7] noted

a negress of Havana, with well-developed breasts at birth, in whom the menses appeared soon after birth and were regular at the end of a year. At 2½ years old she was 3 ft. 10 in. tall, and her breasts and sexual organs were as well developed as those of any negress of 13, in whom by that age they are almost fully developed.

AGALACTIA, OR THE ABSENCE OF MILK.

It is quite possible for the breasts to secrete milk which may not be able to flow for some mechanical reason, such as the scar of a burn which has destroyed the nipple, but we are dealing now with cases in which the breast for some reason does not secrete milk.

Harlan [8] reported the case of a mother who had thirteen children but had never secreted a drop of milk. Peuch noted the instance of a young mother twice pregnant without any milk appearing.

O'Flynn [9] noted the case of a healthy woman aged 30, the mother of seven children, who when she came under observation, was pregnant for the eighth time. Her breasts were small and flaccid like those of a girl at puberty, and during her previous pregnancies they had never enlarged nor given milk. Her nipples were prominent but neither of them had any areola. Her mother had had rudimentary breasts, and though she had eleven children, no milk was ever secreted.

These breasts were rudimentary and perhaps malformed, therefore they could not be expected to perform their functions, but agalactia may be present in breasts which enlarge in the usual way and have every appearance of being normal.

Edgerton [10] noted the following example which is worth quoting:—

Mrs. E., aged 25, primipara, whose breasts enlarged during the first month of her pregnancy, though she had never noticed any milk in them, was delivered normally at term, at which time her breasts appeared full and normal. Not a drop of milk or colostrum, however, was ever obtained either by massage, the breast pump, or by putting the child to the breasts; the child was reared by a wet nurse.

Her mother, who was living and had had nine children, Mrs. E. being the eighth, had told her that although her breasts were always normal, she never secreted a drop of milk. Her sister, who was married, had had a child at the age of 25, she, too, never secreted a drop of milk.

James Young [11] related the instance of a whole family who were affected in a peculiar manner:—

A lady had four children but had never had any milk in her left breast, although to all appearance this was perfectly normal and healthy. She had three sisters who all had families, one eight, one six, and the third four children, but none of them had any milk in their left breasts.

GALACTORRHOEA, OR THE EXCESSIVE SECRETION OF MILK.

This curious phenomenon is sometimes met with in women who are or have been nursing, and either secrete an excessive quantity of milk or continue to do so over a very prolonged period. On still rarer occasions the galactorrhœa may be met with quite apart from pregnancy and sometimes even in spite of it. The normal amount of milk secreted by a healthy woman may be said to be about 45 ounces to 2½ pints or 1½ litres. This quantity may be increased in galactorrhœa to quantities like 4 litres in a case reported by Peach, 12½ pints reported by Professor Rémy [12], and even 7 litres from one breast as reported by De Mussey [13]. Pierre Frank [14] defined galactorrhœa "as the flow of milk to such an extent that the powers of the individual are lessened."

This definition ignores both the quantity and the quality of the milk, and the circumstances in which it is secreted. While it is true that most of the cases in which the flow is excessive are very markedly weakened by the constant loss, some people can secrete enormous amounts over lengthened periods without showing any signs of weakness. The above definition is therefore of very little use to us.

Horace Green [15] reported:—

A lady of 47 who suffered from a continuous flow of milk from the breasts for a period of nearly thirty years, during the last nine years of which she was a widow. She suckled two of her grandchildren during the temporary illness of their mother. After the birth of her children, four in number, the menses did not return for two years, they had the effect of increasing the flow for the time being. To the end she was in excellent health.

In most cases however the patient is reduced to a very lamentable condition by the loss. De Sinety [16] reported:—

A woman of 35 who had had three normal pregnancies with abundant milk so that she could feed two children at the same time. The first lactation lasted thirty-four months, the second thirty months. The last delivery was eighteen months ago; she did not nurse the child but the milk flowed spontaneously and abundantly, and she got thinner and thinner.

De Mussey reported a similar but more severe case:—

M. M., aged 20, delivered at term, had too much milk for her own child and gave milk to the children of the neighbourhood; later the milk flowed without suction. Abscesses developed and healed, the left breast recovered and the milk from it ceased. The right breast continued to develop and when she stopped suckling, milk still flowed to the amount of seven litres a day. She became more and more feeble in spite of all forms of treatment, by medicines, baths, and diets. She kept the breast, which was very pendulous, wrapped up in a cloth; it was bathed in milk, and the skin of the breast, abdomen and thigh was ulcerated from the continual soaking. Eventually she recovered.

This quantity of seven litres from one breast is the largest flow of which I can find any record.

Dr. R. A. Gibbons [17] reported a rather similar case.

A lady of 23 suckled her first child for five months, then developed abscesses of both breasts. The second child she nursed from both breasts at first, then only from the left, as the milk ceased from the right breast. After four months she gave up nursing altogether, but the milk continued to flow in spite of all forms of treatment. Drugs, diet, electricity, were alike useless to stop the flow. Her clothes were always saturated with milk though several napkins were used to soak it up. Eczema of the breast and thighs ensued from the constant wetting. Eventually she wore a belt with a circular shield over the breast and a tube passing to a receptacle. In this way the amount secreted could be measured: it varied from 20 to 30 ounces a day, and was of good quality. Menstruation started eleven months after the birth of the child and the flow disappeared in two months' time.

For long periods over which there has been a continuous flow of milk the two following cases may be quoted:—

Gomes Pamo [18] mentions: "A young woman married at 16 years of age, and her menses, which had been established at 14, continued without interruption until the first month of marriage, when she became pregnant. After delivery, lactation continued for twelve months without the appearance of the menses. Becoming again pregnant, she weaned her child, and this occurred fourteen times without any complication.

"She nursed each one of her fourteen children up to the time that she found herself again pregnant. During her pregnancy the flow of milk diminished somewhat, but never disappeared entirely. Immediately after delivery she gave the breast to the infant; the milk was of good quality and abundant, and during all this time, that is, from the first month of marriage to the present time, seven years after the birth of the last child, the menses have not reappeared. She weaned her last child five years since, but the flow of milk has not diminished in spite of all treatment . . . and the breast has to be drawn frequently to relieve the pain caused by tension."

But this is surpassed in length of time by Judith Waterford, who had a flow of milk continuously for forty-seven years:—

No infant, however vigorous, was ever nearly able to use her supply of milk, and in consequence she often had her breasts "drawn" to the amount of two quarts in a day. Her belief is, that in general she could have suckled four lively children at the same time. Many attempts were made to suspend the secretion, even under medical direction. They all failed. After her seventy-fifth year her health became variable; the milk came back repeatedly after this time, and since her eightieth year the secretion has been constant, and she thinks would be sufficient to nourish an infant, but fears that if she took an infant "it would soon be the end of her." In 1831, being in the middle of her eighty-first year, she filled a teaspoonful readily by squeezing her left breast. This milk was nice, sweet, and not different from that of young and healthy mothers. (Dr. K.'s case quoted by Stack.)

In the "*Ephémérides des Curieux de la Nature*," 2nd ed., xi, p. 99, mention is made of a woman who was pregnant. Since her fifth month she was incommoded by a flux of milk which amounted to one and a half pounds in the day. She was bled three times, banded, dieted and exercised and the flow reduced to half a pound in the day. She was delivered at full time of a vigorous child.

Very numerous references are to be found regarding galactorrhœa.

We can only suppose that the cause of galactorrhœa must be the exact opposite of the cause of agalactia, but we really know nothing of the cause of either.

SECRETION OF MILK AT THE EXTREMES OF AGE.

The secretion of fluid in the breasts of infants immediately after birth, and the early development of the breasts has already been noted. We deal now with secretion of milk in sufficient quantities to support life. We must suppose that the child of nine, reported above by Dodd [20], could have suckled the child she bore, at all events for some time if it had not died. Murat and Patissier [21] mention a youthful wet nurse.

A negress of Alençon, eight years old, often applied to her breast the mouth of an infant which was nourished by her mother. She developed such a flow of milk that she was able to nurse the child herself for a month, when the mother on account of cracks in her nipples was no longer able to do so. Several witnesses in the town testified to these facts. She was presented to the Academy of Surgery in 1783, and milk of an excellent quality was expressed in jets from her breasts.

Old people may easily resume the function of suckling children long after the usual period of life. Stack [19] reported an old woman of 68

"whose breasts were full, fair and void of wrinkles, though her face is very much withered, her cheeks and mouth vastly sunken in, her eyes red and running with a clammy humour, and in short showing every sign of old age. Her name was Elizabeth Brian.

"About four years since, her daughter, being obliged to leave an infant she then gave suck to in the care of this, her own mother . . . the old woman, finding the child froward for want

of the breast, applied it to her own, barely in order to quiet the infant, without the least thought of milk, and this having been reiterated several times, a son of hers, by that time a grown man, perceived that the child seemed to swallow somewhat from the nipple, whereupon he begged leave of his mother to try if she had not milk. . . . The youth drew milk from the same breast from which he had been weaned twenty-nine years before and which had been unaccustomed to any for seventeen or eighteen years before. The good woman then continued to suckle her grandchild in earnest."

At the end of two years her daughter produced another child :—

"Whereupon the grandmother weaned the first and suckled this latter, which she had done for the last two years and still continues to do, and this infant in my presence took the nipple with as much eagerness and seeming delight as I ever perceived a child of two years old . . ." Her milk abounded to that degree in the breasts that, to convince the unbelievers, she would frequently spout it above a yard from her. "The poor woman seems perfectly honest and artless, and even inclines strongly to dotage."

Montégre [22] mentions another case :—

A delicate female had twins, but from great weakness of her constitution she could hardly supply one of them with milk, and from poverty could not engage a wet nurse. Her mother took one of the infants and put it to her own breasts; her breasts yielded a fluid in small quantities, but in a few days her infantine grandchild drew from them an abundance of healthy milk. She continued to nurse the child twenty-two months, and he was more vigorous than his brother who was fed by the mother.

Many such cases could be quoted; Judith Waterford, for example, already mentioned, gave milk in her eighty-first year.

SECRETION OF MILK APART FROM PREGNANCY.

James Oliver [23] wrote an interesting article in the *Edinburgh Medical Journal* on the secretion of milk apart from pregnancy, and stated that in the married state it was frequently met with in those who had never been pregnant. It is very important to recognize this, as serious verdicts have depended on its recognition. It is related by Birkett [24] that :—

A woman was brought to justice on the suspicion of having murdered her child which could not be found, the only evidence being that her breasts contained milk, though it was twelve months since she weaned her last child. Heister, the judge, wisely stated: "That nothing could be determined with certainty, for it frequently happened in unmarried and married women, nay, sometimes in old women; and, what was more remarkable, milk had been frequently observed in the breasts of men and children; and this therefore might happen to this woman without her being pregnant, particularly as her menses had been obstructed the whole time."

John Dix [25] reported two cases illustrating this condition :—

A girl, aged 25, from whose nipples well-formed milk could be at any time pressed out. She was anemic and of a highly nervous and hysterical temperament. There was no reason for suspecting her to be otherwise than strictly chaste, nor were there any indications of organic disease. . . . She was under observation for two years, during the whole of which time the secretion continued, often escaping spontaneously in a sufficient quantity to wet her linen.

The second was that of a prostitute, aged 20, who had never been pregnant. With her, likewise, the secretion was abundant, and the milk, which was examined under a microscope, appeared well formed. She was under observation for some months.

Dr. Elliotson [26] says :—

"I myself saw two married women with milk in their breasts, one of whom had never been pregnant for upwards of six years, and the milk was increasing at the time I saw her."

He also saw a young lady whom he believed had not been pregnant, but who was apparently the subject of some ovarian trouble as she had not menstruated for five months. Milk oozed copiously from her breasts, and her medical attendant told him that it had been doing so from the left breast for many months.

Knott [27] reported :—

A nursing mother had a slight indisposition and her child was put to sleep with the nurse, and "the attempts of the latter to keep her troublesome charge in somewhat better order by encouraging its instinctive attentions to the mammary glands led to the unexpected and unpleasant result" of the secretion of milk by the nurse. Removal of the stimulus cured the condition, but we may be sure that the nurse was more careful in future.

A good example of abundant milk in a virgin is the following :—

Madame L. embarked on a sailing ship from St. Christopher for France, taking with her a negress slave aged 18, a virgin, and her child of two months who was at the breast of a wet nurse. The ship having put off it was discovered that the wet nurse had been accidentally left behind. "It became necessary to nourish the infant with biscuit, sugar and water." The child disturbed all on board to such an extent by its heartrending cries that the mother put it to the breasts of her negress slave, and from her the infant obtained in a short time sufficient milk to nourish it, and the babe was fed from this source for nearly a year.

THE SUDDEN SUPPRESSION OF MILK.

Milk, like other secretions, may be affected by general diseases which impair the health ; thus it is usually diminished in high fevers. At this time its chemical composition may be altered, but certain diseases may quite suddenly and completely arrest its secretion. Epilepsy is one of these and a very good example was reported by Féré [28] as follows :—

A woman aged 28, who had suffered from epilepsy since the age of 19 years, but who curbed the disease by the use of bromides so that the fits died away, was delivered at term. She suckled her child, for she had abundant milk. She had left off her bromide while nursing, and was suddenly attacked by violent fits and prolonged spasms, after which she lay for some hours in a semi-conscious state. When she came to herself, she found the front of her chemise all wet with milk, but she could not get her breasts to secrete a drop more milk, and the child had to be fed artificially.

It is well known that the secretion of milk is very closely related to the nervous system. In pseudocyesis the breasts may enlarge and even secrete milk, so that it is no surprise to learn that strong impressions on the central nervous system may inhibit the secretion of milk, and, as in the case of epilepsy, fright may have a very potent effect. Smith [29] noted a good example of this :—

A neighbour visited a mother who was nursing her five weeks old baby. Unknown to the mother, she brought with her a dog, which unperceived went under the table. The mother unwittingly trod upon the dog with the usual result, and the mother was so startled and frightened that she had suppression of the secretion of milk and her breasts went quite flabby. Persistence with hot fomentations and good food brought back the secretion but not till twenty-four hours had passed.

REJUVENATION OF THE BREASTS.

Still more extraordinary than the suppression of milk is the occasional rejuvenation of the breasts with the re-awakening of sexual life in very old people. Yet even this has been noted. Le Conte [30] reported the following curious incident :—

Five negresses were suddenly struck with lightning, three were killed outright, and two were rendered unconscious, but recovered. The young one had her menstrual functions completely upset as the result, but the interesting feature of the injury rested with Sarah, an old woman of at least seventy. After recovering slowly from the shock the periods, which had ceased for more than twenty years, were "completely and thus permanently established and after the lapse of more than a year her moons return as regularly as when she was a young woman." Her mammae too underwent an obvious and preternatural enlargement, apparently originating in a sympathetic irritation, emanating from the establishment of the reproductive functions. The author then proceeds to speculate upon the possibility of her becoming pregnant again.

SUDDEN SUPPRESSION OF THE PERIODS WITH THE SECRETION OF MILK.

That nursing mothers do not, as a rule, menstruate is well known ; and as long as the function of producing milk is persisted in, even over long spaces of time and, as we

have seen, in galactorrhœa, the periods are usually suppressed. Among the laity this is regarded as meaning that conception cannot take place, in which, of course, they are mistaken. But the curious observation has been made which is the converse of this, that the menstrual function may suddenly become suppressed and the breasts may, as a consequence, take on the function of producing milk though no pregnancy exists.

Gauthier [31] noted the following case:—

Mlle. B., aged 25, had three sisters, all normal. She commenced to menstruate when 15 but was not regular until she was nearly 20. At 20 the periods were suppressed without known cause for three months. At the age of 25 they were again suppressed from August to November. In June the following year they were suppressed, but this was accompanied by phenomena in the breast. Milk flowed abundantly from them for four or five days and then diminished and disappeared in the intervals. This was repeated till December, when the flow was less and there was a show of blood. In January her periods returned but were accompanied by a flow of milk which continued till the following May. She was twice examined by a gynecologist who said her uterus and ovaries were normal. She was hysterical.

COLOURED AND NOXIOUS SECRETIONS.

Before the days of analysis of milk much attention was paid to its appearance by the older writers. Mention is to be found of milk which in appearance was black, green, red or yellow. These statements though they may at first sound strange are probably quite true. In women about the menopause it is not uncommon to be able to express from the nipples a fluid which is greenish, bluish, or even blackish in colour. It is merely the fluid which collects in the larger ducts and ampullæ; many of the involution cysts which form at this time contain the same coloured fluid. In extreme cases some of the colouring matter may be derived from blood. I have known at least two people who have come to consult me with no other complaint than that of having a leakage of green milk. I also remember a case in which there was an abundant discharge of green fluid on massaging the breast; dissection showed that a small cyst was being emptied by the massage.

A writer signing himself J. D. M. [32], reported in the *British Medical Journal* the following case:—

"A lady, multipara, not pregnant, had a slight secretion of black milk from both breasts, slight pressure causing it to escape. When she first noticed it, it escaped of its own accord in her bath. Later, she underwent an operation for a large tubal abscess."

Rhys Griffiths [33] recorded a case in which green milk was secreted. Halliburton examined the fluid and excluded the possibility of bile being present; he came to the conclusion that the colour was due to bacteria.

Bacillus pyocyaneus, *Bacillus cyanogenes* and *Bacterium syncyanum* can all produce a blue coloration in milk.

Red fluid from a breast may, of course, be due to a blood-stained discharge from a duct papilloma or a duct carcinoma, but apparently simple engorgement prior to the establishment of the secretion of milk may sometimes be met with.

Buckle [34] reported such a case:—

A woman of 26 who had three children and no trouble with the breasts, had noticed blood come from the left nipple. This increased in amount and started to ooze forth after the birth of the child, at which time the same thing was noticed from the right nipple, so that both breasts were always wet with blood, the amount of which was increased on pressing on the breasts. The right breast commenced to secrete blood and milk on the sixth day after the birth, and on the seventh day only milk. In the case of the left nipple, after a week the milk gradually replaced the blood.

This engorgement might possibly be less unexpected in a first pregnancy as in the following instance:—

A young woman pregnant for the first time was being examined by her doctor when he noticed blood upon her chemise. She said that there had been a discharge of blood since the

fifth month. This continued all through the pregnancy, and she had one or two attacks of epistaxis as well. Two days before labour the bleeding stopped but increased directly afterwards. The mother was anxious to nurse her child but only blood came for seven days; then the colour grew lighter, and on the eighth day colostrum appeared, and she was able to nurse. She appeared to be quite healthy (Habergritz) [35].

Reiffel also reported a case in which the red discharge was due to the patient suffering from purpura.

Micrococcus prodigiosus grows with great rapidity in milk which is allowed to stand, and may so redden it by the production of a fuchsin that it may resemble blood. *Bacillus erythrogenes* will bring about the same result.

Viger [57] reported a case which was possibly of this origin:—

The milk evacuated by the breast of his patient, when received upon the napkin, retained its white colour only for a period of half an hour and then changed to a "couleur de rose brillante;" and so fixed was the stain that simple water could not remove it. This was only observed for a period of some three days. The writer enclosed for inspection a portion of the discoloured linen.

Another case which sounds familiar, in which the secretion went red after standing some time, was reported by Perez, as follows:—

E. S., aged 46, had had four children, all of whom she had suckled for a year; the youngest was 15 years old. She noticed that every morning her chemise was wet with a green fluid which came from her left breast. The flow was less during the day; later the flow increased, and after some weeks the liquid used to change its colour and become rose, "et chaque jour de plus en plus foncé." She went to a doctor, who injected iodine, which stopped the flow but caused so much pain that she did not go again. When seen, her breasts appeared normal and equal, the left being the softer. Ten to fifteen drops were easily expressed, the fluid being greenish, like that contained in an ovarian cyst. This fluid quickly got thicker and coagulated, the fluid part became a rose colour and the solid part blackish, like coffee. Microscopically, it contained red blood corpuscles, colostrum corpuscles and lymphocytes, and there were no epithelium or crystals.

In the instance of yellow milk reported, the colour was ascribed to the eating of rhubarb, but may have been due to the growth either of *Sarcina lutea* or *Bacterium synxanthum*. The presence of actual pus from a breast abscess should be kept in mind.

It is well recognized that many drugs taken by the mother can be excreted in the milk, and will produce their action on the infant. The best known of these are morphia, strychnine, mercury and quinine. Symptoms of both lead and arsenic poisoning are said to have been produced in infants, the substance having been ingested with the milk of the mother.

The next case reported is the one in which the poison of a rattlesnake was supposed to lie dormant in the mother's breast and to have poisoned everything put to the breast until the first milk passed off. Imagination, however, seems to have been active in the report of the case:—

In the summer of 1801, Madame — was bitten by a rattlesnake, but recovered; she was then four to five months pregnant. Her child was born healthy, but on being put to the breast "became the colour of a snake," and swelled up and died. The milk was suspected, and two days later a puppy was put to the breast, and immediately died, then a lamb was applied with the same result. Another puppy and three other animals were tried in turn, but all died. At last a puppy which was applied lived. The mother continued well and at her next pregnancy suckled her child without trouble. This was all ascribed to the previous bite of the rattlesnake.

Curious cases scattered throughout the literature make it apparent that organisms may perhaps gain access to the milk in the breasts through the mouths of the ducts. They can exist in, and produce their action on, the milk in the larger ducts and

reservoirs. Some of the cases in which the milk changed colour after being shed are possibly due to this; the following horrible case may be quoted in this way:—

Madame V. was delivered in October and thereafter suckled her child. Early in December she had occasion to leave the child and go a journey consisting of one hour in a carriage and five and a half hours walking, and one hour's rest; so she was away from the infant for seven and a half hours. On her return she gave her breast to the infant, when there came a most horrible odour. It filled the room, and the husband and friends had to leave holding their noses. The stench was intolerable, like rotten eggs, but acrid. It made the mother ill and the child was violently sick. The doctor saw the family next day and heard the history, and could still smell the clothes upon which the child had vomited. The breasts were normal, no fissure, and now the milk was again quite wholesome. The woman said that several times if she withheld her milk it began to putrefy in like manner, but never as badly as in this instance as she had never withheld it so long (Jorissenne) [36].

We know that the milk may contain organisms which have been excreted by the breast, the best example of this being the tubercle bacillus. Cows suffering from tuberculosis, though they have no focus in connexion with the udder, may give milk which contains active tuberculous organisms and so spread the disease. *Tabes mesenterica* in infants is attributed to this cause. In consequence of this the inspection of cows has of late become much more strict. In the same way the bacillus of typhoid fever may be spread by milk from a typhoid-infected mother to her infant. There is no doubt, too, that the ordinary pyogenic organisms are also excreted by the same channel.

VICARIOUS MENSTRUATION FROM THE BREAST.

In classical writings reference can be found to menstruation from the breasts and the records of accredited observers leave no doubt that such may take place.

Ambrose Paré recorded that the wife of Pierre de Feure menstruated such quantities from the breast each month, that several serviettes were used to receive the discharge.

Sir Astley Cooper [37] noted: "A young lady, aged 17, had a bruised appearance of the breast. There is one large and several smaller extravasations like those which leeches, had they been applied, would have produced under the skin. This extravasation of blood, to which she repeatedly became liable, begins about a week before and disappears a week after menstruation."

Laycock states that blood sometimes escapes *guttatim* from the nipple at the period, and that this may be accompanied by signs of venous congestion in the breasts, and even ecchymosis extending down the arm to the fingers, which assume a reddish-blue and mottled appearance.

Wiltshire [38] notes:—

"One clear and striking case arose in a woman who, during suppression of menstruation by the normal channel, bled at the catamenial epochs from both nipples."

Barnes [39] saw a girl who had vicarious menstruation in the shape of vomiting and also of oozing of blood from the nipples.

At a meeting of the Cambridge Medical Society in 1882, Stear reported the following case:—

A woman of 50, married many years, but never pregnant, menstruated in the ordinary way regularly till two years ago, when the periods stopped. For the last twelve months she had had a monthly discharge from the nipples; the breasts at this time became painful as at the periods. The flow was such as to oblige her to wear a napkin. The phenomenon was witnessed by the narrator.

Hoshkevitch [40] noted

a patient, aged 42, who had suffered from metrorrhagia, due to a polyp of the uterus, for seven years. After removal of the polyp the periods ceased and there began a discharge of blood from the right nipple lasting several days each month.

Tuefard [41] saw another case:—

A lady, aged 56, who had regular monthly bleedings from the nipples lasting eight days, the breasts at this time being large, tense and painful; in the intervals they again subsided. This condition had only appeared after the menopause.

It is possible that many of these cases were not strictly speaking cases of menstruation from the breast. It may be that there was some pathological condition of the breast, such as a duct papilloma or a duct cancer, which became over-engorged at the period, and blood was discharged from the nipple. It is curious, however, that no explanation was ever produced, though the patients were in many cases under the observation of their medical practitioner for long periods as curiosities.

Dr. Bultner [42] noted:—

A young lady affected with hysterics who had a menstrual discharge several times from the breasts. This at last became habitual every month; for four or five days she lost about 3 oz. of blood. Towards the end the discharge became white and mucilaginous; the breasts were swollen and painful at the time.

In another case published in German medical annals of 1822, in addition to there being bloody discharge from the breasts, there was also a similar secretion from the axillæ.

Baker [43] reported very fully the case of a woman of 20, who menstruated freely from a persistent sore of the right breast which originated in a wound received when she was menstruating. He gives two excellent coloured plates illustrating the condition.

Other cases have been reported by Foulger, Jacobson, Hancock and Roxeau, but I have not been able to see the original papers.

Perhaps the best example of the condition is given by Hirschberg [44] as follows:—

A healthy woman who commenced to menstruate at 11, and whose periods were normal and regular, at the age of 15 began to have a watery discharge from the nipples at each period. At 17 this became bloody, and for the next ten years, for two days before, till six or seven days after each period, there was a bloody discharge from the breasts, more profuse from the right one. She had two miscarriages, and during the pregnancies the bleeding both from the uterus and the breasts ceased, but returned after the first miscarriage, which was at the fourth month. After the second miscarriage the bleeding from the breast never returned.

EXPERIMENTS IN CONNEXION WITH THE SECRETION OF MILK.

Many interesting experiments have been carried out on animals with regard to milk function. Hammond and Marshall [45] had doe rabbits covered by sterilized bucks, after this they showed signs of a sort of pseudocyesis. They made nests preparatory to having their young and they even secreted milk. It was noted that a cat four weeks after an infertile coitus began to secrete milk and continued to do so for two weeks.

The same thing sometimes occurs in the human being in pseudocyesis, for Engström [46] mentions the case of

Mme. Th., aged 28, married, but with whom cohabitation was impossible owing to a vulval abscess. Though she did not want to have children she developed the typical state of mind of a condition of pseudocyesis, and her breasts produced milk. She was an erotic and nervous woman.

Lane-Claypon and Starling [47] caused development and secretion from the mammary glands of a virgin rabbit by the injection of the extract of dried fœtuses.

Actual experiments on the nerve supply of the gland give interesting but inconclusive results.

Roehrig [48] operated upon curarized goats and divided the external spermatic or pudic nerve which supplies their glands. The amount secreted was at once diminished. On stimulation of the peripheral end the flow ceased. He came to the conclusion that the milk flow depended far more upon the vasomotor changes in the gland than upon direct secretory influence, and that a rise in the blood-pressure was accompanied by milk flow. Increased flow occurred after strychnine, caffeine, and digitalin were given, all of which increased blood-pressure, and this after the nerves had been divided. On the other hand, drugs which diminished blood-pressure also diminished secretion; the most powerful of these drugs were chloral hydrate and atropine; potassium bromide was less powerful.

De Sinety's [16] experiments help us but little. He divided the nerves which accompany the vessels to the glands in the guinea-pig, without any result upon the flow of milk. Neither did stimulation of the nerve yield any result. He thought, however, that the nerve contained fibres of an excessive sensibility, a conclusion which Eckhart also arrived at [49].

We therefore see that secretion of milk may be brought about by the central nervous system in pseudocyesis, by a hormone, from the injection of extract of fœtus, and by raising of the blood-pressure. There is, however, another and a most powerful factor which may be brought into play, and that is reflex in its action. For the sucking, titillation or stimulation of the nipples will very rapidly bring about the secretion of milk in the lower mammals, in woman, of whatever age, and even in the male sex, both in animals and, strange as it may sound, also in man. Examples of these different conditions may now be given. Hervey de Chégoni [50] mentions that a young goat which had never been covered was brought to milk by being sucked continually by a kid and after several days the kid had enough milk to nourish it. Legroux [51] noted the same phenomenon in the case of a bitch sucked by a puppy. Other stimulants to the area would apparently do as well if there was enough sting in them, for we read that nettles were in common use among the goat-herds of Mount Oetas for rubbing the udders of goats in order to make the milk appear.

Cardani [52] mentions the instance of a young woman who had never been pregnant, whose breasts yielded milk on their being excited with the stings of nettles.

One of the best examples of suction stimulating secretion in an unexpected quarter has already been quoted in the case of the young negress of Alençon who was doubtless proud of her achievement. Less pleasure was expressed by the nurse mentioned by Knott.

Quite a large number of such cases could be quoted. Of male animals mention may be made of a bull which was put successfully to cows and also had rudimentary female organs of generation; according "to satisfactory testimony" this animal gave milk. There is the importunate lamb which belonged to Sir William Lowther [56] which had lost its dam in early life and persistently sucked a wether which grazed

in its company. The lamb obtained its milk "and was maintained by him all the summer." Milk was still present a month after the lamb was weaned. As the male has rudimentary mammary glands, it is only fair to point out that these glands sometimes take on the function of producing milk without the stimulus of suckling. Aristotle made famous a he-goat of Lemnos; this generous animal yielded a profusion of milk from which the goat-herd made excellent cheese.

Blumenbach [53] noted another goat which, on account of the engorgement of the mammae, it was necessary to milk every other day. Lastly, St. Hilaire [54] kept for many years in the Jardin des Plantes at Paris a large-uddered he-goat which gave milk freely. The question of men suckling infants is a subject which is too great to be enlarged upon here. I will therefore only mention one example, that of the Zulu Chief, Chengwayo, reported by Schufeldt [55], who shared with his forty wives the pleasures of nursing his 100 children. Among the white races the same thing has been reported from time to time.

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Dr. R. A. GIBBONS said that as Mr. Fitzwilliams had mentioned the case of unilateral galactorrhœa which he (Dr. Gibbons) had brought before the Obstetrical Society, he might say that the patient seemed to evince an abnormal tendency to secrete milk, for in a previous confinement she had nursed another infant for three months besides her own. One of the points of interest relating to the case was that the amount of milk averaged only 20 to 30 oz. in the twenty-four hours, and that the milk was not watery, but normal in all respects. Another point of interest was that none of the remedies ordered—drugs, diet, compression, galvanism, &c., did any good, but that when the menstrual period returned, eleven months after the birth of

the child, the amount of milk gradually diminished, and the flow spontaneously ceased after the second period.

Mr. Fitzwilliams had mentioned two matters of much interest concerning infants, one, that of swelling and discharge of milk from the breast, and the other, that of infantile menstruation. With reference to the former, he (Dr. Gibbons) had had a fair number of cases occurring in infants he had brought into the world. In some the swelling went on to suppuration, and in others milk appeared. He treated them on Bier's plan by induced hyperæmia directly he saw signs of swelling. With regard to the latter, he had seen cases of sanguineous discharge considered by the mother to be menstruation, but on carefully following up the progress of the infants, menstruation was proved to be absent. He had, however, seen a very few cases of precocious menstruation, which was undoubtedly extremely rare. He thought it right to say that one should be careful in accepting the statement of the mother or the nurse regarding genuine menstruation during infantile life.

Section of Odontology.

President—Mr. J. G. TURNER, F.R.C.S., L.D.S.Eng.

PRESIDENT'S ADDRESS:

By J. G. TURNER, F.R.C.S., L.D.S.Eng.

LET me first express my appreciation of the honour of being your President for the ensuing year. Our Section of Odontology has a long and distinguished history, and the list of past Presidents goes far in explaining the pre-eminent position it holds in the domain of dental science. I shall do my best worthily to uphold our proud traditions.

Now a presidential address is by custom above criticism—or at least immediate discussion. I have, therefore, left for other times purely professional subjects which would provoke, and should be subjected to, discussion. I have, rather, bethought me that I would take advantage of my momentary immunity, to bring forward two questions, not purely professional but yet, I think, of great professional interest.

My first point relates to the use of English. We are adding yearly to what should be a most valuable history of the evolution of the science of dentistry. Our printed records should be unequivocal statements of what was meant, such as students fifty years hence may read and understand, even as they run. In reading three of our recent contributions, I noted six passages which left me wondering what the author meant. If this is the case now when we are in touch with each other's thoughts, what will be the value of these records to the student of fifty years hence? Putting it plainly and sheltering myself behind my temporary immunity from retort, courteous or other, I am asking for less slipshod English. A dear old lady who used to "do for me" when I went a-fishing, used to say, when displeased with me, "Now, Mr. Turner, you did'an hadn't ought-er do it." Some of you may think the same now; but if you do, the meaning is at least clear, and I would rather such illiterate English than polite English showing a confusion of thought.

Now, while some of you may be thinking you have chosen a pedant for a President, I am going to pass on to another subject which reveals in me that proverbial pedant, the pedagogue.

We, as a scientific society, are heaping up knowledge and we know not, and almost seem to care not, who gathers it.

You may wonder what I mean. I am going to suggest that a scientific body such as ours, with all the best thinkers of the profession at its disposal, might do yet more for the common weal than it already does. We seek truth in the brave hope that some day it may be of use in the fight against disease. But why not ensue it? That is, direct it to the best practical use?

At the present moment unauthorized practice of dentistry is banned, and the

dental schools ought to be overflowing with students to take the places of those of the (?) 20,000 registered dentists who fall by the wayside. But, in fact, they are not; and if the dental schools are unable to supply qualified practitioners to meet the demand for dental services, then it takes little foresight to prophesy a return of the unqualified practitioner in but a few years' time, or at the least an urgent demand for a reduction of the curriculum. The difficulty lies in the cost in time and money of the necessary training. The possibilities of dental practice among the multitude are not such as to appeal to the highly trained man with capital to help him. The expense of education is too great for those who would be content with such possibilities. The sons of small tradesmen, for instance, might well find in such practice a more favourable opening than in the parental calling. I suggest that this Society might concern itself with an attempt to evolve a course of instruction less costly both in time and money than the present curriculum, yet capable of turning out an efficient dental practitioner. Those among us who are teachers may not be the best qualified to elaborate such a course; we are always asking for more time to impart ever-accumulating knowledge. But we have men among us of wide experience in all classes of dental practice, and with their help we might achieve a code or index of our present knowledge which would begin with essential knowledge and gradually work down to hypothesis. By selecting, then, from each subject the items tabulated as essential knowledge we might, and I think we should, arrive at a useful curriculum considerably less costly than we have at present. This is what I mean by ensuring our knowledge.

To make such a curriculum as I am imagining useful, some curtailment of the preliminary examination would probably be needed. This should be done not by lowering the standard, but by lessening the number of compulsory subjects. From the teacher's point of view the essential subject is English. A student with a good knowledge of the English language is teachable; those who cannot make intelligent use of language had better at once be debarred from entering a scientific profession.

Some Points in the Pathology, Diagnosis and Treatment of Diseases of the Maxillary Antrum.

By E. D. D. DAVIS, F.R.C.S.

¶THERE are certain details in the surgical or applied anatomy of the maxillary antrum which are worthy of consideration. The antrum, as we know, is a pyramidal-shaped cavity with the apex of the pyramid directed to the malar bone and called the malar recess, and the base of the pyramid forms the outer wall of the nose; but when the question of drainage arises, which is the object of the great majority of antral operations, it is not such a simple cavity as it appears, and there are recesses and depressions in which pools of pus collect, and are difficult to drain.

An examination of the specimens exhibited will show that there is one such depression in the anterior portion of the antrum which lies well below the floor of the nose and cannot adequately be drained, when the patient is erect, by the usual large permanent opening made into the nose; but fortunately when the patient is lying down at night, and if the ciliated epithelial lining of the antrum is not badly damaged, drainage is good. Similar difficulties arise in connexion with the malar and pre-lacrymal recesses, and the cavity may be divided almost into two compartments by a steep ridge crossing its floor. The lining of the antrum is a mucoperiosteum with a ciliated epithelium and numerous mucous glands, and when one remembers the action of the cilia in the frog's mouth which wave on particles of food at the rate of 1 in. per minute, it will be seen how important is the preservation

of this mucous membrane to maintain good drainage and a healthy antrum. The destruction of this mucosa leads to prolonged suppuration; and, in addition, it is a periosteum, and for this reason alone curetting and scraping the antrum should be avoided, as it must expose the patient to that dangerous condition of osteomyelitis and periostitis of the maxilla. Moreover, antral suppuration is nearly always limited by this muco-periosteum, and rarely extends to the bony walls except in the very chronic or virulent cases. In speaking of antral suppuration, the term "bone disease" is too loosely applied, because caries or necrosis of bone is rare, so why scrape the antrum except to do more harm than good?

A brief description of the various operations carried out for suppuration of the antrum with the object of establishing drainage is now necessary. There are two types of operation, the radical antral operation known as the Caldwell-Luc and modified by Denker, and secondly the intranasal operations, of which the Canfield operation is an example.

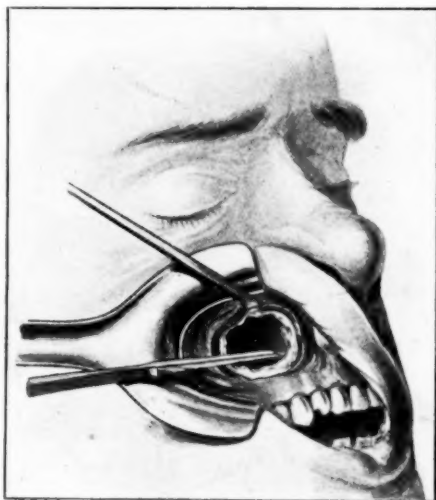


FIG. 1.—The Caldwell-Luc radical antral operation. ("Diseases of the Nose and Throat." By Sir StClair Thomson.)

The radical operation, well illustrated by this picture, and the one most commonly performed and known as the Caldwell-Luc operation, consists of a large opening made in the cheek sulcus and through the canine fossa and also into the nose (fig. 1). A good inspection of the antrum is obtained, particularly if the operation is done with the patient in the sitting position; by this operation it is mechanically easier to remove a portion of the antro-nasal wall to make a large and permanent opening into the inferior meatus of the nose. The opening in the canine fossa is allowed to close immediately, and though it is the most effective drain, for various reasons it cannot be kept open.

Denker's operation is an even more radical operation, in which the nasal process of the maxilla is removed up to the vestibule of the nose. This operation is not often done in this country and is hardly necessary. I must again emphasize the

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importance of preserving the lining mucosa of the antrum, and curetting should be forbidden at all costs. If there are polypi, they should be picked off with forceps, and the cedematous condition of the mucosa will subside if the suppuration is treated by drainage. Packing of the antrum with gauze should also be condemned, because it damages the mucosa, it produces suppuration and quickly adds sepsis, in spite of the fact that it may be soaked in all the antiseptics and gums of a drug shop. It is entirely unnecessary, it delays healing, and is disagreeable to the patient. I need not remind you how quickly a gum mastic dressing in a tooth cavity becomes foul, and you can imagine how much worse it can be in a cavity of the nose. Gauze

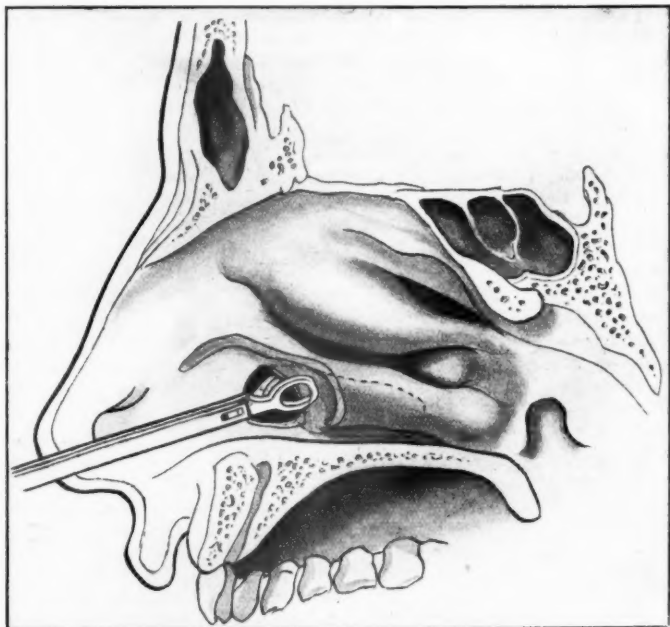


FIG. 2.—The usual intra-nasal operation. ("Diseases of the Nose and Throat."
By Sir StClair Thomson.)

packing in any operation of the mouth and nose should be avoided as if it were poison. If uncontrollable hæmorrhage forces you to pack, the packing should be used as a last resort, and removed at the earliest possible moment.

The intranasal operation is done from within the nostril, and is used more in cases in which the antrum is a cesspool rather than the seat of disease (fig. 2.) It is less severe, but inspection of the antrum cannot easily be carried out, and Canfield has designed an operation by which the bone at the anterior angle is removed and direct inspection can be made and continued through the nostril (fig. 3).

This operation is not so easily performed as its description may imply, because the bone in this position is very thick and dense, and cannot easily be removed through so small an aperture as the nostril.

Finally, the oldest and now least used method of drainage of the antrum is alveolar puncture; and it is one in which you are interested. A spear-headed drill is used to make a hole into the antrum through the socket of one of the buccal roots of the three upper molars, preferably the socket of the buccal root of the first molar. But I have seen two cases in which the second bicuspid was diseased, and an attempt made to drain the antrum through its socket, with the result that the drill did not pass into the antrum at all, but passed in front of it to the middle meatus of the



FIG. 3.—Diagram showing amount of bone removed in the Canfield operation.

(Drawing by Sir J. Dundas-Grant [2].)

nose, so that this operation can be a shot in the dark, particularly if it is hastily done under gas anæsthesia. As another example of this difficulty, I can recall four cases of dental cysts in connexion with the second bicuspid which were opened by alveolar puncture and mistaken for antral suppuration. Later on, in speaking of dental cysts which encroach on the antrum, you will see that it is quite easy to mistake a discharging sinus of the gum in the molar region arising from a large dental cyst for that of antral empyema. Similarly, a mistaken diagnosis has frequently occurred between an antral empyema of nasal origin and that of dental origin; and the former has been unsuccessfully treated by alveolar puncture because nasal antral empyema is practically always secondary to suppuration in the ethmoidal or frontal sinuses and is a cesspool for the nose. Therefore mistaken diagnosis can and does explain some of the failures of alveolar puncture, and this method of treatment does not give the same opportunity of checking the diagnosis as in the other antral operations.

Naturally, you may claim that alveolar puncture is a simple, easy procedure, as it opens into the lowest point of the antrum and should be the best drain. But in actual practice it is disappointing because the mucosa at the antral orifice of the puncture becomes œdematous and forms a ball valve; the cilia of the mucosa also wave towards the upper part of the antrum to its ostium and away from the alveolus; besides, this drain is like the neck of an inverted bottle, and it is stated that re-infection of the antrum occurs from the mouth. It may be said that these are theoretical objections, but the fact remains that alveolar drainage is not successful. I have frequently allowed the alveolar opening to close, and if, after repeated syringing with a trocar passed into the antrum from the nose, the suppuration is not arrested, a radical antral operation has been performed, and it is surprising how many times foreign bodies in the form of cotton-wool pledgets used by the patient to swab out the alveolar opening, bits of rubber plugs, and even drainage tubes, have been found in the antrum. The mucosa around the orifice of the opening is nearly always œdematous and suppurating. Patients are thoroughly "fed up" with the syringing and toilet of the antrum after a few years; and they

ask if the opening cannot be closed and willingly submit to an operation for permanent drainage into the nose. I may say here that syringing of the antrum through the nasal opening after the operation wound has healed is unnecessary, and I have found it is detrimental rather than beneficial.

Now I propose to deal with those conditions of the antrum which are seen in ordinary dental practice. The antrum is occasionally opened by the extraction of one of the upper molars, more commonly the wisdom. This may happen in a careful and skilful extraction, and if a sufficiently large number of molars are extracted it is a complication which will occur sooner or later. After the extraction, when the patient rinses out the mouth, some of the water comes down the nostril as he stoops forward to expectorate, or a little bleeding from the nose takes place. I have seen some twenty or more such cases, and if the antrum is healthy and left entirely alone the opening rapidly closes without further trouble, but if it is syringed, particularly with such lotions as hydrogen peroxide, &c., suppuration is liable to occur. All that is necessary is to keep the mouth clean and the less that is done to the antrum the better. As an analogy, I would recall cases of ruptured drum of the ear which happened during the war as the result of gun fire, in which it was soon observed that if the ear passage was cleaned and a little sterile wool kept in the meatus, the rupture healed quickly without damage, but if the ear was tinkered with by means of syringing, the application of drops, &c., suppuration occurred and healing was delayed.

If a large piece of the alveolus is removed with the tooth, resulting in a large opening into the antrum, the fistula takes some time to close and small plastic operations have been devised to slide a flap of gum over the opening and close it by stitches, but with patience the fistula will ultimately heal. A rarer accident is that of forcing a root of a molar or second bicuspid into the antrum during extraction. In such a case it is important to make sure that the root is actually in the antrum. On two occasions I have been asked to open the antrum to remove such a root, but in each case the root was found underneath the detached gum and between it and the alveolus, in spite of the fact that the X-ray photograph in one case appeared to show the root lying horizontally within the antral cavity. However, roots have been found in the antrum lying horizontally and quite close to the antral opening of the fistula.

In such a case I would advise enlarging the antral fistula, then syringing out the antrum by means of a trocar passed through the middle meatus of the nose, and if this fails to eject the root the antrum must be opened through the canine fossa. In one case a second bicuspid root had been lying in the antrum for four years in spite of the almost constant daily syringing through an alveolar puncture. At the radical antral operation, the root was found lying in a bed of suppurating granulation tissue near the orifice of the alveolar drain, and the rest of the antral mucosa was quite healthy. One or two cases of infection of the antrum have followed a difficult extraction of an upper molar in which there has been severe laceration of the gum and injury to the alveolus with sepsis, but under expectant treatment the infection has quickly subsided.

There are two chief causes of suppuration of the maxillary antrum, and by far the commonest cause lies within the nose and is of nasal origin. Among the accessory sinuses of the nose the antrum is the one most frequently affected by suppuration; it usually takes much longer than any other sinus of the nose to recover and is frequently the residue of a nasal sinusitis. Pneumococcal and streptococcal infections diagnosed as influenzal colds, and chronic nasal obstruction with the consequent retention of mucus and attacks of catarrh, are examples of causes of nasal origin.

The second cause of antral empyema is due to suppuration in connexion with the teeth and is of dental origin. Of 101 cases of antral suppuration I collected

during 1912 and part of 1913, only twelve were of dental origin; the remainder were all due to nasal causes. According to Grünwald's statistics only fourteen out of ninety-eight cases were caused by dental disease. It will at once be asked how can an antral empyema of dental origin be distinguished from that of nasal origin? There are four distinct points of difference, which are as follows:—

- (1) The presence of, or a history of, suppuration in connexion with certain teeth.
- (2) Suppuration of dental origin is limited to the antrum and is usually unilateral, with the absence of other sinus suppuration, and therefore more successfully treated by operation. I have not seen suppuration of the antrum of dental origin extend to the ethmoid and other nasal sinuses. The nasal antrum is practically always associated with other sinus suppuration and is frequently the cesspool or reservoir for frontal and ethmoidal suppuration. Polypi often accompany nasal antral empyema, but I have never seen polypi in antral suppuration of dental origin.
- (3) The dental antrum contains greyish, foul pus smelling strongly of *Bacillus coli*, with a sandy deposit, while pus from a nasal antrum is creamy, stringy muco-pus of a musty odour.
- (4) Skiagrams of the roots of the teeth will sometimes show the extension of disease to the antrum.

If the few specimens I have shown of the floor of the antrum be examined, it will be seen that the sockets of the molar teeth are separated by a layer of bone as thin as paper and perforated by a number of pin holes. The roots project into the antrum and are practically in contact with its mucous membrane, and it is surprising that antral suppuration is not more frequently a complication of an alveolar abscess or of a septic dead tooth.

In my experience, the wisdom or the third molar is most commonly responsible for dental antral empyema, and, if a number of skulls are studied, it will be seen that the root of the wisdom projects further into the antrum than the others. Any of the upper teeth behind the canine may be the cause, but the three molars are the most common, and the first bicuspid the rarest. The lesion is generally a chronic abscess or pyorrhœa in connexion with one of the molars. As we know, an acute alveolar abscess bursts through the thin outer plate of the alveolus and passes under the muco-periosteum to form what is popularly called a gum-boil. In a very small proportion of cases of alveolar abscess, pyorrhœa or chronic sepsis in connexion with one of the buccal roots of a dead molar extends upwards and results in antral suppuration. The acute alveolar abscess tends to burst through the thin outer alveolar plate and the offending tooth is usually extracted without delay; but the more chronic forms of suppuration have a greater opportunity of involving the antrum by direct extension. Its mucosa becomes œdematous, the cilia are destroyed and there is a secretion of clear, straw-coloured serum which rapidly suppurates, forming an empyema which, if allowed to remain untreated, results in permanent damage to the mucosa and perhaps in necrosis of the alveolus.

In children, dental antral suppuration is a rare disease, but severe infection or suppuration of a tooth germ has resulted in an osteomyelitis of the maxilla, a dangerous illness with all the signs of pyæmia, and if the child survives the acute stage, prolonged suppuration and necrosis of bone occur. Repeated abscess formation with shedding of sequestra continues for weeks and these cases have been mistaken for antral suppuration. A similar but more chronic condition has been seen in adults following an alveolar abscess.

I have seen one case of an acute catarrhal inflammation of the antrum as the result of an impacted upper wisdom tooth, in which the antrum caused considerable pain and headache, and contained a turbid straw-coloured fluid. Syringing out the antrum and extraction of the tooth ended in a cure.

In the acute cases of antral suppuration, the patient complains of pain in the upper molar teeth which he cannot localize to any particular tooth, the cheek over

the antrum is tender, there is a temporal headache on the same side with malaise, and perhaps a raised temperature.

In the cases of chronic suppuration, pain and tenderness may be entirely absent and the only complaint is an offensive discharge from the nostril on stooping down.

It is quite frequent in cases of antral empyema of nasal origin for the patient to complain of a diffuse ache in the upper molar teeth which he cannot localize, and to state that his dentist has examined his teeth and cannot find anything amiss. So-called vaso-motor rhinitis, i.e., sudden nasal obstruction with profuse clear discharge and sneezing, particularly if it be unilateral, is sometimes produced by catarrh or suppuration of the antrum. Displacement of the eyeball or lacrimal obstruction never occurs in suppuration and if it is seen it always indicates a new growth, of which I will speak later. The predominant and characteristic symptom is then an offensive discharge from the nose or unpleasant smell on stooping down.

Transillumination is now applied, and it will be seen that the crescents of light below the eye are asymmetrical, i.e., the light is absent or duller on the affected side. It is this want of symmetry which is important, because both sides may be dark or poorly illuminated, and in this case transillumination does not help and it is only useful as an additional sign when there is asymmetry.

On looking into the nose, pus will be seen streaming from the middle meatus below the middle turbinal, which is usually oedematous. It is still more common on looking into the naso-pharynx with a post-nasal mirror to see the pus under the posterior end of the middle turbinal in the posterior nares, and it is often seen in this position when it cannot be observed from the nostril. These signs are present in what is known as the manifest antral empyema, but they may all be absent, and it is then known as a latent empyema. The proof positive is the puncture of the antrum with the trocar passed through the nose, and the character of the pus so obtained gives a good idea of the state of suppuration.

Brown Kelly has used a large trocar passed through the canine fossa in the cheek sulcus and has then inserted an aural speculum to examine the interior of the antrum; but this procedure is too much like an antral operation and a diagnosis can be made without it.

If the antral suppuration of dental origin is recent and of short duration, I have found that extraction of the offending tooth, followed by syringing of the antrum with a trocar passed through the nose once a day for two days and then at longer intervals, using a mild lotion such as boracic or normal saline, frequently results in a cure. If this method fails after six syringings, and in the more chronic cases, the radical antral operation is indicated; but the mucous membrane lining of the cavity must be respected and treated with great care, and only a thorough drainage operation should be done.

It is characteristic of a dental antrum that recovery is complete; the result is excellent and similar to that of a normal antrum which has been operated on as the result of an inaccurate diagnosis. On the other hand, an antral empyema of nasal origin does not heal anything like so quickly, and this feature is a distinguishing sign between dental and nasal antral empyemata.

This paper would not be complete without a brief reference to dental cysts, especially the larger ones which encroach on and involve the antrum. This X-ray photograph of a cyst, into which the finest fuse wire has been passed to make its outline clear (fig. 4), and this specimen from the Onodi collection of the Royal College of Surgeons, illustrate the type of cyst to which I refer.

The patient usually comes complaining of a discharging sinus of the gum in the upper molar region, which has persisted after the extraction of a tooth. On passing a probe into the sinus it is noticed that the cavity is large, and the probe disappears for 1 in. to 1½ in., and it is thought it must be in the antrum. A rounded swelling may be felt in the cheek sulcus and the cheek looks fuller than that of the

opposite side; in addition there is no nasal discharge and nothing to be seen in the nose to indicate antral suppuration. Owing, however, to the direction of the probe in the sinus, this cyst is often mistaken for an antral empyema. In the first case I saw, I made this inaccurate diagnosis, and I have had the consolation of having seen four others fall into the same error: but if the sinus is syringed no lotion passes into the nose, and this is a definite proof that the cavity is not the antrum and is shut off from it.

Out of forty-three cases of dental cysts which I have treated and of which I have the notes, only five were of this type, which nearly obliterated the antrum. Such should be treated by freely opening the cyst in the cheek sulcus and removing the party wall between it and the antrum so as to form one cavity, and finally draining

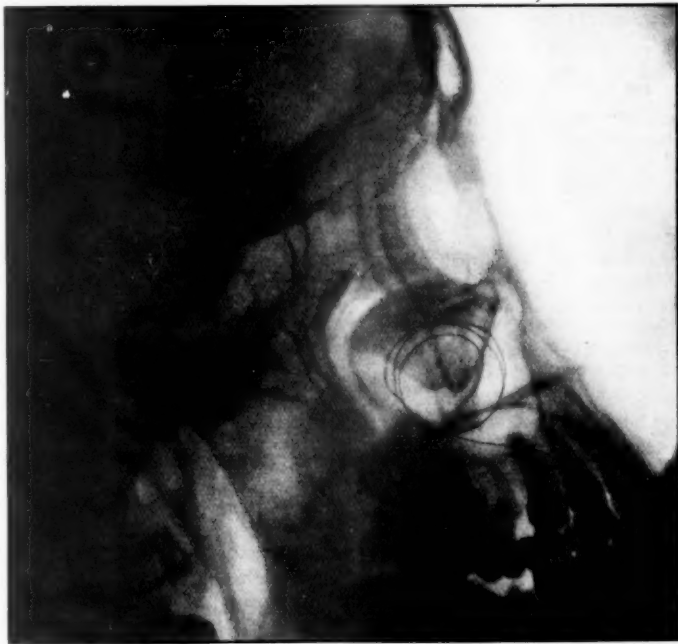


FIG. 4.—Dental cyst with fuse wire passed into it.

this cavity into the nose and closing the wound in the mouth—an operation similar to the Caldwell-Luc method of draining the antrum. Owing to the position of the cyst, it cannot be kept open and the cheek always falls over it, and the wound closes within a few days. The cavity is much too large to obliterate, and if it is not drained into the nose the suppurating sinus remains. In five of the cases in which this operation was carried out healing was rapid and the course was entirely satisfactory. Two of the cases were dentigerous cysts in connexion with the wisdom. On the other hand this X-ray, similarly prepared by fuse wire, illustrates the commoner and smaller type of cysts, which occur in connexion with the teeth in front of the antrum—i.e., the bicuspid, canines and incisors—and which do not encroach

on the antrum or nose to any serious extent. This type of cyst is treated by the removal of the whole of its outer wall so as to create a saucer-shaped cavity with no overhanging edges, and care should be taken not to open the nose. For this reason, the inner portion of the lining membrane of the cyst is left, because injury to the thin party wall between the cyst and the nose may lead to a permanent fistula; but I do not think the leaving of the inner part of the lining membrane makes any appreciable difference to the filling up of the cyst after operation. Packing of the cyst cavity should be avoided, as it delays healing, and if the cyst wall is freely removed it is entirely unnecessary. A mouth wash is, of course, used to keep the wound clean and is all that is required.

Simple growths of the antrum are rare, but one or two cases of malignant growths which involve the antrum are seen every year, and are nearly always squamous-celled carcinomata growing from the ethmoid or floor of the orbit in the region of the infra-orbital canal. Taking the line of least resistance, the growth fills the antral cavity and erodes its walls. I have never seen a squamous carcinoma or round-celled sarcoma originating in the muco-periosteum of the antrum, and the growth is always a soft, friable, suppurating mass which fills the antral cavity.

Occasionally an epithelioma of the palate or cheek will erode into the antrum and the burrowing epithelioma of the gum or "*épithélioma térébrant*" of French authors is most commonly the squamous carcinoma described above, which bursts through the alveolus along the sockets of the molar teeth, this being often facilitated by the extraction of these teeth to relieve pain.

Wendell C. Phillips [3] has collected and recorded in detail sixteen cases which were described as boring or burrowing epitheliomata or "*épithélioma térébrant*," occurring in connexion with a tooth or tooth socket; these were undoubtedly squamous carcinomata which filled the antrum and then burst through the alveolus.

On the other hand, Stanley Colyer [4] has described a true case of burrowing epithelioma which was limited to the alveolus and did not invade the antrum. Another similar case is recorded in Tomes' "Dental Surgery," and, so far as I know, no other cases have been reported, so that the true burrowing epithelioma must be rare. I have not seen a case.

The squamous carcinoma which invades the antrum is very malignant, and first makes its appearance in the middle meatus of the nose; hence one of the early signs is a blood-stained nasal discharge or severe epistaxis, and, upon looking into the nose, a soft vascular polypoid growth is seen which bleeds profusely if touched.

The next point of least resistance is the floor of the orbit, and the growth soon extends to the eyeball: in fact it may commence in this wall of the antrum, and is shown by proptosis and upward displacement of the eyeball. Pressure on the nasal duct, resulting in epiphora and lacrymal obstruction, is a very late sign and is often absent. Another late sign is the erosion of the facial wall of the antrum immediately below the infra-orbital ridge, producing œdema of the cheek and invading the soft tissues so as to make the complete removal of the growth almost impracticable. The palate and alveolus are the last to be involved, and in the operable case the palate has been saved and the recurrence of the growth has not occurred in this portion of the maxilla.

The commonest first symptom is a persistent pain in the cheek or teeth, and again toothache is diffuse and cannot be localized. The pain radiates to the temporal region and forehead, and is accompanied by a blood-stained discharge from one nostril. The teeth are frequently held accountable for the pain and have been extracted, but a blood-stained discharge from one nostril with pain in the cheek in an elderly patient should raise suspicions of a growth.

Unfortunately, early diagnosis is rendered difficult by the fact that cancer in

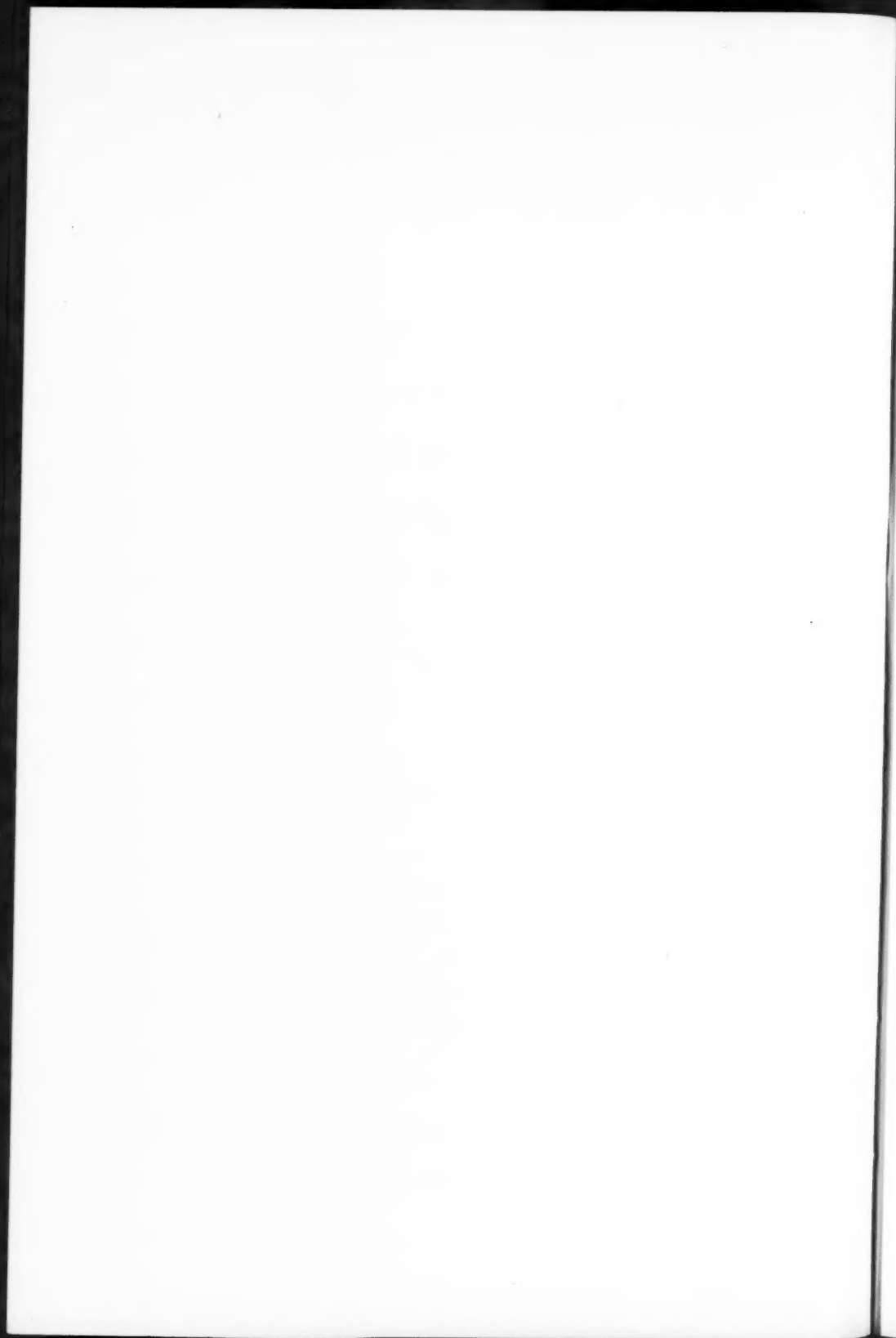
itself does not produce any specific clinical signs or symptoms, and it is only when the cancer cells form a tumour that a series of mechanical results appear. In addition the development of these is slow and insidious, particularly when the growth is in a hidden cavity like the nose and a patient only consults a doctor when such symptoms are well marked, frequently too late for the successful removal of the growth. In spite of this difficulty, a number of cases have been seen early enough to permit of excision of the growth, and the orthodox anatomical excision of the upper jaw has been discarded because the removal of the palate is unnecessary and the extensions of the growth into the ethmoid and orbit were left behind. The growth is thoroughly exposed by reflecting the cheek and removing the outer surface of the maxilla, and then it is completely excised with as free a margin of healthy tissue as possible, irrespective of anatomical structures. The growth is followed into the ethmoid and orbit, and it is often necessary to remove all the ethmoidal cells up to the base of the skull and similarly the whole of the floor of the orbit. None of this is usually done with excision of the upper jaw.

The immediate result of this operation is good and in every way better than the older operation of wrenching out the upper jaw. The deformity is considerably less, the shock is nothing like so great, and the chances of pneumonia are diminished because the mouth is left intact. This type of epithelioma is very malignant and owing to the difficulty of early diagnosis recurrence after operation sooner or later occurs.

I have operated on ten cases of carcinoma of the maxilla in this way and at the time of operation I thought that I had effected a complete removal of the growth. In one case, in order to clear the orbit, I went so far as to enucleate the eyeball as the eye was already blind. In six of the ten cases the growth recurred at the end of eight months. Four cases remained free from twelve months to three years, but in only one case was there survival without recurrence for more than three years. In all ten cases the diagnosis was made too late. Nevertheless the results of this operation are better than that of excision of the upper jaw.

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Section of Odontology.

President—Mr. J. G. TURNER, F.R.C.S., L.D.S.Eng.

The Problem of Pulpless Teeth.¹

By L. E. CLAREMONT, L.R.C.P.Lond., M.R.C.S., L.D.S.Eng.

ANATOMY.

TEETH treated by Hess's method show in a very great number of cases that the pulp is irregularly shaped. Small offshoots frequently pass into the dentine, and there are minute prolongations through the apical tissue, apart altogether from the main apical foramen. Cementum is very similar in its structure to bone. It contains lacunæ and canaliculi, each lacuna containing a nucleated cell, the processes from these cells passing along the canaliculi. These processes communicate one with another, and also with fine terminations of the dentinal tubes. By this means a chain of communication of protoplasm exists between the periodontal membrane and the pulp. It is a living tissue, allowing of the interchange of body fluids with the periodontal membrane.

The periodontal membrane performs many functions. It forms an attachment for the tooth, acts as a cushion, supplies nutriment to the bone and cementum, supports the gingivæ, presides over the absorption and deposition of bone and cementum, and, lastly, has a tactile sense. Bundles of fibrous tissue pass from the bone to the tooth, entering into the cement as Sharpey's fibres. The cells are fibroblasts. Clusters of epithelial cells, sometimes calcified, are present. In pathological conditions, giant multinucleated osteoclasts can be demonstrated. The blood- and nerve-supply are received from the apical supply to the pulp, the alveolus and the gums. According to Hartzell the lymphatic drainage of a tooth is the most perfect in the body.

PATHOLOGY OF PERIAPICAL TISSUE DESTRUCTION.

The changes that may occur beyond and round the apex may be summarized as follows: The periodontal membrane thickens. A large number of lymphocytes and a few polymorphonuclear leucocytes hurry to the spot, resulting in chronic proliferative periodontitis. The round-celled proliferation also brings about a chronic rarefying osteitis. The space in the bone thus formed is filled with a mass of chronic inflammatory granulation tissue known as a granuloma. This granuloma will be found to consist of round cells and polymorphonuclear leucocytes, fibroblasts and fibrous tissue, capillaries and occasionally some epithelial cells. The streptococcus can always be demonstrated by direct "smear" or on cultivating. If, as frequently happens, a staphylococcus is added, the granuloma will suppurate and consist partly of granulation tissue and partly of pus, or the granulation tissue may break down completely and be replaced by pus, thus becoming a chronic abscess. The pus so formed may fill the cavity or discharge through a sinus.

It is perhaps not generally recognized that a granuloma is more common than an abscess. Chronic apical abscess is, therefore, a loose and unsatisfactory term to use in this connexion. As the bone becomes rarefied and absorbed the periodontal membrane is detached and destroyed, thus depriving the apical cementum of blood-supply. As far as the tooth itself is concerned there is virulent infection of (1) the dentine surrounding the septic pulp-canal; (2) of the pulp-canal with its possible ramifications at the apex, and, lastly, (3) of the lacunæ and canaliculi of the cementum. The apical tissue is transformed into a septic sponge, becoming rarefied and absorbed. The cementoblasts still living lay down new cementum, thus leading

¹ Owing to lack of space it has been necessary to condense this paper.

to irregular thickenings of the root. Occasionally this new tissue fills the bony cavity previously occupied by the granuloma.

It occasionally happens that small masses of squamous epithelial cells are incorporated in the granuloma. The chronic inflammation present stimulates them to growth, resulting in cyst formation.

A review of the pathology leads us to the following conclusions:—

- (1) The apex of a pulpless tooth is a place of lowered resistance.
- (2) The danger of infection is greatly increased if the apical tissues have been killed and the periodontal membrane stripped by acids, alkalies, caustics, &c.
- (3) If infection passes through the apex, the following conditions may arise: (a) chronic proliferative periodontitis, (b) granuloma, (c) chronic abscess, (d) dental cyst.

(4) The contents of these pathological processes are directly connected with the blood-stream and lymphatics, and may then lead to serious lesions in remote parts of the body. To cite three instances of this:—

I know a medical student who found that his eye-sight was failing. He consulted the ophthalmic surgeon of his hospital, who found nothing abnormal in the eyes. The patient was sent on to me for an investigation of the mouth. The dentition was fairly sound. I found two teeth with extensive fillings in them, and one tooth had been crowned. X-ray examination revealed large well-defined areas of rarefaction round the apices of these three teeth. I removed them and curetted the sockets. The patient's eye condition rapidly improved and is now normal. The second case concerns a hospital patient who was diagnosed as suffering from brachial neuritis. I X-rayed some septic roots in the maxilla on the right side, and found large pathological areas at their apices. These roots were removed, and the patient's neuritis subsided. The third case of interest concerns a patient whose dental condition was far from satisfactory. X-ray examination revealed five dead teeth with well-defined apical infection. As the teeth had given no pain or discomfort the patient was very averse to having them removed. Some years later that patient developed ulcerative endocarditis and eventually died. In view of the fact that streptococci are always present in these periapical infections it is possible that the condition of the teeth brought about the cardiac condition.

TREATMENT.

The treatment of the permanent teeth can be conveniently considered under three headings:—

- (1) Devitalization of a living pulp.
- (2) Cleaning and filling of sterile canals.
- (3) Treatment of septic teeth.

(1) *Devitalization of a Living Pulp.*—This operation is of common occurrence in daily practice; how often do we find these teeth with apical infection later on. If in doubt, to remove the pulp is a good safe rule. We know that the pulp has very little power of self-repair. It is far better to remove the pulp and fill the canal under aseptic conditions than for the pulp to die subsequently and then painlessly infect and destroy the tissues round the apex. Occasionally it is justifiable to place zinc oxide, oil of cloves, and a little beta-naphthol at the bottom of a deep cavity or on an exposure, with a cap, and place a temporary cement over it leaving the tooth on trial for a few months.

The question as to whether the infection was introduced during treatment is of the utmost importance. We are very apt to forget that everything coming in contact with a tooth under treatment is a certain source of infection unless it is sterilized. Infection can only develop subsequently in two ways. The filling may leak sufficiently to allow sepsis to reach the periapical space, or it may be brought to the apex by the blood-stream. If organisms can travel one way, from a septic apex to a heart-valve, they can travel the other, from a near neighbour such as a septic tooth, or from a distant part such as a chronic appendix. The removal of

the pulp may be performed under anaesthesia or the pulp may be killed with arsenic. It is extremely difficult to maintain sterility under a general anaesthetic such as nitrous oxide. Any of the local methods are sound. Cocaine pressure under rubber has one great objection—namely, the superficial sepsis may be forced through the apex. It is quite possible that this is one of the great causes of ultimate failure. This danger can be avoided by anaesthetizing the infected upper layer only and removing it in the presence of a concentrated disinfectant such as tricresol, before anaesthetizing the remainder. If arsenic is used its action must be limited. It has been suggested that in deep interstitial cavities it is wiser to cap the exposure with some anodyne and temporary cement, and place the drug, in the proportion of 10 per cent. arsenic in a paste of zinc phosphate, in a special pit cut in the crown of the tooth. It is, of course, extremely dangerous to apply cocaine pressure after using arsenic.

(2) *Cleaning and Filling of Sterile Canals.*—In cleaning and filling the canals our watchword must be sterility. How are we to approach this ideal? I suggest the following. We cannot be aseptic, but we can do the next best thing and work in an antiseptic atmosphere. Our choice of an antiseptic is limited. We require one with great bactericidal power, rapid action, small power of penetrating living tissue, and free from irritating properties. Tricresol is the nearest approach to the ideal.

The tooth is isolated with rubber; if the cavity is below the gum level it may be wiser to fill it with temporary cement and gain access to the pulp by way of the crown. All instruments should be lying in tricresol; if fouled they should be wiped and replaced in it. A pool of tricresol should be kept in the cavity so that if possible no air may enter. Bristles and broaches and all canal instruments should preferably be kept in formalin vapour, and taken from this direct to the tooth. All pulp-tissue should be removed. Secondary dentine or pulp-stones may be dissolved out by means of sulphuric acid or hydrochloric acid. The tricresol is removed with alcohol, then the acid replaces the alcohol.

Various methods have been devised for filling the canals: pastes such as zinc oxide, zinc sulphate and 10 per cent. tricresol; zinc oxide, zinc sulphate and carbolized resin; zinc oxide, zinc sulphate and 10 per cent. paraform; chloro-percha paste. All of them are antiseptic and harmless. If gutta-percha points are used they should always be kept in formalin vapour and taken straight from this to the canal. Whatever method is adopted care should be taken to see that the filling completely fills the canal, or canals, as the case may be. In this connexion, X-ray photographs may be very helpful.

(3) *Treatment of Septic Teeth.*—Septic teeth conveniently fall under three headings:—

(i) Septic tooth with healthy cementum and periodontal membrane; (ii) tooth with infection beyond the apex; (iii) tooth with apical infection and a sinus.

Before considering any sort of treatment, it is of the utmost importance to arrive at a correct diagnosis. This will be furnished by the history, the symptoms, the clinical examination and skiagraphy. Errors are unfortunately frequently made by (1) a want of co-ordination of the various factors; (2) X-ray examination.

I will mention three pitfalls in the reading of radiographs: (a) the fact that certain root-fillings are not seen in the photograph, many sound fillings being thus condemned; (b) early periapical infection may not show any variation from the normal; (c) the translucent zone seen under a developing tooth.

Having placed the tooth to be treated in one of the three classes mentioned above, we proceed to treatment.

First, consider the septic tooth, with healthy cementum and periodontal membrane. It is essential that the contents of the septic canal be sterilized before

removal. Failure to carry out this elementary principle may result in failure, and the tooth is immediately placed in the second group with periapical infection. The antiseptic chosen for sterilization should be a strong germicide, with power of diffusibility and penetration, and it should be unable to injure the healthy tissues round the apex. I suggest the following: (1) di-chloramine-T. dissolved in chlorozone; (2) paraform (half a grain in a paste of carbolic and cloves); (3) formalin. The objection to the last mentioned is the fact that it irritates the tissues and rapidly disappears. Having selected a suitable drug, I recommend the following procedure:—

Place in position, cover with a cap, and seal in with a temporary cement for seven days. At the next visit isolate the tooth, wipe over with tricresol, and remove the contents of the canal in a pool of tricresol. I make a point of carefully reaming the canals to remove all sodden and necrotic dentine. If there is any doubt as to sterility at the second visit, a further dressing is readily applied, such as 10 per cent. paraform and tricresol, and left for a further seven days. When the canal is sterile it may be filled using the same precautions as those previously adopted for a live tooth rendered pulpless. In the case of acute inflammation following upon death of the pulp, preliminary treatment is necessary before proceeding to sterilize the canal contents. In many cases there is a collection of pus in the canal under pressure; this evacuates itself immediately an opening is made into the pulp chamber. The opening may have to be made through a filling. The tooth, which is usually extremely tender, may be kept firmly in position for the drilling by means of a small splint made from modelling composition. The canal should be kept open until all the pus has oozed away, a wisp of cotton wool being placed loosely in the opening to prevent food blocking it up.

The second and third types of septic teeth are the most unsatisfactory of all and it is round them that so much controversy arises.

The following danger signals confront us: (1) The condition is apt to arise in a tooth supposed to be well filled; (2) the septic focus is in direct contact with the blood-stream; (3) the systemic effects, if any, are very gradual; (4) the extent of tissue destruction is only seen by radiography.

Let us consider treatment under two headings: (i) By way of the pulp-canal, and (ii) surgical treatment.

(i) *By way of the Pulp-canal.*—Before commencing treatment by way of the pulp-canal, one should consider the age of the patient and the question as to whether there is any systemic infection present. The initial sterilization of the canal contents is not of so much importance as in the first group; in other words, as the periapical space is already infected it does not matter very much if a little septic material is forced through the foramen. In the case of septic teeth without a sinus, the following drugs are useful: Di-chloramine-T., formalin, paraform, proflavine in tricresol, and osogen, a new and interesting preparation from the U.S.A. Dressings are repeated as necessary, with careful reaming of the canals. Ionization has also been extensively employed, a solution of zinc chloride being used, and the positive pole being placed in the canal so that chlorine is given off. Many of the teeth in this class may remain comfortable for months or years. But the question is: "Have all the organisms present in the spongy apex and beyond it in the cancellous bone been killed?" We must not lose sight of the fact that in many of these cases the apex is frequently necrotic and it seems theoretically impossible to render this free at the onset from further mischief.

The sinus so often associated with pathological processes may arise in two ways. First, from the bursting of an acute apical abscess, and, secondly, from a chronic abscess. In the latter instance one realizes from the start that there is considerable destruction of periapical tissue, but the fact that the sinus has been established probably reduces the rate of bone destruction that has been hitherto going on.

In the case of an acute abscess with a sinus the contents of the canal should be immediately removed and the canal freely washed with an antiseptic such as 5 per cent. phenol. Concentrated germicides such as pure phenol, tricresol or perhydrol may then be forced through the

apex under pressure, thus saturating and cauterizing the root-canal and sinus. It is important, when using these strong drugs to prevent injury to the gum when the solution appears at the sinus orifice. The canal may then be filled at once with an antiseptic cement such as 10 per cent. paraform and trichresol. With a chronic abscess and sinus the same procedure may be adopted, but the forcing through of strong germicides should be repeated on two or three occasions, finally placing a temporary antiseptic dressing in the tooth for some weeks or possibly months. Ionization has also been used in these cases. Theoretically the teeth with sinuses would appear to offer more satisfactory scope for rendering the periapical tissue sterile. Whatever form of treatment is carried out by way of the pulp-canal it leaves a feeling of uncertainty on the part of the operator.

(ii) *Surgical Treatment.*—This may be conveniently considered under three headings, into the details of which I cannot enter owing to lack of time.

(1) Preliminary sterilization and filling of the root-canal as dealt with previously, followed by the removal of the diseased periapical tissue or drainage. Treatment of this nature is applicable to those teeth that show no denudation or necrosis of the root-apex, but present periapical granulomata, suppuration or cystic formation.

(2) Preliminary sterilization and filling of the root-canal as mentioned previously, followed by removal of the necrotic apex and the surrounding diseased tissue. Always provided the patient is healthy and that there are no systemic effects, the following teeth are suitable: upper incisors, canines, and premolars, and possibly the lower incisors with apical rarefied areas involving not more than one quarter of the periodontal membrane and root.

(3) Extraction coupled with curettage of the diseased area. Pathological processes such as granulomata, chronic abscesses, and cyst formations may continue to proliferate after removal of the offending tooth and frequently necessitate further surgical treatment at a later stage. It would, therefore, appear extremely important thoroughly to curette the apical region of the tooth-sockets after extraction.

The following conditions seem to be comprised under this heading:—

(a) Teeth with large periapical areas of destruction where more than one-quarter of the periodontal membrane and the cementum has been eroded and destroyed. (b) Teeth with the side of the root or roots perforated, and, as a result, lateral infection of the periodontal membrane with surrounding bone destruction. (c) Upper molars and most lower teeth with root roughening, absorption, or larger areas of destruction. (d) Teeth with a sinus opening into the antrum, nasal fossa, or on to the skin.

My personal observations on the operation of apical resection are the following:—

(1) The presence of a septic tooth adjacent to the one to be operated upon is dangerous. (2) The pulp-canal must be rendered sterile and be filled before operating on the apex. (3) Radiographs are essential in order to ascertain first the extent of the disease, and secondly, the length of the root. (4) All other dental treatment should be completed first. (5) The patient should be young, in good health and should pay reasonable attention to oral hygiene. (6) Maxillary teeth are much more suitable than mandibular for apical resection. It is impossible to treat molars in this way.

SUMMARY.

The following points are worthy of attention:—

(1) Many of our failures are due to faulty technique. In other words, our methods are not sufficiently sterile. This is particularly evident in the process of rendering a live tooth pulpless. If arsenic is used, it should not be left to act for too long a time. Cocaine pressure is contra-indicated afterwards. If cocaine is used by the pressure method from the start, care should be taken not to force infection through the apex. If the tooth to be treated is painful and the pulp suppurating, to hurry in the removal of the canal contents may ensure failure.

The tooth should be allowed to drain and the canal contents afterwards rendered sterile before removal. Mummifying paste is not above suspicion, its use may betray insufficient expenditure of trouble and lack of skill. With healthy periapical tissues great care should be taken to avoid forcing instruments and fillings through the apex. It is of the utmost importance to try to classify dead and septic teeth before deciding on the form that treatment should take. Skiagrams are essential; great care should be taken to obtain good ones, and the various pitfalls in diagnosis carefully noted. Dead teeth with healthy periapical tissues should be rendered sterile before removal of the canal contents. Apical infection, with or without a sinus, should be considered on its merits. Consideration of the following factors is helpful: (1) The age of the patient; (2) the sex; (3) the question as to whether the tooth has more than one root; (4) the oral hygiene, and (5) the patient's general health.

In the case of a young female with a clean mouth, a healthy body, and a single rooted maxillary tooth, a cure may be achieved.

Satisfactory results appear to be obtained in a number of cases treated by way of the pulp-canal, but apart altogether from extraction, it seems fair to say that the operation of trephining the alveolus and removing the diseased tissues ensures more efficient surgical drainage.

Section of Odontology.

President—Mr. J. G. TURNER, F.R.C.S., L.D.S.Eng.

A Note on Protagulin in Hæmorrhage after Extraction.¹

By G. J. GOLDIE, L.R.C.P. & S.Edin., L.D.S.Edin.

I WISH to say something on blood which is deficient in coagulating power and to bring to your notice a remedy, namely, protagulin. I desire, further, to impress on you at the outset that this new substance is no mere empirical drug, but that it is the result of patient physiological research and experiment, and realizes, in fact, the harnessing of physiology to serve our needs clinically in the arrest of hæmorrhage.

In less perfect form than we have it to-day, it came first to my notice in Dublin early in 1913.² During the war its manufacture fell into abeyance because the world was occupied otherwise, and chemicals used in its preparation, notably acetone, were required for other purposes. Thus it was forgotten: but recently under happier conditions it has come into its own in an improved form and it is this improved form which is my apology for addressing you to-night.

Protagulin contains a substance, thrombin, now believed to be the prime mover in the process of clotting.

The histological structure of circulating blood is to-day regarded as the same as when most of us were students, and consists of (1) plasma; (2) corpuscles: (a) red; (b) white—leucocytes and lymphocytes; (3) blood-platelets.

But modern observers have discovered that in the cycle of events which produces coagulation certain bodies are called into existence by physico-chemical change, and these have entirely altered the view of the mechanism of the process from what was formerly accepted.

When blood is freshly shed the following substances are known to be present:—

- (i) Fibrinogen, from the plasma.
- (ii) Thrombokinase, from the white corpuscles and blood-platelets and injured tissue-cells (orthodox, but questionable).
- (iii) Prothrombin, present as precursor of thrombin and always associated with fibrinogen.
- (iv) Calcium salts, from the plasma.
- (v) Antithrombin, present in blood and demonstrable by *in vitro* experiments.
- (vi) Corpuscles.

By the older physiologists clotting was held to result from the action of a fibrin ferment on the fibrin derived from fibrinogen, but now the view accredited is that—

- (1) Prothrombin, in the presence of thrombokinase and calcium, is activated into thrombin; that
- (2) Thrombin and fibrinogen = fibrin, and that
- (3) Fibrin and corpuscles = clot;

or in equation form, thus:—

- (a) $\text{Prothrombin} + \begin{matrix} \text{thrombokinase} \\ + \\ \text{calcium} \end{matrix} = \text{thrombin.}$
- (b) $\text{Thrombin} + \text{fibrinogen} = \text{fibrin.}$
- (c) $\text{Fibrin} + \text{corpuscles} = \text{clot.}$

From this it is evident that the clotting is a three-stage action and that the ultimate factor is thrombin. Thrombin was called fibrin ferment by the earlier

¹ Owing to lack of space this paper has been abridged.

² G. J. Goldie "Concerning Thrombin (Protagulin) in the Practice of Dentistry" (read before the Odonto-Chirurgical Society of Scotland, May, 1913), *Dental Record*, 1913, xxxiv, p. 357.

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physiologists, but this latter term has been abandoned in favour of the former for the following reasons:—

The substance is not a ferment. It has none of the characteristics of this group of bodies, e.g.:—

- (1) It is destroyed by proteolytic ferments (this is not true of any known ferment).
- (2) Its rate of action is seemingly unaffected by temperature (the action of ferments is altered by change in temperature, cold-retarding and heat-accelerating).
- (3) Solutions of thrombin give protein reactions (which ferments do not).

Thrombin is therefore a protein, probably of a peptone nature. It is prepared by a modified Gamgee method. Gamgee's method is to whip freshly shed blood to obtain fibrin; to wash this fibrin in water until all red colour disappears; to remove water by squeezing and to extract with 5 per cent. sodium chloride solution. You will note that in this, the original procedure, no effort is made to secure sterility, but in Professor Collingwood's modification special steps are taken to obtain such sterility as gives negative results with agar-agar culture. In the clear fluid thus obtained thrombin is not in solution, but occurs in suspension. Besides thrombin there are present other protein molecules of a larger and coarser nature, and these, being heavier, sink to the bottom as a flocculent precipitate when the fluid has stood for a few hours after preparation. This is in no sense a decomposition, nor does it indicate any impairment of coagulant action.

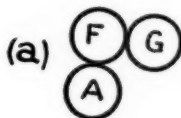
Some interesting phenomena in the physiology and pathology of the blood now come up for consideration. I have just said that three agents require to be present to initiate clotting. Two of these are present in circulating blood, namely, prothrombin and calcium; and the third, thrombokinase, is obtained on injury to the vessel. This would seem to assume that clotting can only take place when a vessel is cut, and cannot occur in an intact blood-stream; but this is not so. We know, clinically, that in all cases of abnormal vessel wall, clotting does occur without external injury, as in aneurysm, phlebitis, and around the heart-valves after rheumatic fever, &c. Why, then, does this clotting, once begun, not go on till it pervades the whole vascular system? It is stopped by an *anti-thrombin* the presence of which in the normal circulation is now recognized. This anti-thrombin is probably allied to a substance known as "hirudin," which occurs in the head of the leech and is the agent in the use of leech-bite, which retards clotting and produces its effect as a blood-letting remedy.

Physiological antithrombin is secreted by the liver and is sufficient to deal with small formations of thrombin such as arise pathologically in the blood-stream in phlebitis and arterial thromboses, &c. Its presence and function are definitely demonstrated by the injection of 1 c.c. of thrombin solution into the vein of a rabbit; here there is no intravascular clotting, the thrombin being destroyed by anti-thrombin. The existence of antithrombin can further be shown by *in vitro* demonstration.

The experiment on the rabbit's circulation is well worth detailing and explaining. If you inject into the vein of a rabbit as much thrombin as would produce coagulation in five seconds when added to blood in a glass vessel outside the body, no ill effects in the circulation of the rabbit are discernible. The animal appears just as before the injection. But a change has none the less taken place, for if the blood be now withdrawn from the rabbit it will be found to have entirely lost its power of clotting, and oddly to contain (i) no thrombin, (ii) no kinase, (iii) no fibrinogen, all of which are required, as we have seen, to produce clotting.

The explanation—one which shows that physiological processes cannot be defined in terms of mere chemistry, that vital processes co-exist, and that the living organism has to be taken into account—is this:—

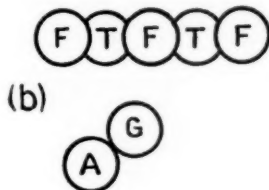
Blood in circulation, i.e., living blood, contains its three proteins—fibrinogen, globulin, and albumin, in a living complex (cf. living protoplasm), which may be shown diagrammatically as



but when blood is shed the arrangement is altered :

(i) Fibrinogen breaks free from the other two proteins—is liberated and links itself up with

(ii) the thrombin which is called into being from the blood-platelets when blood escapes from its normal vessel wall and condition, and the diagram becomes



So much, then, for preparation and modes of action; the fields of usefulness of the protagulin will now be discussed, and here *hæmophilia* first claims our attention.

This disease is primarily the result of a delay in the coagulation of the blood due to the absence of certain of the three factors mentioned previously. Normal blood clots in from five to ten minutes. *Hæmophilic* blood may take any time to clot, varying from fifteen to ninety minutes. As to its ætiology the old theory of fragility of the vessel wall is now abandoned, the œdema of dry-cupping having been shown to be no greater in *hæmophiliacs* than in normal individuals.

The defect in *hæmophilic* blood lies in its deficiency in thrombokinase, its precursors, i.e., the elements from which it is derived, platelets and leucocytes in this disease being respectively absent and fewer in number. The addition of leucocytes to *hæmophilic* blood causes rapid clotting because the derived thrombokinase thus added completes, together with calcium and prothrombin (which are already present) the three initiatory factors necessary for coagulation. That this is not a theoretical assumption has been proved at St. Mary's Hospital, where clotting has been produced in the blood of a *hæmophilic* patient by the addition to it of thrombin. In the treatment of *hæmophilia*, then, thrombin would appear to play a specific part, all important to us as dental surgeons. The quantity of calcium in *hæmophilic* blood is relatively as great as that in normal blood. That empirical remedy, calcium lactate, to which as students we were told to pin our faith, is of questionable value; its action on the vessel-walls, if any, is due to its vaso-constrictor power and not to any coagulant quality. There is, in addition, no evidence of calcium deficiency in *hæmophilic* blood, and without doubt the continued administration of the drug, if it increases the amount of calcium in blood, delays clotting. The calcium fetish is so firmly established that I must here relegate

it to its proper place by stating that Professor Collingwood has demonstrated that the clotting period was delayed in the case of the blood of a patient to whom calcium had been given for several days. Further, the addition of thrombin to two specimens of blood, the one *calcium-containing* and the other *calcium-free*, produces rapid clotting in both cases; and this proves that, apart from starting the cycle of events in normal clotting, calcium has no value as a coagulant. It is thus fairly evident that when blood refuses to clot, a deficiency of thrombokinase, and therefore of thrombin, is the cause. If further confirmation of this fact be required it is forthcoming from evidence supplied by Sir Almroth Wright, who has successfully treated hæmorrhagic conditions by the administration of tissue-extract obtained from the thymus gland, which is rich in thrombokinase.

I have indicated the source of thrombin and shown you the part it plays in inducing coagulation of the blood. It only remains for me to refer to its uses clinically in the arrest of hæmorrhage; and these are obvious. There is no degree of bleeding which may occur in our day's work to which its employment is not applicable, but its most important use is in hæmorrhage after extraction. This is shown by the following brief case-records:—

Case 1.—A hospital patient in Dublin had been bleeding after extraction for some hours with that indeterminate oozing over a large surface which cannot be plugged and which is so difficult to control. My colleague, Mr. Sheridan, who was on duty, saw the case, worked with the other styptics and hæmostatics in common use for three-quarters of an hour without avail. He sent for some protogulin and applied it on a cotton-wool roll to the oozing surface with no more pressure than would keep it in position, and in something like forty-five seconds bleeding had ceased.

Case 2.—This case, also occurring in a hospital patient, was one in which two upper central incisor roots had been extracted, and one of them continued oozing from twelve o'clock in the day till about seven in the evening. Mr. Sheridan again applied thrombin, not by packing but by placing it on cotton loosely across the mouth of the socket and leaving it to come away of itself, after the clot had formed beneath.

Case 3.—This case occurred in my practice in London in July last year. I had extracted from the mouth of a lady at 3 p.m. a second upper bicuspid (a pyorrhæic loose tooth with a degenerated socket and pericementum). It was an easy extraction, and bleeding, at first copious, had almost ceased when, against my wish, she left my house. She returned at 10 o'clock next morning; there had been bleeding all night from the socket and it had not ceased. I made a solution of protogulin with normal saline 15 m, and held it over the mouth of the socket; the hæmorrhage ceased immediately and did not return. I did not time it, but the whole procedure could not have taken two minutes for performance.

The record of this third case induces me to mention another good quality of protogulin—its absolute permanency. In the old days when the fluid solution only was obtainable one could not trust it for more than two to three months, but the dried extract which we now use does not degenerate with keeping. The sample which I used in the case just quoted was made more than eight years ago and it acted like a charm.

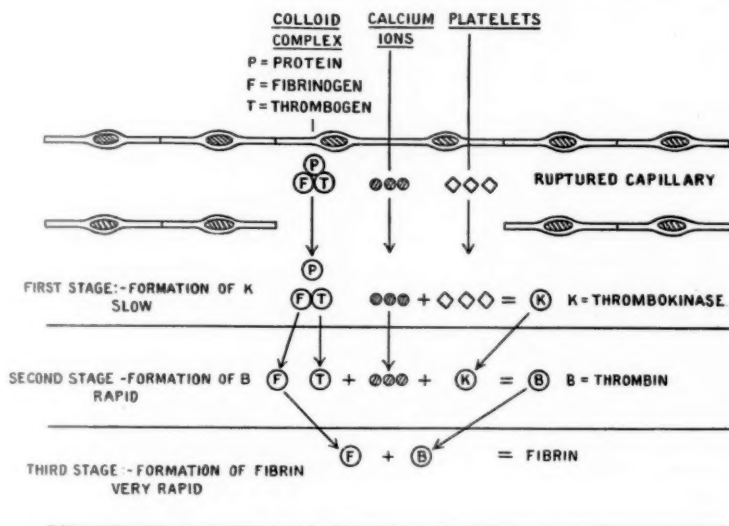
Case 4.—A left lower canine incisor loose and socket inflamed; bleeding continued copiously for about fifteen minutes and showed no sign of ceasing. I applied protogulin, directing the patient to keep the pledget of cotton wool over the socket with his own finger lightly, and bleeding ceased almost immediately.

Case 5.—A colleague in London had occasion to do some extractions for a man aged about 45 to 50 with a history of bleeding. He warned him to go home and keep quiet, and if any trouble arose to ring me up as he (my colleague) would not be in London on the following day. I was rung up about 10 o'clock in the morning to say that my friend's patient had begun to bleed about midnight and had been kept awake by it all night. The bleeding was not violent, but it was continuous and showed no sign of stopping. I made up a solution of protogulin and sent it down by special messenger with directions for use. I was rung up shortly afterwards and told that bleeding had ceased on its application.

All these cases are exceedingly satisfactory. They are not many in number, but so far I cannot record a failure. The preparation itself is no longer issued in liquid form on account of its instability, but is put up as a dry extract in a little glass tube (Bell and Croyden). Each small tube contains sufficient for one application, and each large tube sufficient for two. There is a file mark on the side of the glass tube, and the solution is made by filling up to this measure with normal saline solution 15 m : a piece of cotton wool sufficient to cover the mouth of the socket for bleeding surface is soaked in the solution and lightly applied to the bleeding part with the finger. One great advantage of protagulin is that anyone can apply it; where you have reason to suspect bleeding, that is, in cases where there is a previous history, you can make your patient safe and your mind easy by giving him a solution to use should occasion arise. I have once or twice done this with happy results.

ADDENDUM REMARKS BY PROFESSOR B. J. COLLINGWOOD, M.D.

I am asked to come as a physiologist to explain protagulin. What is the origin of blood? It is formed in what is called the vascular area of the embryo. This consists of columns of solid cells. These solid cells break down in the centre, and



the peripheral cells from the endothelium of the vessels, the central cells, the corpuscles, and the liquid in which they float form the plasma. Blood is not a tissue by itself; it is only complete when it is joined to the endothelium, for the endothelium is an essential part of the tissue. Blood, once it is outside the blood-vessels, is blood which will soon be dead. The real cause of clotting is the tearing in two of a tissue which is normally one. Coagulation is death of the blood, just as rigor mortis is the death of muscle. The certain sign of muscular death, the sign that is absolutely unmistakable, is the hardening of muscles; similarly, coagulation is the hardening of

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the blood and the death of the blood. That, I believe, is a sound foundation for any theory of clotting.

Why does blood clot? Is there any physiological function in its clotting? In so far as arterial hæmorrhage is concerned clotting will not stop it. The blood comes out so quickly that it has not time to die and clot before it is washed away. It is only in slow oozing and capillary hæmorrhage that clotting has any function in the arrest of hæmorrhage. One might ask: "Would it not be a good thing if blood clotted much more rapidly than it does—if it clotted instantaneously?" The answer is in the negative. The flow of blood has a definite function. It washes away the bacteria which have been introduced by the knife, or whatever may have caused the wound. It is an excellent method of producing sterility.

I think it might be useful for me to show you a diagram. Here is a blood-vessel with the endothelium lining (see p. 23).

Let us endeavour, if we can, to see what is inside the vessel with the eye of modern knowledge. With the red and white corpuscles we are not concerned at the moment; but you will see in the vessel what Mr. Goldie has called the protein-complex. You observe a molecule of fibrinogen, a molecule of thrombogen; and a protein molecule attached together. These three bodies are in that state of union inside the blood-vessel. You will also see calcium ions in the solution, closely accompanied by an acid radicle which I have not shown. You will notice in addition the platelets in the blood-vessel. Nobody can be sure where these bodies come from, or what their function is in circulating blood; but directly blood is shed they begin to get busy. Now, imagine a break in this blood-vessel, and the blood oozing out. The plasma is dying and the first sign of death is a break-down. In shed blood the protein breaks off, leaving fibrinogen and thrombogen still united. In addition to this the platelets in the plasma begin to react with the calcium, the reaction leading to the formation of thrombokinase. And now a third change occurs. The fibrinogen and the thrombogen break away from each other, and the original colloid complex of circulating blood is in ruins, and fibrinogen and thrombogen are free. The next change is a reaction between the thrombogen, the calcium ions and the thrombokinase, to form thrombin. Finally, this thrombin meets with the fibrinogen to form fibrin. Clotting accordingly occurs. How complex are these changes! But blood is a most complex fluid when alive, and we cannot expect its death to be simple. The story of coagulation is a long story, but there is experimental evidence in every chapter it contains. For the more recent work we are greatly indebted to the researches of Pickering, Hewitt, and De Sousa, which have thrown much light on the problem.

There are some very remarkable things about those changes. One is that the clotting of blood takes a considerable time; and here I would supplement one Mr. Goldie's statements. He said that normal blood clots in five to ten minutes and hæmophilic blood in fifteen to ninety minutes. Unless one defines the temperature of the experiment, these figures are lacking in an important detail. For normal blood will clot in a minute and three quarters at 37°C ., and in about eight to ten minutes at 18°C ., so that it is necessary to state the temperature when stating the times of clotting.

Now if one brings in the question of time, it may be asked in what stage of clotting is time required? It is in the formation of thrombokinase that most of the time is spent. Stage No. 1 is a very slow stage. Stage No. 2 is much more rapid, whilst Stage No. 3 is almost instantaneous (see diagram, p. 23). If one mixes fibrinogen and thrombin together you discover two remarkable facts: first, thrombin is practically instantaneous in action, and, secondly, it is uninfluenced by difference of temperature. It will, if sufficiently concentrated, form a clot instantaneously at 18°C ., and it will also form a clot instantaneously at 37°C ., so that it is impossible to demonstrate

any temperature influence. If the total time is, say, eight minutes, certainly seven-and-three-quarter minutes will be occupied in the first two stages, and the last stage will be practically instantaneous.

With regard to one of the experiments to which Mr. Goldie referred, that is, the injection of thrombin into a rabbit's vein, there was injected into the veins of a rabbit enough thrombin to produce intravascular clotting in five seconds if the blood had been outside the body. We calculated the quantity of blood in the animal by taking one-fifteenth of its weight to be blood. As a result of this injection there was no intravascular clotting at all; nothing whatever happened to the animal so far as we could judge. The animal was finally bled into a vessel, and the blood that was shed remained practically uncoagulated for twenty-four hours; it contained neither thrombin nor thrombokinase, nor fibrinogen. How is one to account for this most remarkable absence of intravascular clotting? One can answer that the fibrinogen was protected by its combination with protein and thrombogen in the complex from the action of thrombin, for it appears that thrombin cannot coagulate the complex of the living plasma; it can only act thus upon the fallen stones, the edifice resists its powers.

Apart from theories of coagulation, what can be said of thrombin as a styptic? One can say that it is certainly the most rapid clotter, more rapid than thrombokinase, this latter body having to form thrombin before it can exert its coagulant action. From the physiological aspect, thrombin is the ideal coagulant. In hæmorrhage, if one can get thrombin in contact with the blood as it is escaping, one will certainly clot that blood in the quickest possible time. What higher claim can be urged for any styptic, and what are the other hæmostatics in common use? Take tannic acid as an example. This drug produces precipitation of protein and not coagulation. If one produces a precipitate in a tube, the contents of the tube can be emptied into another vessel, whereas if one produces coagulation the tube will not empty—there is soup in the first case and a jelly in the second. Thrombin and kinase are gelatinizers. The advantage of thrombin over kinase is that thrombin is the finished article. It is surely better to use the thing that is formed already rather than its precursor? To apply thrombin to a bleeding surface is the physiological aid to gelatinization on the surface, and such gelatinization, if started on the surface, will presumably extend to the injured parts beneath the surface.

As to the preparation of thrombin, it is formed by a method which is an adaptation or evolution of Gamgee's method. It is curious to note the enormous amount of blood that is required to form very little thrombin; in other words, thrombin is extremely concentrated. The amount of blood necessary to give you twelve dozen tubes is the blood of about forty sheep. That is a fair estimate. Protogulin is simply a dried, active preparation of thrombin. The drying without destruction was the difficulty; we tried various methods of drying for years and one and all failed, in that they destroyed activity. After many failures success at last came; an active dried preparation which would keep indefinitely was obtained. With regard to the sterility of protogulin, the tests we are now applying are animal tests. A sample is injected into an animal to exclude the possibility of anthrax or tetanus. Those are two real dangers which must be absolutely eliminated. It is almost impossible to dry a powder and keep it free from bacteria in the air, but our mouths are saturated with these air bacteria.

Now in regard to hæmophilia, it is not sufficient to prevent hæmorrhage on the surface.

There was a case in St. Mary's Hospital to which I was called—that of a boy who had a wound in his hand. He had been bitten by a dog and had been bleeding seriously for a week and was very anæmic. Protogulin was applied to the surface, and the bleeding nearly stopped. But there was still a little oozing. Subcutaneous injections of fibrinogen (Merrell) were given and the last thing we tried was to put compression on the artery.

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These measures eventually stopped all external hæmorrhage, but in hæmophilia there is always the danger that if one stops hæmorrhage superficially hæmatomata may occur, and this was precisely what happened. In spite of this the boy ultimately recovered.

That this boy was treated by three remedies renders the case useless from the point of view of science. It was anything but a clean experiment. But when a patient is hovering on the brink of the grave, one forgets all about clean experiments and tries everything that holds out prospects of success. Such methods are fully justified when dealing with human life. Fortunately hæmophilia is an extremely rare disease. I suppose it is confined to two or three families in England and one or two in Ireland.

Section of Odontology.

President—Mr. J. G. TURNER, F.R.C.S., L.D.S.Eng.

The Permeability of Enamel.

By ALEXANDER LIVINGSTON, M.B., Ch.B., M.D.S.Liv.

(From the School of Dental Surgery, King's College Hospital.)

IT is now about five years since Mr. Rushton sent to me a translation of Eckermann's book on "Oral Osmosis." Previously, I had doubted assertions that I had observed in various places, to the effect that enamel was a fully formed substance, "dead" to all intents and purposes, being in this respect a unique tissue, comparable only to the lens of the eye—a structure devoid of nerves and blood-vessels. A study of Eckermann's work convinced me that in the main he was wrong, but that it would be necessary further to examine certain of his statements, and possibly repeat some of his experiments.

A study of the literature relating to enamel, and my own attempts to stain enamel, showed that while we were all agreed that enamel of human origin might be stained to a possible quarter of its thickness, more especially by silver methods, enamel free from cracks and spaces could not be stained homogeneously right through, *en masse*, as other tissues of the body could be.

When discussing "mottled enamel," Black [1] stated that "We cannot stain the body of enamel with any staining agent we know." These "mottled teeth"—an endemic phenomenon—were, however, stainable in bad cases for one-third of the enamel thickness, the stain being deposited in the deficient or absent interprismatic substance, or in actual spaces.

Pickerill [2] said he was unable to stain enamel from within by capillary attraction methods, and added (though he appears to have somewhat changed his opinions since):—

"I think it by no means follows that because a highly volatile and deeply penetrating stain like alcoholic carbol-fuchsin may pass occasionally across the amelo-dentinal junction that serum or lymph would do the same."

While I would not venture flatly to contradict Pickerill on this point in this particular quotation, I think the results I have reached so far largely negative such a conclusion as regards some of the constituents of serum. More important, but less easy of disproof (if that be possible) is his further statement in the same paper; he says he does not think there is any evidence that normal secretions or body fluids pass from dentine to enamel.

It will readily be called to mind that Caush [3] so early as 1905, after performing many very beautiful experiments in staining, said: "I am convinced that the enamel is a living tissue as long as the pulp is alive." In addition to the inferences he drew from his method of staining there were those that he drew from clinical observation of the staining of enamel over copper fillings. I would add to that observation one which was made to me by Professor Gilmour, repeatedly confirmed by myself, and familiar to those who use rubber-dam—that a tooth so isolated, in the human mouth, for any length of time, slightly changes colour and looks "drier" than its neighbours when the rubber dam is removed.

At this point it must be mentioned that both Fischer [4] and Von Beust [4] have stated that enamel is penetrated by fine tubes—finer, that is, than the enamel spindle spaces.

Walkhoff [5] and Mummery [5] also are of the opinion that there is hardly to be found a tooth of civilized man which does not show some defect of structure, but

I do not think such defects are of a kind that would vitiate the experiments I have performed, care of course being taken to reject obviously poor specimens.

In the first "Report of the Committee for the Investigation of the Causes of Dental Diseases" [5], in the section dealing with structure, it is stated that "it does not seem possible to conclude that enamel is entirely inorganic." This question has, in my opinion, been entirely settled by Malleson [6], of whose work, including his beautiful sections showing organic matrices in enamel, it is scarcely necessary to remind you. The same report [5], which must be allowed great authority, states with almost complete decision that enamel is stainable.

From a study of these authorities, and from my own experiences in attempting to stain enamel, it appears that, though enamel may be stained,—certainly as regards the interprismatic substance,—from without or from within, fairly easily, to a depth of a quarter of its thickness, it is more difficult to stain completely right through, and this can only be done so comparatively lightly that it does not supply conclusive proof that the enamel is permeable to any marked degree.

Experiments have been made by Eckermann [7] and others which apparently showed two things: (1) That enamel might permit the flow of water through it, and (2) that enamel might allow salt solutions placed against one surface to diffuse a salt through it, into distilled water placed against the other surface. Other more complicated variants of these experiments have been performed, notably by Eckermann, Caush [8] and Bunting [9].

The difficulty in these experiments is that a tube must be attached to the tooth, with a cement, and it is impossible to make quite sure that the cement itself is not spoiling the experiment, or permitting a leak. With regard to osmosis, Eckermann makes the following statement (p. 62, loc. cit.):

"The sound enamel is almost always impermeable or only slightly permeable to salts. Only in isolated cases where the enamel exhibited the well-known white spots did it prove susceptible to osmosis."

Bunting (loc. cit.) came to the conclusion that the tooth behaved as a permeable membrane. So far as can be gathered from his published statement he did not perform his experiments with any adequate controls. The temperature, duration, and actual quantities which permeated, are not stated, nor are any remarks made as to the source of his material. Nor does he appear to have used a "hard" glass, which is essential, for ordinary glassware is itself very slightly soluble in water. In spite of these omissions, his paper was so very definite that it seemed more necessary than ever to examine the phenomena, although none of these experiments had carried any conviction to myself. So I commenced to investigate the subject some three and a half years ago.

Given a permeable membrane, on either side of which is placed, respectively, a solution of a salt and distilled or salt-free water, certain phenomena may be observed. Water will pass through the membrane *from* the distilled water *into* that containing the salt solution. Not only may that phenomenon be observed, but it can be shown that the salt in the salt solution passes through the membrane into the distilled water. This tends to slow down the passage of water in the contrary direction, and the increasing concentration of salt in the distilled water itself increasingly inhibits the further passage of salt; in fact salt may tend to pass back again into the original salt solution.

The pressure within the *salt solution* during the process of osmosis *increases*; if the salt solution is enclosed within a suitable vessel the increases in pressure may be measured; the process of the passage of *water* through the membrane into the salt solution is called osmosis, and the pressure developed is termed the osmotic pressure. The passage of the salt molecules is known as diffusion [10], and the membrane as a permeable, semi-permeable, or dialysing membrane, according to various processes which it may perform.

At first osmosis is more rapid than diffusion, when an experiment is set up. While dialysis is possible (always given suitable conditions) for crystalloids (such as common salts), it is not possible for colloid substances such as common carpenter's glue.

I determined to investigate enamel to see whether it permitted osmosis—whether it would act as an osmotic membrane, and I constructed a small apparatus consisting essentially of two glass tubes, the approximal surfaces of which are ground perfectly flat and at right angles to the long axis of the tube. Brass crosspieces are bolted transversely to the tubes one centimetre from each end. These crosspieces serve to allow two brass plates united by long brass rods, screwed and adjustable, to draw together the ends of the glass tubes. The enamel slice, cut as far as possible transversely to the enamel prisms, was placed between the ends of the glass tubes, rubber washers, suitably perforated, intervening between glass and enamel, and forming a seal. The nuts on the rods were then gently tightened up, so that the seal might be as perfect as possible.

The teeth from which the three slices were derived for this series of experiments were all extracted for prosthetic reasons, free from caries and without macroscopic defects, nor did they show cracks subsequently on staining with eosin and examination with a microscope. Immediately on extraction they were dropped into normal saline, containing thymol to saturation, 1 in 2,000, as a preservative, the saline being at body temperature. They were immediately cut up, and the slice ground down, under a steady stream of warm distilled water. The apparatus was set up with *serum* on one side of the slice and distilled water on the other, any pressures being indicated by the mercury levels in capillary tubes attached to the larger tubes.

It was found in trial experiments that: (a) Evaporation from the free liquid surfaces in the capillary tubes; (b) expansion and contraction of the whole apparatus with varying temperatures, and (c) a definite pressure of glass-against-rubber-against-enamel to prevent leakage, were points needing close attention, otherwise, the readings of the heights of the capillary tube liquids were erroneous.

Each experiment occupied some three to six hours in the setting-up, and as far as was possible, the tooth, together with the slice therefrom, was kept at body temperature, and never allowed out of either, first normal saline, or later, distilled water, so that if it contained water, at any rate it did not dry. In fact, one attempted to keep the enamel alive, and to ensure sterility. To return to the first experiments, which might be regarded as successful, after six hours—the temperature of the whole apparatus being steady at 37.5°C . in an incubator, carefully packed in cotton wool—the mercury levels were marked with gum-paper tags. Twenty-four hours later, the temperature having remained constant and undisturbed, a rise in the level of the mercury on the *serum* side of 3 cm. had taken place. During the next two periods of twenty-four hours each, first a slight rise took place and then a slight fall.

Two similar experiments were performed giving apparently similar results.

It will be understood from the smallness of the available slices—which were about 1 mm. thick by 3 mm. in diameter, that many failures occurred before the whole was got together without a leak occurring: also that the various junctions caused trouble—sticky wax, the best joining material at my disposal, not being ideal. It will be understood that the mercury bead used as an indicator was rather difficult to control.

Ultimately I used a form of "trap" containing mercury, sealing the surface of the liquid under investigation from the outer air, and at the same time providing a manometer. Such a trap may be seen at the end of one of the main containing tubes on the apparatus. In all, only three really satisfactory experiments were

performed—all showed a similar result, but I hesitate upon this evidence to state definitely that osmosis takes place through enamel as though it was a membrane.

I should here mention that this work was carried out in the Thompson-Yates Laboratory, Liverpool, Professor Ernest Glynn kindly providing the serum from that surplus to the Wassermann reaction requirements. I have also to thank Mr. B. Clothier, of the Liverpool Dental Hospital, who kindly co-operated in finding and obtaining suitable specimens.

A second series of experiments was attempted, with the use of serum on the one side and saliva on the other. To ensure sterility of the saliva, acriflavine (1 in 1,000) was added to it. The saliva was derived from five healthy students; it was then "pooled," and shaken up with the acriflavine. No conclusive nor steady movement of the capillary indicators took place; after a fixed level was reached there was a slight upward or downward movement scarcely perceptible, and varying from specimen to specimen.

It became obvious that the apparatus would work, but was too difficult to set up in order to obtain reliable results (if any) in sufficient numbers to settle the question.

At this stage I approached the Committee investigating the cause of dental disease, and I am indebted to them for a grant which enabled the apparatus shown to-night to be obtained and used. It was also suggested by the Committee that it would be better to experiment first in the direction of the "permeability" (or penetrability) of enamel.

I will first briefly describe the apparatus, then state the results, together with my observations.

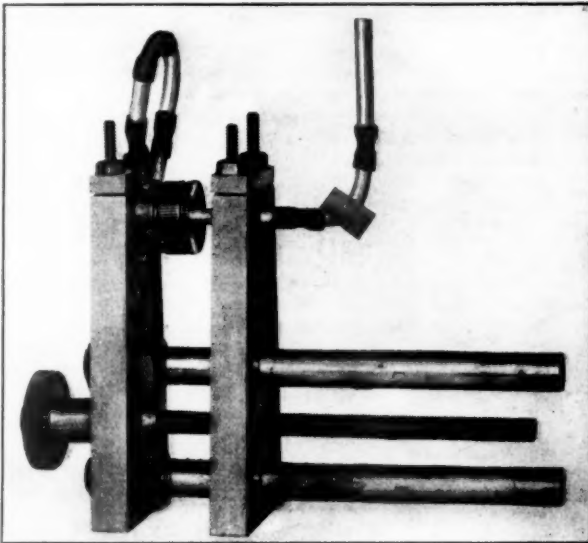
The apparatus (*see* photograph, p. 31) consists of two upright members, which may be approximated to, or separated from, one another, by means of a fine screw. Two guide-bars are placed parallel to the screw, so that the relative position of the uprights remains unchanged with distance. The upper end of each upright carries suitable blocks of lignum vitæ for the reception of the tubes acting as reservoirs for the fluids applied to the surface of the slices, which may easily be clamped in position with their rubber washers.

But it is exceedingly desirable that the whole of the enamel of a tooth may be used—a greater area is available through which permeation may take place; to make this available I therefore constructed a "tank" in which the crown of a tooth might be placed. To the cut end of the rootless crown placed in the tank, which is supported by one upright, a rubber washer may be apposed with the aid of a suitable tube held in the other upright. The margin of the washer is sealed against the open end of the tank by a tin washer and locking-ring. Two tubes are in communication with the interior of the tank, and this, together with all metal tubes, is heavily coated with pure tin. Extensions are made with rubber tubes and "hard" glass as may be required. It has been proved conclusively that the set-up experiment is leak-proof.

The teeth, fresh from the extraction room, are brought to me in normal saline containing thymol to saturation as an antiseptic. If it is desired to use the whole crown, the root is sawn off short of the enamel-cement junction, and the pulp removed, care being taken that the specimen shall be kept as far as possible in distilled water at body temperature throughout. The bulk of the dentine is then removed from the interior of the crown (for we are not discussing, nor experimenting with, dentine), care being taken to leave a supporting ring at the root-end of the crown, and to leave dentine opposite the bottom of any occlusal surface sulcus, lest, as happened in one experiment, the thin layer of enamel at the bottom of the sulcus be damaged. The root surface is now ground so that if a multi-cusped tooth is being used, this surface is parallel to a plane touching the highest cusps. This will enable the tooth to rest in the tank with the root surface parallel to the face of the tank.

The rubber washer is then placed in position; the tin washer and the clamping ring are tightened. The tube communicating with the interior of the crown is moved up, until it is considered that a seal has been effected.

It should be noticed that *all* the small apparatus is boiled up twice over before use and washed in many changes of distilled water in order to eliminate any traces of foreign salts. It may then be considered sterile, most of it being handled with forceps as it is placed in position. This is done not only to keep the apparatus sterile, but to prevent traces of salt from the skin vitiating results. The distilled water is next run into position in the tank or tube with the aid of a pipette, the distilled water *first*, to prevent accidental contamination, and it is quite closed off. The salt solution (normal saline containing 0.901 per cent. of salt) is then run into place and also closed off. The level of each liquid is, if necessary, marked by a blue



pencil line or by a paper flag on the outside of the tube. Any gross leak would be indicated almost immediately by a change in either or both levels. The apparatus is then placed in an incubator at 37.5°C ., the larger piece of the apparatus being kept there ready for use at that temperature.

At the end of a definite period—two to ten days—the fluids are drawn off with pipettes, which are always submitted to a rigid cleansing. The distilled water is analysed for salt content, silver nitrate normal solution diluted 100 times being used to titrate the chlorine content, with potassium chromate as the indicator. The burette used is one graduated to the requirements of the National Physical Laboratory. Even with this dilution of silver nitrate, and a delicate burette, some readings must be put down as giving a trace only of salt present. Each titration was repeated three times.

RESULTS.

Group I.—Molar teeth, removed for uneruption or impaction; Four teeth, seven experiments, average duration ten days:—

Greatest amount of salt found in the distilled water 0.072 per cent. NaCl.
Least " " " " " 0.030 " "

Note.—Normal saline = 0.901 per cent. NaCl.

Group II.—Three premolar teeth, removed for orthodontic reasons, and one canine tooth: Six experiments, average duration eight days:—

Greatest amount of salt found in the distilled water 0.005 per cent. NaCl.
Least " " " " " 0.001 " "

Group III.—Three slices cut from upper incisor teeth, approximately 1 mm. thick; four experiments; three, ten days, and one, nineteen days in duration.

Greatest amount—0.034 per cent. NaCl (19 days; ? solution of tooth which showed slight etching of the immersed surface).

Least amount—a trace only of NaCl present.

Group IV.—Reversal experiments (4). The apparatus was taken down after performance of an experiment, and thoroughly washed as usual. Instead of the tooth being set up again with normal saline applied to the *exterior* of the crown, it was set up with *distilled water* applied to the crown externally, and saline internally. The amounts of NaCl found in the distilled water under these conditions were always slightly less (0.002 per cent.) than in the unreversed experiments.

Group V.—"Ageing" of tooth experiments (2). Analyses of the distilled water were made at three-day intervals for twelve days. At the end of the twelfth day (fourth analysis) no salt appeared to be passing through.

Group VI.—In some of the experiments (3) aqueous carbol-fuchsin was added to the normal saline. This served the purpose of demonstrating that the apparatus was not leaking. I had an idea that the permeating salt might carry with it some of the dye-stuff, as it were, and this proved to be the case. The enamel was seen to be stained (very slightly) pink, uniformly, throughout its entire bulk, an observation confirmed by Mr. Mummery, who saw two specimens just after the experiment. The stain, perceptible to the unaided eye, was too delicate for demonstration with the microscope.

Group VIIa.—To the crown there has been applied a 5 per cent. solution of ordinary cane sugar (which, hydrolysed, gives glucose and levulose) for two periods of five days each. After inversion of the distilled-water side has been attempted, should any sugar be present, no glucose has been demonstrated by the blood-sugar test method. Dr. Lawrence, Lecturer in Biochemistry at King's College Hospital Medical School, very kindly performed tests for sugar.

Group VIIb.—One experiment has been performed with *glucose*; a 5 per cent. solution has been applied to the crown for five days. *Result* (quoting Dr. Lawrence): "A faint trace, less than 0.01 per cent., of glucose was present, but this requires confirmation."

CONCLUSION.

It appears to me that the enamel behaves as a membrane permitting diffusion, certainly to salts in normal saline solution, if the experiments are performed under conditions (except for the distilled water) which approximate to its habitat during life.

I have not yet attempted to examine the permeation of saliva and its constituents, or serum, against distilled water, and I hope that it will be possible to continue experiments with various sugars, both against distilled water and serum. I suggest that the passage of salts, and possibly the passage of water, occurs along Malleson's matrices.

I have not yet been able to determine accurately any relationship between permeability and age, sex, condition of patient, &c. To effect this purpose many more experiments must be performed.

My thanks are due to the Dental Disease Investigation Committee of the Medical Research Council for their generous help; also to my assistants, Messrs. V. D. Walker, dental registrar; J. C. Crossley and P. R. Asplet, house surgeons, at King's College Hospital Dental School, where the later portion of this work has been

carried out with the aid of a laboratory and small apparatus kindly provided by the Committee of King's College Hospital Medical School.

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A Dental Cyst in connexion with a Deciduous Tooth.

By GERALD HARBOROW, L.D.S.Eng.

THIS cyst was growing in a boy, aged 8, who was brought to me at the Metropolitan Hospital for the treatment of what was taken by the parents to be an abscess arising from a decayed deciduous tooth. Examination revealed a small, rounded swelling in the region of the right lower, first deciduous molar, extending to the canine on the same side. The molar was badly carious and septic, the canine appeared healthy, but the second deciduous molar was slightly carious. The swelling had caused no pain, nor was there any history of pain, but the swelling had been evident to the parents for at least two months before the boy was brought for treatment. X-ray examination showed a cystic condition in connexion with the first deciduous molar, extending towards the canine; the premolar did not appear to be involved in the cyst. Under chloroform and ether anaesthesia the temporary canine and the first and second molars were extracted. The first molar was in direct connexion with the cyst, and there was only a very thin bone on the outer wall. The cyst was enucleated and the first premolar was not visible. I explored the cavity and scraped part of the inner wall before I came upon a small portion of the crown of the first premolar. The cyst membrane was sent for histological examination, but unfortunately it was not too well prepared. The report of the result, however, was that of a growth of a cystic character. I have shown the section to Mr. Turner, who kindly had a photomicrograph prepared of the portion showing epithelium.

I was unable to demonstrate the presence of any cholesterin.

The important points about this cyst are the following:—

- (1) The premolar was not in the cyst cavity.
- (2) It was not apparent until the intervening tissue was scraped away after the removal of the cyst membrane.

In the "Report on Odontomes," the description of dentigerous cysts is given as follows:—

"Dentigerous cysts are innocent tumours arising from the dental epithelium and are connected with unerupted teeth. . . . From a dental cyst it can be distinguished by the age of the patient and the history of previous infection."

In describing dental cysts the authors state that they are—

"Innocent, simple cystic tumours arising, as the result of irritation, from the dental epithelium about the root of a tooth which has undergone eruption. . . . They have only been recorded in connexion with the permanent teeth."

The cyst I have described is without doubt a dental cyst, arising from the epithelium about the root of the deciduous molar, due to the irritation caused by its septic condition. It corresponds to the description of a dental cyst just quoted and does not fit in with the description of a dentigerous cyst. It may easily be imagined that this cyst, had it been left alone, might have grown, and so involved the premolar. It would then have been mistaken for one arising from the premolar.

The appearances shown in this case seem to strengthen the theory propounded by Mr. Sprawson, viz., that dentigerous cysts are dental cysts arising from the deciduous dentition and that owing to their growth they embrace the permanent tooth and are, consequently, described as dentigerous cysts.

34 Harborow: *Dental Cyst in connexion with a Deciduous Tooth*

Mr. J. G. TURNER (President) said that Mr. Harborow had submitted the sections to his (the speaker's) judgment and he had had lantern slides prepared at the Research Laboratory of the Royal Dental Hospital. The first slide was a low-power photograph showing part of the irregular cyst cavity. Epithelium was found only at one part. The surrounding tissue was thick and granulomatous and showed no fibrous-tissue formation (fig. 1). The second slide was a higher power photograph of part of the epithelial-lined cleft (fig. 2).

The condition was an early stage of dental cyst, before the epithelial sponge-work had been flattened out and before a fibrous wall had formed. He did not think the record of the case supported the dental cyst origin of dentigerous cyst, since this particular cyst was undergoing inflammatory destruction.



FIG. 1.



FIG. 2.

[May 25, 1925.]

Circulation of Lymph in the Dentine.

(PRELIMINARY REPORT.)

By E. W. FISH, M.D., Ch.B., L.D.S.

(From the Institute of Physiology, University College, London.)

In the dental literature of the last ten or twelve years descriptions have appeared of lymph spaces around the dentinal fibril [1] and [2]. Nerve fibres have been described as entering this space [3] and elaborate investigations have been undertaken, such as putting children in institutions upon experimental diets [4] in order to see whether it was possible to modify the resistance of the dental tissues to caries—the circulation of body fluids in the dentine, if not in the enamel, of course being implied.

The evidence for the presence of this circulation appears, however, to rest upon somewhat controversial interpretations of microscopical sections, some of which were prepared by forcing pigment into the pulp-canals of extracted teeth under pressure.

It was thought to be of interest to investigate *in vivo* the extent to which dentine is irrigated by the body fluids, and it is proposed to give in this report, first, the result of experimental attempts to introduce foreign substances into the dentine by way of the blood-stream, and, secondly, to give the results of a series of analyses of human dentine at different periods of life which seem to indicate the occurrence of metabolic processes in the dentine.

I.

The first attempt to introduce a new constituent into dentine was made upon a rabbit; 4 c.c. of a 1 per cent. solution of trypan blue were injected subcutaneously each day for two weeks. All the soft tissues of the animal became dyed an intense blue colour. The saliva, the urine, and even the cornea, were tinged with the dye. At the end of the two weeks, the rabbit was killed, the teeth examined, and dark blue crescents were seen on the occlusal surfaces of the first two upper molars on each side. The blue area corresponded in shape with the area of dentine exposed on the occlusal surface.

As, however, the rabbit's teeth are of persistent growth, and it was possible that the blue portion of the dentine represented a soft, badly calcified zone, these results are not of value, and the subsequent injections were given to cats.

Specimens from five cats are described and shown:

Cat A.—A young, adult female cat, had a subcutaneous injection of 10 c.c. 1 per cent. solution trypan blue each week for two months.

Cat B.—A young pregnant cat, was given a subcutaneous injection of 10 c.c. 1 per cent. trypan blue on the first day; 15 c.c. 1 per cent. trypan blue on the seventh day, and was killed on the nineteenth.

Cat C.—An old male cat, was given a subcutaneous injection of 30 c.c. 1 per cent. trypan blue on the first day and was killed four days later.

Cat D.—A female cat several years old was anaesthetized by the chloroform chloralose sequence, a canine tooth was extracted as a control and 14 c.c. of a 1.25 per cent. solution of ferri et ammon. cit. were injected intravenously. Half an hour later a second tooth was extracted and 20 c.c. more of the iron solution were slowly infused. One and a half hours after the first injection a third tooth was extracted.

Cat E.—A very young female cat was given chloroform and chloralose and had a control tooth extracted, then 30 c.c. 1 per cent. solution of trypan blue were given intravenously with a little saline. A second tooth was extracted half an hour and a third tooth one and a half hours after injection.

Specimens of dentine were prepared from the teeth of all these cats, immediately after extraction as follows: The canines were chiefly used and the middle third was sawn out. The cementum was ground and filed away and the pulp surface of the dentine ground down with pumice, leaving what we may call a middle section of dentine. In the case of the cat injected with the iron salt, pumice and carborundum tools were used to avoid contamination with iron.

The dentine of all the cats contained varying quantities of the drugs injected as follows :—

Cat A.—(Two months' exposure to the blue.) The dentine shows a very definite blue colour throughout, especially marked in the portion nearest the pulp.

Cat B.—(Two injections; killed on nineteenth day.) Shows loss of the normal yellow colour of the dentine which is replaced by a faint blue tinge.

Cat C.—An old cat which had only one injection, and was killed four days later, showed only very faint traces of the dye in the dentine; these have now almost entirely disappeared from the specimens.

Cat D.—(Intravenous injection of iron.) Both the control tooth extracted before injection and the one extracted half an hour after the first injection showed only the merest trace of iron with the potassium thiocyanate colorimetric test. The tooth extracted one and a half hours after the first injection—a second larger injection having been given—gave a very much stronger colour and clearly contained much more iron.

Cat E.—(Intravenous injection of 30 c.c. of trypan blue.) Both the tooth extracted half an hour, and the one extracted one and a half hours, after injection show a very faint bluish tinge in the dentine, which, however, becomes more apparent if the specimens are rendered translucent by being moistened.

A microscopical examination of a very thick longitudinal section of a tooth from Cat A showed, when moist, the tubules superimposed one on another as watery blue lines. The matrix did not appear to be dyed and the blue colour had extended along the tubules half-way from the pulp to the cementum in the radicular portion of the tooth.

II.

Black [5] working in 1895, made a large number of estimations of the lime salts in the dentine and some of his deductions are of considerable interest. He based his calculations, however, on the percentage of ash rather than upon the percentage of calcium.

It was therefore decided to prepare a number of specimens of dentine from patients of different ages and to have a calcium estimation made in order to observe whether any metabolic processes occurred in the dentine as the individual grew older.

From the appended table it will be seen that the average proportion of calcium salts in the dried dentine is 5 per cent. higher in adult life than it is in a newly erupted tooth.

When a tooth becomes translucent and loose from pyorrhœa its calcium content appears to diminish.

The series of cases is, of course, small and therefore inconclusive. The result, however, as far as they go, appear to be definite. The accuracy of the estimations has been controlled by duplicate analyses which have shown only negligible discrepancies.

CALCIUM CONTENT OF THE DENTINE AT DIFFERENT AGES.

Case No.	Condition of tooth and alveolus	Age	Sex	Tooth	Percentage of CaO	Average percentage of CaO	Equivalent percentage of natural salts
6 ...	Normal	9 ...	F ...	4 ...	37.15	36.65	66.7
7 ...	Normal	12 ...	F ...	4 ...	37.05		
11 ...	Normal	9 ...	F ...	4 ...	35.5		
17 ...	Normal	10 ...	F ...	4 ...	36.9		
1 ...	Pyorrhœa, tooth firm	55 ...	F ...	3 ...	41.05	39.42	71.74
4 ...	Caries, vital	21 ...	F ...	6 ...	40.09		
10 ...	Caries, vital	38 ...	M ...	6 ...	38.4		
12 ...	Caries, vital	59 ...	M ...	3 ...	38.7		
13 ...	Pyorrhœa	45 ...	F ...	4 ...	38.9		
18 ...	Pyorrhœa, tooth firm	68 ...	M ...	3 ...	39.4	37.22	67.74
5 ...	Pyorrhœa, tooth very loose	56 ...	F ...	8 ...	37.93		
16 ...	Pyorrhœa, tooth very loose	60 ...	F ...	3 ...	36.5		

The figures in the last column have been obtained by calculating the amount of a mixture of 90 per cent. calcium hydroxyapatite and 10 per cent. calcium carbonate, which is represented by the quantity of calcium oxide found on analysis.

CONCLUSIONS AND DISCUSSION.

(1) From a macroscopic examination the dye appears to have penetrated the whole of the dentine, though it is more concentrated in the radicular portion and especially in the part nearest to the pulp.

(2) Microscopically, it is only possible to demonstrate the dye in very thick sections. These are necessarily unsatisfactory, but they definitely establish the fact that it is possible to introduce a foreign substance deeply into the radicular dentine.

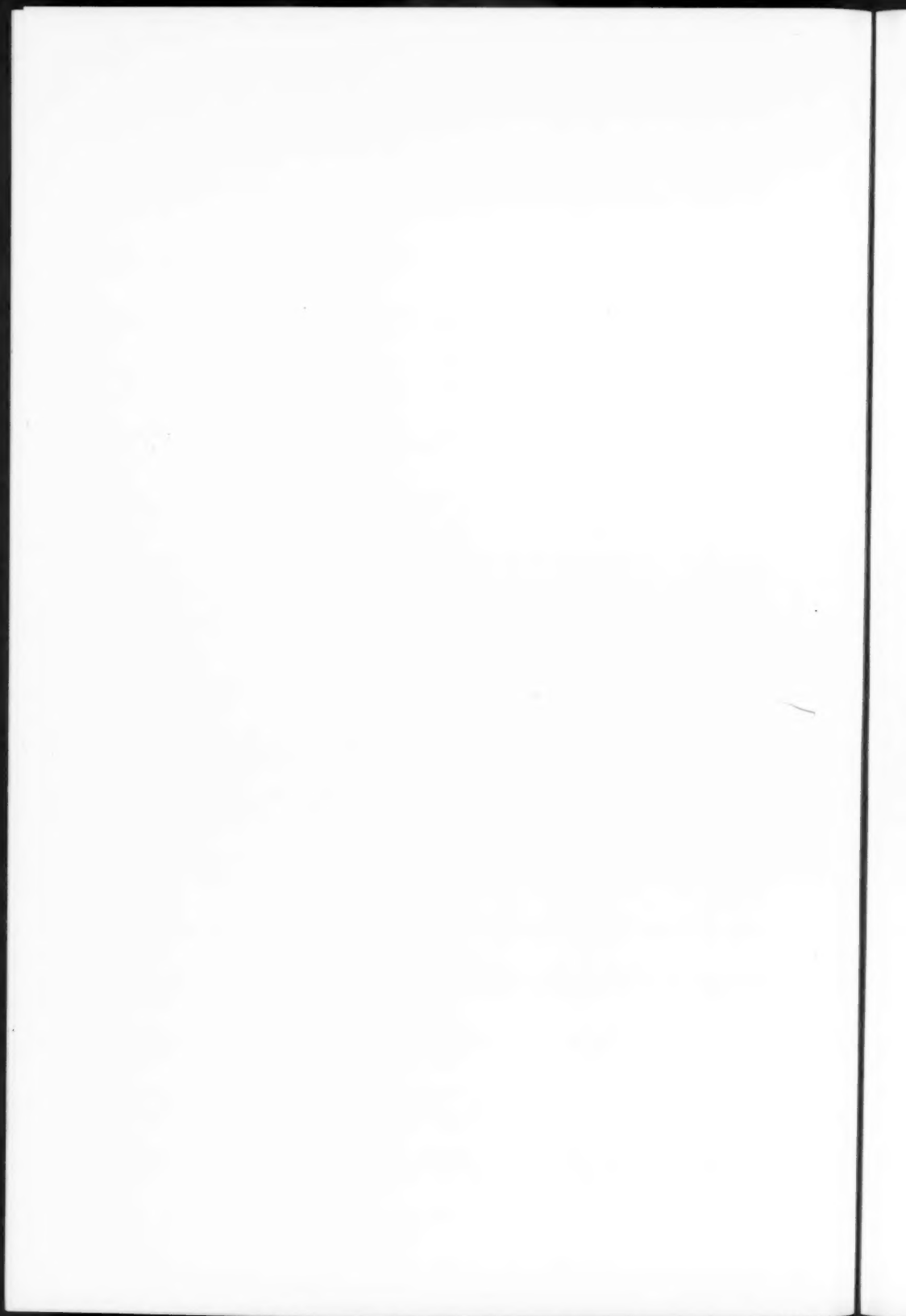
(3) The microscopical appearance of the dentine of Cat A, in which the tubules showed as watery blue lines throughout the first half of their course, suggests that the dye has penetrated by slow diffusion along a semi-solid fibril. On the other hand the rapidity with which iron (Cat D) and trypan blue (Cat E) appeared in the dentine suggests an active circulation of the body fluids in the dentine. Repetition and modification of these experiments may throw more light upon this question.

(4) The analyses of human dentine will be continued until a more representative series is available. The indication of the present results is that anabolic processes are being carried on in the dentine in early life. The last two results suggest the possibility of katabolic changes in later life, but the matter is still under investigation.

(5) Attempts have been made to withdraw calcium from the teeth of rabbits by adrenalectomy, but the loss was negligible. Hypertrophy of the other adrenal and the persistent growth of the rabbit's teeth render the experiment uncontrollable. It is intended to attempt the reduction of the calcium content of dentine in cats by calcium starvation during pregnancy, and in other ways.

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Section of Odontology.

President—Mr. J. G. TURNER, F.R.C.S., L.D.S.Eng.

The Prevalence of Supernumerary and Supplemental Teeth among the Natives of India: Note on Models showing Supernumerary Teeth.

By C. F. BADCOCK, L.D.S.Eng. (Communicated by J. H. BADCOCK, L.R.C.P., M.R.C.S., L.D.S.Eng.)

SUPERNUMERARY teeth (as distinguished from supplemental) are of quite frequent occurrence among Indians and Eurasians. Models were taken only when the teeth were extracted and when time served, and these call for no special remark, except perhaps the one showing three supernumerary teeth grouped together behind the upper central incisors. This is the largest number I can remember having seen in this position.



FIG. 1a.

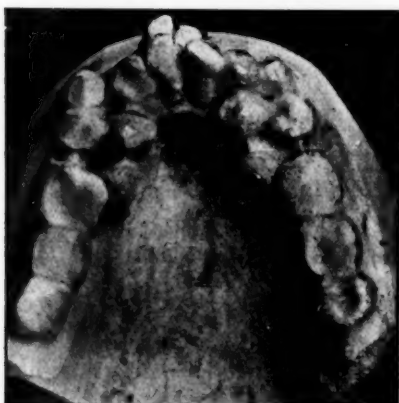


FIG. 1b.

LOWER MOLAR LINGUAL ROOTS.

When looking over a haphazard collection of abnormal teeth, I was struck by the fact that all the extra-rooted lower molars were of the same type—that described by Mr. Gabell and Mr. Whitehouse in "The Science and Practice of Dental Surgery" as having a post-lingual root.

Gabell and Whitehouse mention the occasional occurrence of an extra root on the post-lingual surface of the deciduous lower second molar. Of the permanent lower first molar they say:

"The anterior root is sometimes found to be bifurcated to any extent of its length. Moreover, a third root is sometimes present on the lingual and posterior aspect of the tooth, in size varying from that of a hair up to that of the posterior root, which it seems to displace outwards. This variation should be looked for in root-canal treatment in case the posterior root should be found to be of small size and displaced outwards."

These authors do not allow such a variation to the second molar in this country, but I think it is sometimes present in the better developed dentition of the native of India. Two or three of these specimens appear to be second molars.

The extra root is always lingual, and generally in alignment with the posterior root, but is obviously not produced by bifurcation of that root. Most of the specimens are from indigenous Indians, but some are Eurasian, and two at least British. The condition is so frequent and so true to type as to suggest a variation (the term used by the authors referred to) rather than an abnormality. The fact that, as mentioned above, it occurs in the deciduous dentition is of special interest in this connexion.



FIG. 2.

In contrast with the wealth of lingual-rooted lower molars, I have only one in which there is the slightest tendency to bifurcation of the anterior root. This is the third molar, and I include it with the others. I suspect this bifurcated anterior root of being a myth, arising from the anterior root having two canals. In this connexion, I fancy that the lower incisors in this country frequently present two canals, the flattening of the roots often being exaggerated to the extent of grooving on the approximal surfaces. I also exhibit a section of such a double canal in a lower lateral, extracted on account of inward displacement. I have never come across a double-rooted lower canine.

[February 23, 1925.]

A Report on Certain Drugs and Solutions used in Local Anæsthesia.

By F. N. DOUBLEDAY, L.R.C.P.Lond., M.R.C.S., L.D.S.Eng.

IN previous communications¹ I have described the technique used by me in the production of local anæsthesia. To-night the following are the objects of this report:—

¹ F. N. Doubleday on "Local Anæsthesia," *Guy's Hosp. Gaz.*, 1907, p. 98; "On Local Anæsthesia in Conservative Dentistry," *Brit. Dent. Journ.*, 1915, p. 157; "On Local Analgesia," *Brit. Journ. Dent. Sci.*, 1916; "On Local Anæsthesia in Dental Operations," *Proc. Roy. Soc. Med.*, 1918-19, xxii (Sect. Odont.) p. 1; "The Anatomy of Local Anæsthesia," *Brit. Dent. Journ.*, March 1, 1920; "The Problem of Local Anæsthesia," *Brit. Dent. Journ.*, July 1, 1920.

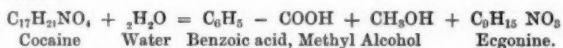
(1) By evidence as to the chemical structure and reactions of certain drugs, to lay a more solid foundation of knowledge regarding their use; and to enable dental surgeons and others to estimate more effectually the probable value of new drugs recommended as local anæsthetics.

(2) To offer evidence as to the causation of minor discomforts, such as stiffness, swelling, and after-pain; these being, in the experience of your reporter, the only ill-effects following the use of local anæsthetics for dental and minor surgical operations.

(3) To draw practical deductions from the observations made.

The main course of my experiments has been directed towards the use of novocain, but in order to provide means of comparison the following drugs have also been employed: stovaine, alypin, atropine, butyn, β -eucaine, cocaine, adrenalin, strychnine, and caffeine. The modern synthetic preparations employed are all substances obtained by studying the cocaine molecule. It will be noticed that the molecule contains a tertiary amine group $N(CH_3)$, a benzoyl derivative (C_6H_5), group derived from a secondary alcohol, and an ester.

The alkaloid cocaine, on hydrolysis with alkalies, yields ecgonine, benzoic acid and methyl alcohol.



Ecgonine has been shown by Willstätter¹ to be the carboxylic acid of tropine. Tropine is one of the alkaloids derived from belladonna, and it is an optically inactive acid.

By comparing the graphic formulæ of these substances we shall see how they are built up, and it will be seen that each of them contains the same primary grouping, and that these local anæsthetic drugs are obtained by alterations in one or two groups of atoms while the basal group remains. I am much indebted to Professor Gibson, of Guy's Hospital, who helped me with the preparation of the chart of the structural formulæ of local anæsthetic drugs which is shown. When we see complex molecules like this, we need to visualize the manner in which substances of such a large molecular weight can diffuse through cell membranes, and become attached to the cell protoplasm producing its effects—in this case local loss of both sensory and motor function—and also to the manner in which it is eliminated. The difficulty of investigation lies in the fact that one cannot analyse what happens in a living cell. Ehrlich has helped us to picture it. He says that every cell has a number of side-chains which have a selective affinity with certain other side-chains of the molecules of food, toxin, drug, &c., with which they come into association. In tetanus the haptophore (or linking group) of the toxin will combine with the side-chains of cells of the central nervous system only. We may picture the side-chains of a very rotund person as having a special affinity for fats, while those of a thin person combine with lipoids very indifferently. In the same manner local anæsthetics must be thought of as needing side-chains which will combine with the protoplasm of cellular tissues and also as having other side-chains, which produce anæsthesia, drowsiness, instability, &c., immediately or more remotely as they are broken up, and the liberated side-chains produce effects other than that of the original drug. Doubtless drugs like novocain will act on the terminal fibres of nerves or on the cells of the central nervous system. In dental practice we interrupt the passage of both efferent and afferent nerves by injecting solutions into the neighbourhood of a nerve trunk in any part of its course. This implies that the drug acts upon the axis-cylinder process. It is a constant experience when mandibular nerve blocks are done that the fibres of the mental nerve supplying the lip become gradually anæsthetized first; then the touch sensations of the periodontal membrane and bone go and finally the nerves to the pulp become insensitive.

¹ *Berichte der Deutschen chemischen Gesellschaft*, xxxiii, p. 1170; xxxiv, pp. 129, 3163.

To me this means that in the nerve-bundle constituting the inferior alveolar or dental nerve the fibres going to supply the skin of the lip are peripherally placed, those to the periodontal membrane constitute a deeper layer of nerve fibres, while those supplying the tooth pulp are centrally placed. Moreover it implies that it takes a drug in a local anæsthetic solution a definite period of time to penetrate the nerve-bundle. Here the drug must anchor itself to the tissues. It is then gradually eliminated—whether in its original form or as dissociation compounds is not known. There is one piece of evidence which suggests that the drugs are broken up in the tissues. When a large dose of novocain has been given, the patient often complains of drowsiness for several days after the injection, sometimes patients say they have been obliged to sleep for some hours. This is due to the action of the benzoyl group of the novocain molecule.¹ As the anæsthetic effect of the novocain molecule has passed it may be evidence that the novocain molecule is broken up and that the action of the benzoyl group on the central nervous system is persisting. One of the factors influencing the diffusion of the drug must be the acidity or alkalinity of the solution employed. Dr. Coles, of the Biological Laboratories at Cambridge, and Dr. Walker, of Guy's Hospital, were kind enough to examine the PH reaction of the novocain solutions I employed. Both reported similar results. The PH reaction of the blood is 7.44, that of a 1½ per cent. solution of novocain in Ringer's solution is 6.8, that of novocain and adrenalin in Ringer's solution is 6.6. I have also done a number of experiments in mixing blood with the various solutions of novocain. Some of the results are exhibited under the microscopes to-night. They tend to show that the usual solution of novocain in Ringer's solution is too concentrated and causes crenation of the cells. This must be a cause of after-pain and local swelling in practice. Most interesting results have recently been published by Krogh² showing that there are muscle cells upon the capillary vessels which have the power of altering their calibre. He has shown that quite dilute solutions of adrenalin produce stasis in the capillaries, and waterlogging of the affected part. This helps to explain the manner in which adrenalin localizes the action of local anæsthetic drugs, and also the way in which it may contribute to after-swelling and discomfort. During this winter, I have myself had twelve mandibular regional injections, and my nurses and some of the students at Guy's have also had them without knowing what was being injected. I think there is little doubt that at the time of injection one knows whether adrenalin is in the solution or not, and the longer duration and greater depth of the anæsthesia are important indications of the presence of adrenalin in the solution. None of us experienced any toxic effects from these injections.

It has been suggested that the temperature of the injected fluid varies sufficiently from that of the body to be a cause of pain.

Experiment.—A thermometer is kept in a tumbler of water at 37° C., so that the initial temperature of the thermometer may accurately represent that of the body. An ordinary novocain and adrenalin solution (two E tablets in 3 c.c. 50 per cent. Ringer) is prepared by boiling and is loaded into a cold all-metal syringe, a dummy head is screwed on, the upper part is removed, and the thermometer is inserted into the fluid. The temperature will be found to be within a degree of normal body temperature.

Another important experiment deals with the evaporation of the conveying solution. For some years past, open mugs have been supplied for boiling local anæsthetic solutions. If 3 c.c. of fluid are put into these mugs and they are allowed to boil freely for a minute, an appreciable loss of fluid by evaporation occurs. This must result in the injection of a fluid which is too concentrated and is likely to cause crenation of the tissue cells, resulting in local stiffness, swelling and discomfort.

¹ May, "The Chemistry of Synthetic Drugs," p. 19; Martindale, "Extra Pharmacopœia," xi, p. 257.

² "The Anatomy and Physiology of Capillaries," ed. 1924.

Where glass test tubes are used for boiling solutions the vapour re-condenses on the cool part of the tube and runs back, so that no loss of the solvent is likely to occur.

Effects of pressure are shown by the following sections of frog and rabbit muscle injected under varying degrees of pressure. For all the animal experiments a Sykes' syringe was employed; this enables the fluid to be injected minim by minim and, as you see, the injection may be made very slowly, so that the fluid accumulates as a minim on the end of the needle and under pressure, and the injecting fluid therefore flies violently out. If injection is made under pressure into muscle, the fibres are separated and much pain will be caused by tension of the sensory nerve-fibres; this is confirmed by clinical experience. We need to take the greatest care to avoid injecting into muscles like the internal pterygoid, the facial muscles, or the areolar tissue of the palate. The injection of air under pressure will have a similar effect.

Further series of experiments have been devised to show the effect of the local anæsthetic drugs and solutions on living tissues. Much important work has already been done on this subject, but the drugs have been either applied directly to the

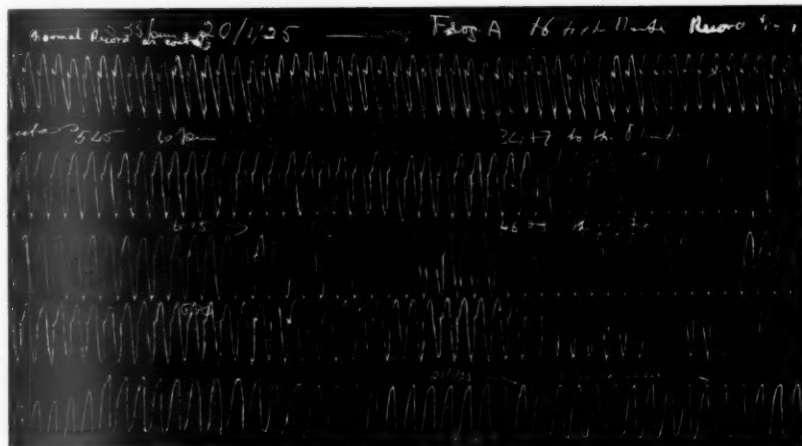


FIG. 1.—Control record showing kymographic tracing of a frog's heart-beat at fifteen-minute intervals, and, in the lowest line, after fifteen hours. (Record No. 1.)

heart, or injected into the blood-stream. The new feature of the present experiments is that the injection has been made into connective tissue, so that absorption by the local tissue-cells must precede the manifestations of the action of the drug on the heart.

In endeavouring to ascertain the possible toxic effect of local anæsthetic solutions I determined to obtain a series of heart tracings from decerebrated frogs, and to check these as far as possible by control observations upon man. In all I obtained eighty-three graphic records of the frog's heart, and as these were independently made by three separate workers (Mr. MacLagan, Mr. Baker and myself) who agreed as to their results, I think the evidence should be accepted as good. I chose the frog for these experiments because it is easy to decerebrate and to handle. It was possible to get a broader statistical basis with the frog; had mammals been used only a small number of observations could have been made. It was necessary first to ascertain what was a normal dose and what was the period of recovery for a frog. A decerebrate frog is passive and sluggish, but when stimulated by light pressure

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from behind, it jumps straight ahead and lands on its four legs like a normal frog. Injections of $1\frac{1}{2}$ per cent. novocain and adrenalin were made into the right leg in a direction away from the pelvis. Now when the frog was stimulated to jump

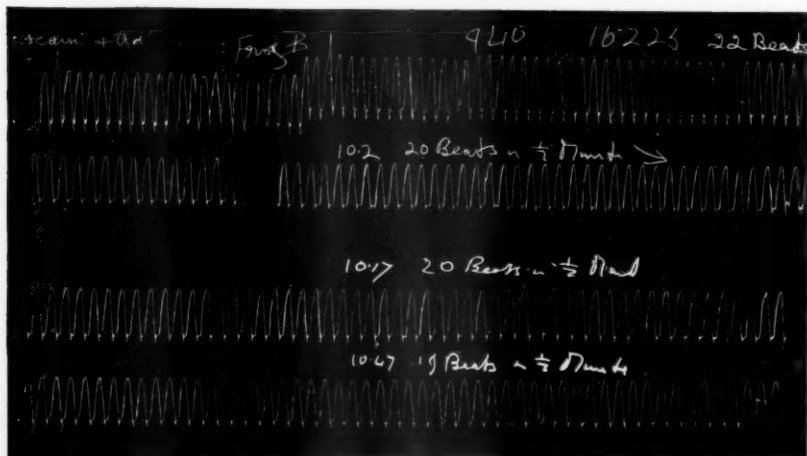


FIG. 2.—Upper line normal, lower lines fifteen, thirty and forty-five minutes after injection of 5 minims of a $1\frac{1}{2}$ per cent. solution of novocain and adrenalin in Ringer's solution. Shows scarcely any effect on heart. (Record No. 6.)

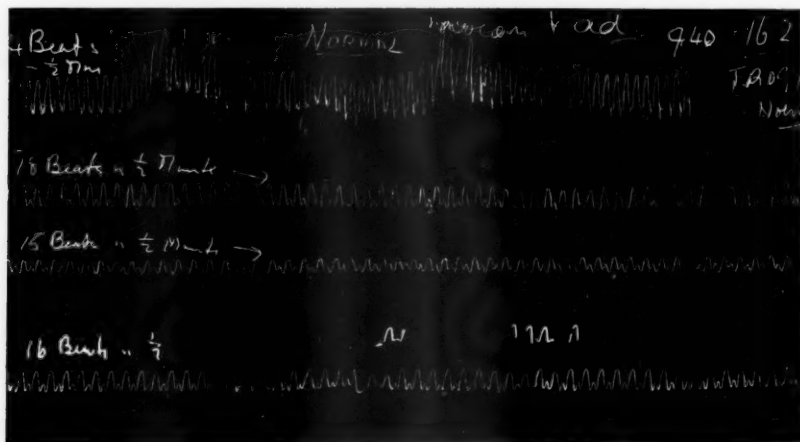


FIG. 3.—Another frog similarly treated but showing marked heart failure. (Record No. 43.)

it swerved towards the paralysed side and no longer jumped straight; when placed on its back it resumed the ventral position by always pushing itself over from the left or unparalysed side; a decerebrate but uninjected frog can be induced to turn

from either side. By varying the dose of the drug and observing the reactions of the frog as described above, I concluded that a frog will recover from five minims of the solution in one hour. This is a larger dose proportionately to the body weight than would be given to a human subject, but the depth and duration of anaesthesia correspond closely to the results of a submucous injection in man, and it was decided to take this dosage and strength as the basis for observation. The next step was to obtain kymographic tracings of the hearts of frogs which had been decerebrated, but not injected, as controls. Fig. 1 shows such a record taken at intervals of fifteen minutes for an hour; then the frog's body was wrapped in cloth soaked in normal saline, and a record made fifteen hours later. It will be seen that although there is some loss of the sharpness of the cardiac curves, the amplitude of the contractions is well maintained. This is typical of similar control records, and shows that if the frog's body is kept moist the heart will continue to beat with little alteration for many hours. Control records were also obtained after dorsal injection of five minims of normal saline, with similar results. The next step was to inject a series of twenty-five frogs with five minims of novocain and adrenalin solution. The

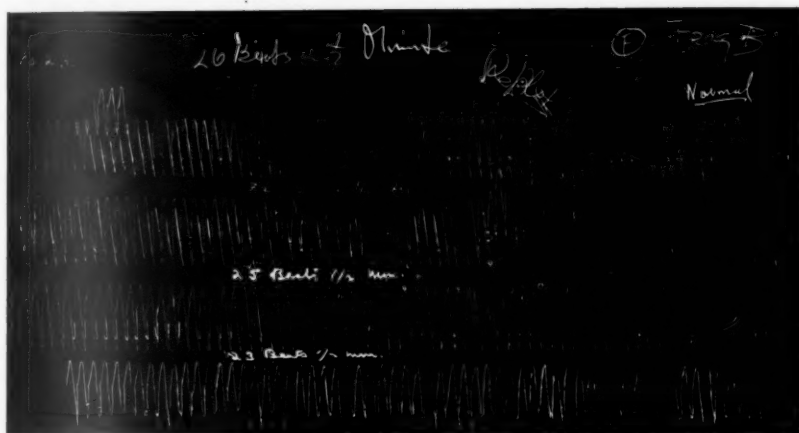


FIG. 4.—Upper line normal, the remaining lines fifteen, thirty and forty-five minutes after the injection of 5 minims of novocain only in Ringer's solution. (Record No. 23.)

injection of local anaesthetic solutions in human subjects is made into connective tissue; injections into blood-vessels are carefully avoided. To secure a like result, the solutions were injected into the connective tissue of the frog's back, and six dissected frogs, with the hypodermic needles still *in situ*, are now exhibited to show you exactly where these injections were made. The exigencies of my hospital work compelled me to carry on this work from September to February; it was therefore done on winter frogs at the temperature of the physiological laboratories at Guy's Hospital. The method employed was to decerebrate the frog, expose its heart, place it on the recording table and take a normal tracing, noting on the record the date, designation of the frog by serial letters, sex and approximate weight of the frog, and the time. The injection was given, and the time, drug, dosage and solvent noted. Records were then obtained after fifteen, thirty and forty-five minutes, and sometimes after several hours. The frogs were completely killed after the experiments had been carried out, histological sections being made from their bodies in some cases. Of these

twenty-five frogs the heart tracings of thirteen showed little effect from the injection (fig. 2); twelve showed definite slowing and failure of the heart's action (fig. 3); only one of the twelve gave evidence of recovery. It was now necessary to discover whether these twelve cases were affected by the novocain, or by the adrenalin, or only when both were used together. Ten frogs were injected with novocain only (fig. 4); nine of them showed little or no effect, one showed evidence of heart failure.

When adrenalin is injected alone a very different picture is seen (figs. 5, 6, 7). With an injection of $\frac{1}{50000}$ adrenalin chloride there is a slowing of the heart-beat and a loss of the sharpness of the systolic and diastolic curves. When $\frac{1}{25000}$ adrenalin chloride solution is injected, this becomes more definite and the relative amplitude of the heart's action becomes much less. With $\frac{1}{5000}$ adrenalin these changes naturally become much more obvious.

These results are in contradiction to those of various writers who have reported that adrenalin produces an increased contraction of the heart-muscle. It is interesting to find that of four frogs, each injected with 5 minims of a $1\frac{1}{2}$ per cent. solution of cocaine hydrochloride, without adrenalin, only one showed evidence of heart failure. I then tried injecting novocain and adrenalin along with various drugs which might act as antidotes to the adrenalin. Caffeine citrate, strychnine sulphate, and atropine sulphate were tried, and of these atropine sulphate gave by far the best result. Since the action of the atropine is to prevent the slowing of the heart by inhibiting the action of the vagi, it is suggested that the adrenalin has a directly depressing action on the nerve-endings or muscle-fibres of the heart.

In all the frog experiments strict precautions were taken to avoid hæmorrhage; the heart was kept moist and rested between the tracings. The experiments dealing with the isototoxicity of solutions were reserved for next year.

N.B.—I must gratefully acknowledge the kindness of Professor Pembrey, who allowed me the use of his laboratory, and the assistance which I received in the experimental work from Mr. McGlagan, Mr. Kenaar, Miss Butler and Mr. Baker.

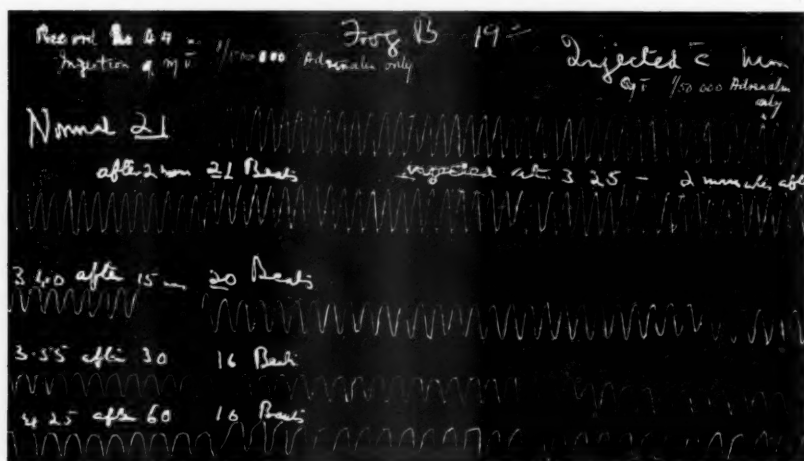
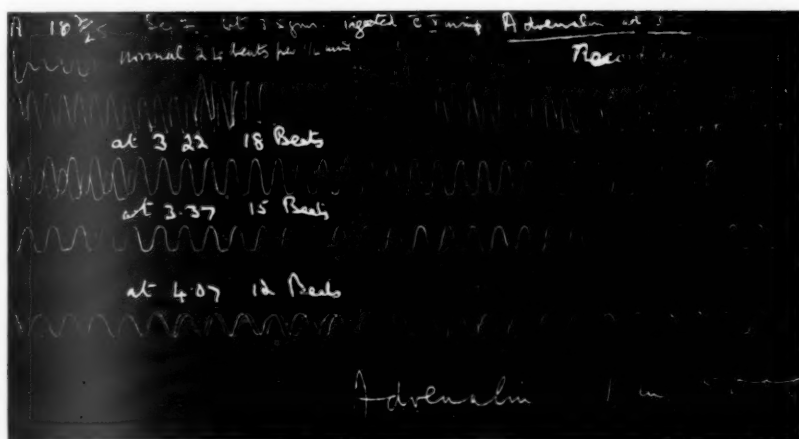
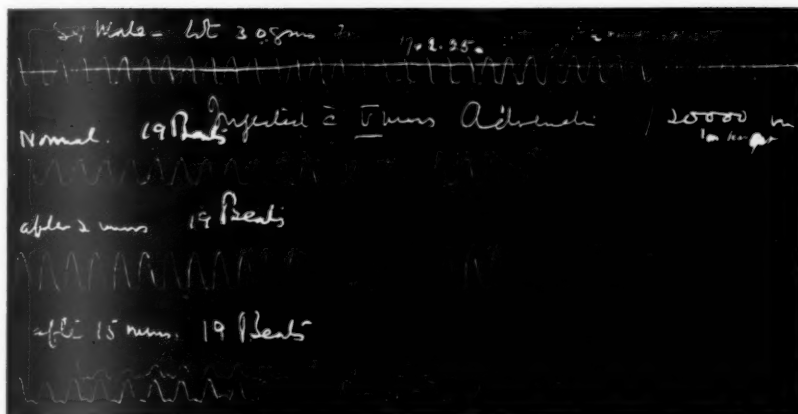
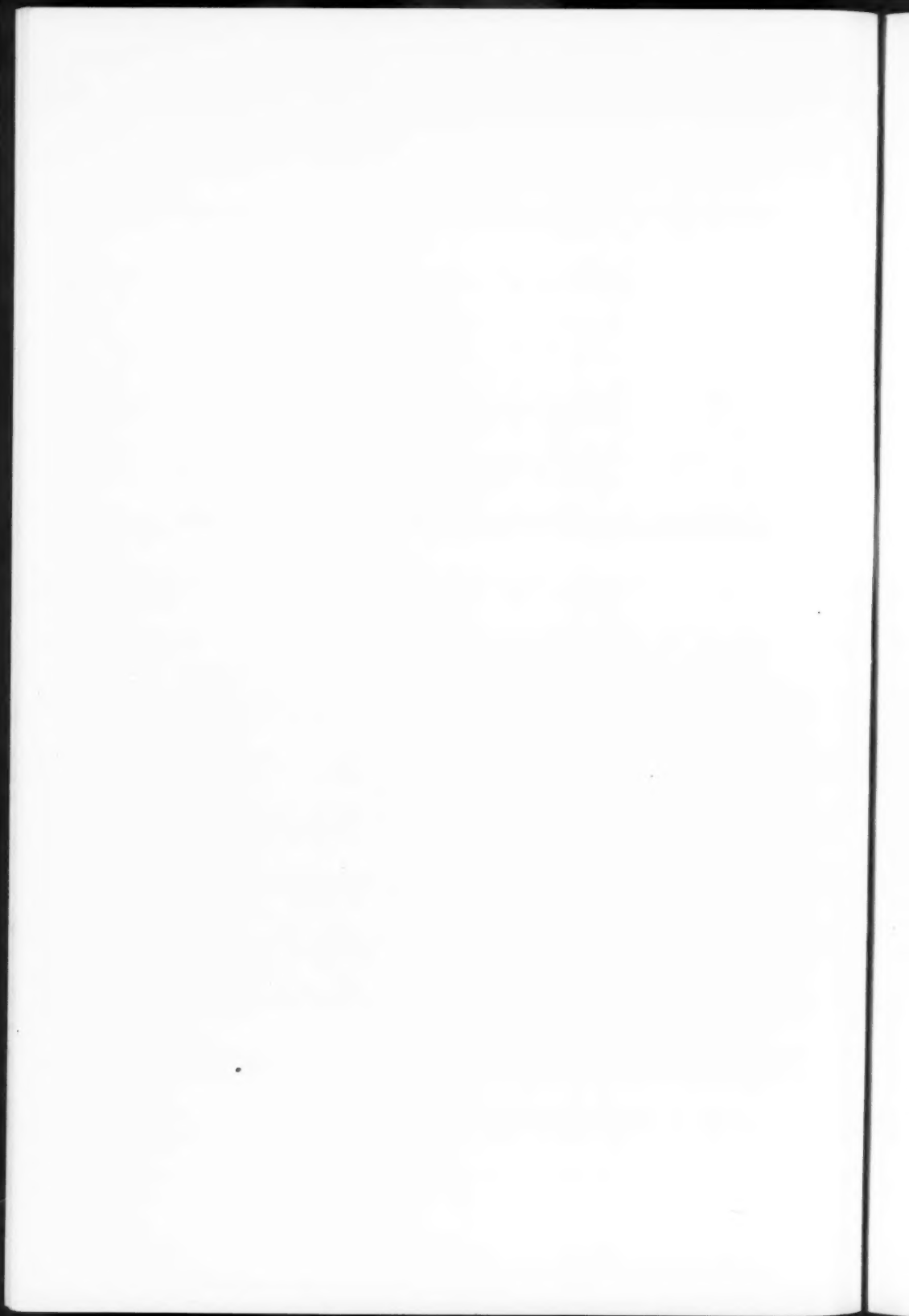


FIG. 5.



FIGS 5, 6, 7.—Show heart failure following injection of solutions of adrenalin of increasing concentration. (Records No. 44, 45, 46.)



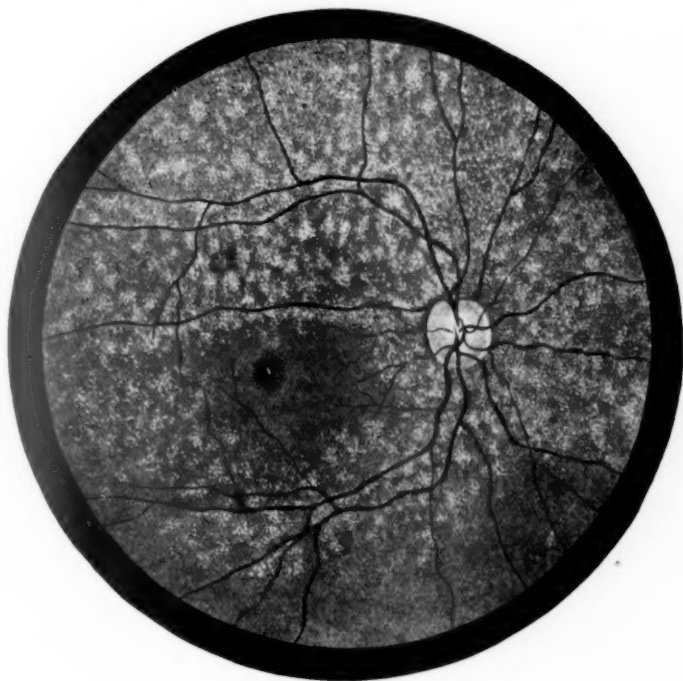
Section of Ophthalmology.

President—Mr. A. L. WHITEHEAD, F.R.C.S.

Case for Diagnosis.

By F. A. WILLIAMSON-NOBLE, F.R.C.S.

PATIENT, a female, aged 45. When I first saw this case, the fundus appearances suggested the diagnosis of *retinitis punctata albescens*. The appearances are similar in the two eyes, and are shown in the drawing which depicts the condition present in the right eye. In the left eye, however, there was no bright spot at the macula. It is a little difficult to say whether this



spot is due to an increased foveal reflex, or to the development of a spot similar to those in the mid-periphery. The peripheral part of the fundi is slightly albinotic, and it is possible here to make out some of the retinal pigment, but the change is not more marked than that which one frequently sees in the periphery of an ordinary albinotic fundus.

An interesting feature is that the colour of the spots is the same as that of the peripheral fundus, also that the spots are absent in the periphery. The fields are full, there is no night-blindness, and the vision of each eye with correction and a dilated pupil is $\frac{6}{6}$ full; I think it possible that with a contracted pupil patient would manage $\frac{5}{6}$; so it seems unlikely that the condition can be retinitis punctata albescentis.

I show on the screen a drawing of a case of this disease, described by Healy in the *British Journal of Ophthalmology*, for January, 1921. You will see that the distribution of the spots is somewhat similar with regard to the mid-peripheral zone, but there is general thinning of the retinal pigment layer in the central part of the fundus, whereas in this case the albinotic change is in the periphery. Healy's case also had definite night-blindness and contraction of the fields.

Another possibility was that this condition might be due to multiple colloid excrescences of the membrane of Bruch. George Coats published such a case in the *Transactions of the Ophthalmological Society*, vol. xxxi, in which the ophthalmoscopic findings were verified by subsequent pathological examination. The condition of the fundus is shown in this drawing. Mr. Coats described the affected areas as having a flat, scaly appearance, which showed a distinct glitter on movement of the light, and seemed to be a little raised above the level of the fundus. The patient, when first seen, had been suffering from bilateral glaucoma for nine months, and the vision in the right eye was reduced to perception of fingers at 3 ft., and in the left eye to perception of light. The appearances do not, except superficially, resemble those in the present case, in which the dots are much smaller, do not glitter, and do not appear to be raised above the general level of the fundus.

Mr. Eason saw this case in 1912, and has kindly looked up his notes. He has no record of any abnormality of the fundus, though he says that such evidence is only of negative value.

Summing up, then, we have a symmetrical change in the two fundi consisting of numerous yellowish-white dots in the mid-peripheral zone, producing no symptoms. The patient has a negative Wassermann reaction and the urine is normal. I do not see how such an extensive change could be inflammatory and yet produce no loss of vision, and would suggest, as a possible explanation, that the dots represent small localized losses of the retinal pigment epithelium, a sort of negative freckling.

DISCUSSION.

Dr. RAYNER BATTEN showed fundus pictures of two cases of his own in which the appearances somewhat resembled those in the present patient. There was the same kind of grouping, with a rather different character of spot. The spots were quite superficial, and there was no accompanying defect of vision.

The first of the cases had been under observation more than twenty years, and in that time there had been no material change. The spots were closer, but there was no evidence of any inflammatory change having taken place. The condition was symmetrical in both eyes.

In the second case also the condition was more or less symmetrical, but here it was complicated by opaque nerve fibres. In this patient, too, vision was full and normal, and there was no evidence of disease. This case he had had under observation only a short time, and therefore was unable to say whether or not it was progressive.

Mr. A. L. WHITEHEAD (President) asked whether Dr. Batten regarded his own cases as congenital, or whether he had any idea as to the origin of the spots.

Dr. BATTEN (in reply to the President) said that he thought the dots were congenital. Having had the first of the cases under observation for so long a time, he

could not think the appearance was associated with any disease. He did not consider that there were any peripheral changes.

Mr. MALCOLM HEPBURN said he thought these cases were generally instances of hyaline degeneration of the membrane of Bruch. As a rule the degeneration sites were surrounded by a small area of pigment. He (Mr. Hepburn) did not see much pigment in the case now shown, but pigment was not a necessary accompaniment of this condition, and the fact of the smallness of the amount in the present case did not militate against the diagnosis he suggested. He (the speaker) did not see why there should not be similar degenerations in any other part of the fundus; and if careful observation were made in a large number of cases, it would often be found. His view on this case was supported by the fact that no effect had been produced on the vision.

Mr. WILLIAMSON-NOBLE (in reply) said that with regard to distribution along the nerve fibres, the dots in this case were deep to the retinal vessels; in one or two places, retinal vessels could be seen passing over dots. Therefore they were not in the nerve fibre layer: they might be in the retina. There was one group of spots in the right eye in which there was a surrounding pigmentary disturbance, but that was the only part of the fundi which showed a change of this kind.

A Modification of Elliot's Scotometer.

Shown by FRANK JULER, F.R.C.S.

Mr. JULER said that he considered Elliot's scotometer was one of the best instruments available for examination of the central visual field. Its drawback was that it required the services of an additional person to record the readings, as the indicator was at the back of the instrument. With a view of overcoming this disadvantage, he asked Messrs. Weiss to fix a rim projecting round the edge of the rotary screen, and mark it in angular degrees visible from the front. With this, one could take the readings on the rotary screen in the ordinary way, immediately read off the angle at which one was working, and make the record on the chart at one's side. This modification made the scotometer a "one-man" instrument.

Mr. R. PICKARD (Exeter) said that in his practice he had been for thirteen years using a screen of his own modification which made the scotometer a one-man instrument. He had a canvas screen, 2 metres square, painted in dead black, and the tangent circles were also painted in black, but slightly raised above the surface, so that they could be seen. One could mark the extreme periphery of the field with a minute coloured chalk dot, then the field could be recorded on the chart. Examination, including that of the blind spot, using a 3 mm. and a 1 mm. object at a metre distance, could be done in that way in ten minutes.

On the Relationship between Subarachnoid and Intra-ocular Hæmorrhage.

By GEORGE RIDDOCH, M.D., and CHARLES GOULDEN, F.R.C.S.

I.—Dr. RIDDOCH.

Introduction.

THE not infrequent occurrence of hæmorrhage into the sub-arachnoid space within the skull has long been known, but up till recent times mainly as a cause of death in cases in which an incorrect diagnosis had been made during life. It was not associated in the minds of clinicians with any distinctive symptomatology, and in consequence the belief was generally held that its recognition was almost impossible at the bedside and its outcome usually fatal. But after the introduction of lumbar puncture by Quincke over twenty years ago it was

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found that the presence of blood in the spinal fluid under certain circumstances was a clear indication of bleeding into the subarachnoid space. Since then many important papers have been devoted to the subject, but our knowledge of it is by no means complete.

Ætiologically there are three main groups of cases: subarachnoid hæmorrhage from (1) traumatic rupture of meningeal vessels, (2) primary intracerebral hæmorrhage, the blood usually bursting through the brain substance, (3) non-traumatic rupture of a meningeal vessel or aneurysm.

Subarachnoid hæmorrhage from a ruptured aneurysm of one or other of the basal arteries is in some ways the most interesting group and the one to which most attention has recently been directed. Symonds [24], especially, has added much to our knowledge of the clinical picture which is often so distinctive that a diagnosis of primary non-traumatic subarachnoid hæmorrhage can sometimes be made with reasonable certainty even without the aid of lumbar puncture.

Such cases often occur at an age when intracerebral hæmorrhage is not very common. According to Ingvar [23] the vascular lesion may be due to inflammatory or degenerative arterial disease, but commonly it is the result of rupture of a so-called *congenital* aneurysm of one of the basal arteries (Turnbull [25], Fearnside [22], and Symonds [24]). Although a temporary rise of blood-pressure may be the immediate cause of bursting such an aneurysm, arterial hypertension and arterio-sclerosis is, as in our cases, often strikingly absent.

Bleeding into one or both eyes is a frequent but not constant feature of subarachnoid hæmorrhage from rupture of an intracranial aneurysm. Four examples of this association have been observed by us and form the basis of this paper.¹ One only of the four patients died and came to autopsy, and an eye was removed and examined histologically in order to ascertain the means by which the intra-ocular hæmorrhage occurred.

Illustrative Case.—Sub-arachnoid hæmorrhage from ruptured aneurysm of the right intracranial carotid at the origin of the middle cerebral artery. Hæmorrhage into both eyes. Death after thirteen days. Autopsy. Microscopical examination of right eye.

L. H. Reg. No. 10584/1924. Charles N., aged 44, skin-broker, was admitted to the London Hospital under the care of Mr. Russell Howard on February 14, 1924. His previous health had been good except that he was believed to have had sunstroke in Palestine.

In the evening of February 14, 1924, he was following some friends upstairs in his house when he suddenly fell down. He did not lose consciousness and managed to walk up to his room. A little later his friends heard him fall and found him lying on the floor unconscious. At short intervals he had three fits, in each of which he foamed at the mouth and was convulsed and cyanosed. In the second fit there was opisthotonos.

He was taken to the London Hospital the same evening and on admission was in a state of "cerebral irritation." He was conscious and answered questions more or less intelligently, but he resented being disturbed and examined. His pupils were of medium size, equal, and reacted to light and on accommodation. His ocular movements were full. All his tendon-jerks were exaggerated and an extensor plantar response was obtained on both sides. All voluntary movements were carried out well and co-ordination was good. Sensation appeared to be unimpaired. His heart, lungs and abdomen were clear and the blood-pressure readings were 140/80; temperature, 97° F.; pulse 100, full and strong; respirations, 24.

On February 15, his pulse was slow (52 to 64), but his blood-pressure had not altered.

On February 17, his muscles were flaccid, his tendon jerks were difficult to evoke and his plantar reflexes were normal.

¹ As the paper is to appear in full in the *British Journal of Ophthalmology* one case only will be reported here.

On February 19, five days after the onset, his temperature rose to 100° F.; pulse 56; respirations 22.

On February 20, he tended to "screw up" his left eye and saw double on looking to the right. His tendon reflexes were absent.

On February 21, temperature 99.2° F.; pulse 72; respirations 22. The diplopia had disappeared, but otherwise his condition had not changed.

On February 22, temperature 100° F.; pulse 80; respirations 24.

On February 24, his temperature had dropped to 99° F.; pulse 70. He had difficulty in swallowing, was very drowsy and could only be roused with difficulty. There was weakness of voluntary movement of the left side of his face and his tongue was protruded to the left. His pupils and ocular movements appeared to be normal. No tendon-jerks could be obtained, and for the first time there was incontinence of urine. Blood-pressure was 140/90.

On February 25, his condition had not altered except that the facial palsy was less and his blood-pressure had risen to 170/105.

On February 26, he was examined by one of us and the following note was made. He was stuporose and cyanosed, his breathing stertorous, grunting, and about thirty-four to the minute, and his pulse 114. Temperature 102° F. He could not be roused. Both fundi showed hæmorrhages in the neighbourhood of the discs, which were definitely swollen. In the left fundus there were two large crescentic hæmorrhages lying alongside the inferior veins close to the margin of the disc. His pupils were equal and reacted well to light; the reactions on convergence could not be tested because of the patient's mental state.

There was a flaccid, left hemiplegia. All tendon jerks and the abdominal reflexes were absent and both plantar responses were extensor. He responded by grimacing to painful stimulation on either side. There was incontinence of urine. His heart's apex beat could not be felt, nor could his heart's sounds be heard on account of his noisy breathing. His urine contained albumin, and there were signs of congestion of the base of each lung.

On lumbar puncture, a small quantity of uniformly pink fluid was drawn off. It was found to contain blood and a slight excess of lymphocytes. The Wassermann reaction was negative.

Next day, February 27, his temperature rose to 104.2° F., pulse 132 and respiration 62, and he died on the thirteenth day of his illness.

Autopsy.—There was no conspicuous flattening of the cerebral convolutions. A considerable amount of red-brown blood and a little blood-clot, altogether measuring a few drachms, was found in the subdural space. In the posterior part of the longitudinal fissure there were two symmetrical areas of subarachnoid hæmorrhage. Other small subarachnoid collections of blood were present, scattered over the cerebral convexities. A large amount of red and brown blood-clot lay under the arachnoid in the right Sylvian fissure and a smaller amount in the left Sylvian fissure and anterior part of the median fissure and cisterna basalis. A saccular aneurysm, one centimetre in diameter, projected backwards from the right intracranial carotid at the origin of the right middle cerebral; it was ruptured at its inner pole. No other aneurysm or arterial abnormality was found. No blood was seen round the optic nerves in the optic foramina, but within the orbits the sheaths of the optic nerves were distended with blood. The right eyeball was opened and multiple hæmorrhages were seen round the margin of the optic disc, the two largest lying upon vessels. The left eyeball was not examined.

The lungs were the seat of purulent bronchitis and of broncho-pneumonia. There was no cardiac hypertrophy and only a moderate amount of atheroma. The liver was large, with congestion in the centre of the lobules, and the spleen was flabby.

The left kidney was grossly atrophied and fibrotic, and its pelvis was cystic and projected inwards. The upper part of the ureter was very contracted and apparently impervious. There was compensatory hypertrophy of the right kidney. No abnormality in the renal vessels was found.

The Wassermann reaction in the blood, post mortem, was negative.

The Clinical Picture of Rupture of a Basal Aneurysm.

Before briefly discussing the clinical manifestation of subarachnoid hæmor-

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rhage from rupture of a basal aneurysm it might be of interest to mention certain symptoms that have been present in our cases before the first seizure.

Patients who have bled into the subarachnoid space from a ruptured basal aneurysm have sometimes had excellent health up to the time of the attack. But careful inquiry into the previous medical history in such cases often elicits the fact that more or less distressing symptoms have been present for a long time. Thus headache, giddiness, defective memory for recent events, disturbance of sleep, &c., may have been troublesome when arterio-sclerosis is marked. But even in the absence of gross, general arterial disease symptoms pointing to some intracranial disturbance appear to be not uncommon.

For example, paroxysmal *headache*, often severe, has been complained of in several instances and may resemble migraine. Thus the patient in *Case IV* of our series for years before his first seizure suffered from recurrent headache so intense that he was temporarily incapacitated for work. The headache tended to be brought on by exertion or prolonged stooping, was associated with tinnitus and frequently culminated in vomiting. One of Ingvar's [23] patients was subject to headaches that were for long looked upon as migrainous.

Tinnitus, usually continuous with periodic exacerbations, is sometimes referred to in the literature and the patient in *Case IV* was troubled for years with noises in his head that waxed and waned in time with his pulse. The association of aggravation of the noise with headache in his case has just been mentioned.

The symptoms complained of in *Case III* are of particular interest. The patient was aged 53 at the time of his seizure. For as long as he could remember he had been periodically subject to attacks of epistaxis, the bleeding having invariably been preceded by lethargy, frequent yawning, and a sensation of heat on the top of his head. As soon as his nose began to bleed these symptoms disappeared and he felt, as he put it, ten years younger. His father, who died at 89, soon after he had a stroke, had suffered from recurrent epistaxis for the greater part of his life, and the patient's two children are also nose-bleeders.

The onset of the seizure is always sudden.

From inquiry into the circumstances under which a seizure has developed it is usually clear that the aneurysm had burst at a time when the patient was carrying out some form of activity sufficient temporarily to raise his blood-pressure. In *Cases II* and *IV* the patients were straining to empty their bladder early in the morning, when they were suddenly seized with severe headache and rapidly became unconscious. In *Case I* the attack occurred in the evening, while the patient was walking upstairs with some friends; without any warning he suddenly fell down but did not lose consciousness and was able immediately to rise and ascend the stairs unaided. A little later he was heard to fall in his room and was found lying unconscious on the floor.

The mode of onset in *Case III* is interesting. It will be remembered that the patient suffered from epistaxis and that the bleeding was always preceded by sleepiness, yawning and sensations of heat on the top of his head. In August last these symptoms appeared and as usual persisted for some time. One evening he went to bed thinking that his nose would soon begin to bleed, but he awoke in the morning with severe occipital headache and rapidly became unconscious. Although this patient's usual blood-pressure was within normal limits there is little doubt but that the periodic epistaxis was a measure of safety, for after each attack he felt much better in health. It is highly probable that on this occasion if his nose had bled as usual he would have escaped the seizure from subarachnoid hæmorrhage that developed.

The coma into which the patient passes may rapidly become profound. All the signs of cerebral and medullary compression appear and he may die

within a short time. In such cases the tear in the aneurysmal wall is large and the clinical picture is indistinguishable from that of fatal intracerebral hæmorrhage.

More characteristic however is a clinical state that, apart from the onset, resembles that of meningitis. There is an early phase of more or less deep coma, during which the patient may be convulsed. He then passes into a condition in which there is clouding of consciousness, disorientation and delirium. He talks in an excited, rambling fashion, often about his work but may answer questions more or less sensibly. He complains bitterly of headache and pains in the back of his neck. Vomiting sometimes occurs and there is often considerable restlessness, the patient tossing in his bed from one side to the other. The pulse-rate may be normal or a little slower than usual. Later, pains in his limbs appear and he may lie with the limbs curled up, resenting interference of any kind. In consequence it is difficult to get him to co-operate in the clinical examination. Signs of local disturbance of neural function are always present but are usually slight. The ophthalmoscopic appearances when abnormalities are present will be described later. One or other of the cranial nerves, most commonly the third or sixth, may be affected, probably as the result of local pressure by blood-clot or a jet of blood impinging on them. Frequently there are bilateral signs of pyramidal disturbance, such as diminished abdominal reflexes, increased tendon-jerks and an extensor plantar response on each side, but usually voluntary power is not seriously impaired. It is impossible, as a rule, to test sensibility in a satisfactory manner, but in none of our cases has it been obviously affected. There may be retention of urine, but complete control of bladder function is not uncommon.

An almost constant sign is rigidity of the neck, and Kernig's sign is often positive on both sides. After the first few days the temperature is often slightly raised, this rise being attributed by Froin, probably correctly, to hæmolysis.

Such, in brief, is the clinical picture presented during the early stage in a moderately severe case of subarachnoid hæmorrhage from a ruptured basal aneurysm. Examples, however, are met with in which the initial disturbance of function is less marked. Although consciousness is probably always affected to some extent, there may be no deep coma or other evidence of gross compression of the brain.

One invariable sign of great importance has yet to be mentioned, namely, blood in the spinal fluid. Froin, in 1904, fully described the characteristic changes in the cerebro-spinal fluid drawn off by lumbar puncture. Of these three only need be mentioned, since they are sufficient to differentiate such a fluid from one that is sanguineous as the result of accidental puncture of a vein. (1) If three successive samples are taken they all show complete admixture of blood and spinal fluid; (2) no coagulum appears when the specimen is allowed to stand; and (3) when the red cells sink to the bottom the supernatant fluid is coloured yellow or orange-brown.

In regard to the course of the complaint, recovery from at least the first seizure is common. After two or three weeks consciousness returns, palsies, when present, disappear, and the reflexes become normal. Sometimes, however, slight mental defects, such as unreliable memory for recent events, may persist, but often the general recovery is remarkably complete. When, however, vitreous or subhyaloid hæmorrhage has occurred in one or both eyes, vision seems to be more or less permanently impaired. Recurrent seizures are common, and one of them may prove fatal. One patient (*Case IV* in our series) has, however, had at least four in the last three and a half years, and is still alive.

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II.—Mr. GOULDEN.

SOME POINTS IN THE ANATOMY OF THE OPTIC NERVE AND ITS COVERINGS.

Within the skull the optic nerve lies in the subarachnoid space covered only with pia mater prolonged upon it from the brain. When it reaches the optic foramen it gains two more coverings, derived also from the meninges of the brain, and all three sheaths accompany the nerve until it reaches the globe of the eye. In this way the intra-canicular and intra-orbital portions of the nerve are covered by three concentric meningeal layers.

(1) *The Pia Mater.*—This is the internal layer, thin and delicate, which constitutes the true neurilemma. It is a direct continuation of the pia mater, and is covered on its deep surface by a thin layer of neuroglia. Connected with the pial sheath on its inner surface are the septa that pass into the optic nerve, dividing it into two compartments. Blood-vessels pass into the nerve from the pial sheath along the septa.

(2) *The Dural Sheath.*—This is much thicker and more resistant than the inner sheath, having a fibrous structure. It becomes continuous with the dura mater of the brain along the circumference of the optic foramen.

(3) *The arachnoid sheath* is a very thin and delicate structure, and is the middle layer lying between the dura and pia mater.

The Serous Spaces of the Optic Nerve.

(1) *The subdural or arachnoid space* represents the subdural or arachnoid space of the brain. It lies between the dura and arachnoid mater, and is merely a slit-like space lined with endothelium divided into many compartments by a series of fine trabeculæ.

(2) *The subarachnoid space* lies between the arachnoid and pia mater, and is continuous with the subarachnoid spaces of the brain, and is crossed in many places by trabeculæ connecting the pia and arachnoid. In this way the subarachnoid space of the optic nerve is directly continuous with the cisterns at the base of the brain, with their enclosed blood-vessels.

The Central Vessels of the Retina.

The *central artery* of the retina, a branch of the ophthalmic, pierces the nerve on its infero-medial surface about 10 mm. from the sclera. It runs for a short distance upon the deep surface of the pial sheath, and gradually reaches the centre of the nerve, accompanied by the central vein, carrying with it a prolongation of the pial sheath.

The *central vein* has the same relations as the central artery, and opens sometimes into the superior ophthalmic vein, but more often direct into the cavernous sinus.

The Anterior Termination of the Optic Nerve.

The nerve, its fibres having lost their myelin sheaths and correspondingly reduced in volume, pierces the sclera by many perforations (the so-called lamina cribrosa), and becomes continuous with the retina.

The following description will show the arrangement of the sheaths of the nerve at this point:—

The *dural sheath* bends outwards at an angle of 110°, and becomes continuous with the outer layers of the sclera so intimately that no line of separation is visible.

The *arachnoid sheath*, attached to the deep surface of the dural sheath, also becomes continuous with the sclera, the subdural space ending in a cul-de-sac.

The *pial sheath*, when it reaches the sclera, also turns outwards like the dural and arachnoid sheaths, becoming continuous with the inner layers of the fibres of the sclera. A few of the fibres, the most internal, enter into relationship with the choroid. The subarachnoid space thus also ends in a cul-de-sac, which is situated in the middle of the sclera on a plane posterior to the lamina cribrosa.

OPHTHALMOSCOPIC SIGNS.

When considering the ophthalmoscopic signs in cases of hæmorrhage into the optic nerve sheath, one must be careful not to confound them with changes that may result from injury to the optic nerve due to fracture of the base of the skull in the neighbourhood of the optic foramen—such an injury may or may not be associated with effusion of blood into the nerve sheath.

The cases discussed in this paper are those in which there is no direct injury to the optic nerve by any such object as a spicule of broken bone, or any damage to the nerve by some sudden strain or crush such as we may imagine is caused when there is fracture in the region of the orbit and optic foramen.

The earlier observers described the ophthalmoscopic signs of hæmorrhage into the optic nerve sheath as being similar to those following embolism of the central artery, that is, thread-like arteries, red spot at the macula followed by primary optic atrophy, and later a pigmentary change around the disc which was supposed to be due to staining with blood pigment. In no instance did they carry out a pathological examination, and it is improbable that the appearances they described were due to hæmorrhage into the nerve sheath without other injury to the nerve. For hæmorrhage into the nerve sheath to result in pressure upon the central vessels of the nerve sufficient to give rise to the appearances of embolism of the artery and sufficiently prolonged to produce optic atrophy, it would be necessary for pressure within the nerve sheath and subarachnoid space of the meninges of the brain to be higher than the general blood pressure—a state of affairs that is not consistent with continuance of life. As to the peripapillary staining, it is also extremely unlikely to be due to blood pigment, judging from our experience with hæmatomas elsewhere in the body. The deep pigmentation described and figured by Nicod [12] from a case of Rollet suggests pigmentation from other causes, and is most likely of congenital origin. That pigmentation from the blood should occur around the disc is also improbable, as we shall see when discussing the pathological findings in cases of hæmorrhage into the nerve sheath. Again, in no case in which the ophthalmoscopic investigation has been followed by pathological examination has the appearance of embolism of the central artery or peripapillary pigmentation been found associated with subarachnoid hæmorrhage and hæmorrhage into the optic nerve sheath.

In all the cases in which ophthalmoscopic examination has been followed by pathological investigations, the appearances are so similar that it is possible to describe a characteristic clinical picture of the fundus that is constant in cases in which blood has spread into the optic nerve sheath from a subarachnoid hæmorrhage, whether spontaneous (as in the cases here reported) or due to fracture of the skull.

Papillædema.—This is the most common abnormality and is mentioned in almost all the cases, the only exceptions being the case reported by Priestley Smith and three recorded by Elschnig, in the reports of which it is stated that the optic discs were normal.

The papillædema is usually bilateral, corresponding to hæmorrhage into both nerve sheaths. Its rapidity of onset is also remarkable. Uhthoff [20] states that he has seen papillædema half an hour after hæmorrhage into the subarachnoid space and well marked in five hours after the hæmorrhage. It is usually slight in amount, but very severe papillædema has been reported (Rollet) with intense swelling and very small arteries. The veins are always engorged and tortuous.

Retinal Hæmorrhages.

In the majority of cases there are retinal hæmorrhages. They are usually

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small and are often near the disc, but they may occur at a considerable distance from it, as was seen in one of our cases. They resemble retinal hæmorrhages seen in other conditions.

Vitreous Hæmorrhages.

These are stated to be rare, but, curiously, they were found in the cases here reported and were so profuse that an examination of the details of the fundus was impossible in the early stage, and only when the blood had to a large extent disappeared could the whole of the fundus be seen. In one case it was thought that there had been a large pre-retinal hæmorrhage as there was a good deal of pigmentary change over a wide area around and including the macula, and, more peripherally, there was one fairly large hæmorrhage along the superior temporal vein that had not been absorbed in four months but had disappeared four weeks later. In the other eye the hæmorrhage into the vitreous was so profuse that a view of the fundus was impossible.

Retinal Vessels.

Disease of the retinal vessels has not been noted in the cases reported and in our own cases they appeared to be healthy. Although papillædema and retinal hæmorrhages are seen in elderly patients who have hæmorrhage into the optic nerve sheath, many of the cases have occurred in quite young or middle-aged individuals, so that the absence of vascular degeneration is not surprising. Apart from the cases of spontaneous hæmorrhage into the subarachnoid space, so many cases have occurred of hæmorrhage following injury in quite young people, that intra-ocular hæmorrhage cannot always be due to disease of the blood-vessels or abnormality of the blood. In fact, as we shall see later when discussing pathology, the association of subarachnoid and intra-ocular hæmorrhages can be reasonably explained.

Ophthalmic symptoms in the early stage are not mentioned in any of our cases or those already reported, and this is scarcely remarkable, since consciousness is affected and often completely abolished whether the cause of the subarachnoid hæmorrhage be spontaneous or traumatic. When consciousness has been recovered the patients complain of defective sight, which is extreme in those cases in which the vitreous is full of blood.

PATHOLOGICAL EXAMINATION OF AN EYE IN CASE NO. 1.

The pathological examination was carried out by Professor Turnbull in the Pathological Institute of the London Hospital. This examination was very complete, and many hundreds of serial sections of the posterior part of the eye and optic nerve were cut. We would express our thanks to him for the trouble he took and for his help in the investigations.

The sheath of the nerve was tensely filled with blood-clot, which gave the sheath the appearance of a distended vein. The posterior half of the eye was removed with about two centimetres of the nerve attached, and was then placed in formalin. The main piece of nerve was divided half a centimetre behind the globe, so that sections of the eye could be made longitudinally, and of the nerve transversely.

The eye was embedded in celloidin, and the nerve in paraffin. The sections were stained in hæmatoxylin, eosin, and different reagents, to show various points.

Microscopical Examination of Longitudinal Sections of an Eye in Case I.

Under a low power the optic nerve sheath was seen to be distended with blood-clot, which was most obvious at the anterior extremity of the nerve, where there was an

ampulliform dilatation of the sheath. Under a *higher* power it was possible to show that there was no infiltration of the nerve itself with blood, but that the blood-clot was exactly delimited by the pial sheath. In the subarachnoid space individual red blood corpuscles were visible, and some infiltration of the dural sheath by blood corpuscles could be seen. The sclera was not infiltrated with blood, and no corpuscles could be found surrounding the vessels within the nerve.

The *papilla* was slightly swollen.

Retinal hæmorrhages could be seen here and there at some considerable distance from the papilla.

The Transverse Sections of the Nerve.

The subarachnoid space was full of blood. The blood did not pass into the pial sheath, and no sign of hæmorrhage could be found within the nerve.

The *central vessels* within the subarachnoid space were surrounded by blood-clot, and there was infiltration of the adventitial coat. This infiltration was also seen after the vessels had pierced the dural sheath. There was no infiltration of the vessel sheath within the nerve.

It was not possible to show by section an obliteration of the vessels by compression.

PATHOGENESIS OF THE INTRA-OCULAR HÆMORRHAGE.

How are we to connect the simultaneous appearance of a subarachnoid hæmorrhage with hæmorrhage into the retina or vitreous, and the development, within a short time, of papilloedema?

Histological investigations have demonstrated the absence of any direct continuity between the hæmorrhage into the optic nerve sheath and the intra-ocular hæmorrhage, a result that was to be anticipated from anatomical considerations. Further, experimental investigation has shown that there is no connexion between the subarachnoid spaces of the brain and optic nerve on the one hand and the lymphatic spaces within the eye on the other. Schwalbe first showed the connexion between the subarachnoid space of the brain and the sheath of the optic nerve, and demonstrated by injections under low pressure that fluid injected into the subarachnoid space of the brain did not pass further forward than the cul-de-sac at the anterior end of the optic nerve sheath. Schmidt Rimpler injected coloured fluid under pressure into the subarachnoid space of dead animals, and was able to make the injection penetrate the nerve itself as far as the papilla, and he described lymphatics piercing the lamina cribrosa. Subsequent investigations by Schwalbe and others have failed to confirm this, and so the results of Schmidt Rimpler cannot be accepted. It is to be noted that the injections were made by him *under pressure* of a degree very different to that found in life.

Sicard has used much more delicate procedures, and has definitely proved that these supposed lymphatic connexions do not exist. He injected grains of Chinese ink into the subarachnoid space of living animals, and showed by subsequent investigation that the grains never penetrate the pial sheath of the nerve, but that they pierce the dural sheath so as to reach the neighbouring cellular tissue in great numbers. At the point where the optic nerve pierces the sclera the granules collect in the cul-de-sac at the anterior end of the optic nerve sheath, where they may be seen to pierce the dural sheath and enter the retrobulbar tissue. On the other hand none pierced the thickness of the sclera, papilla, retina or choroid. The point was proved by decolorizing the choroidal and retinal tissues with oxygenated water; this process always left them quite free from granules of Chinese ink.

Again, pathological investigations in cases of hæmorrhage into the sheath have always proved the pial sheath and optic nerve to be free from blood

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corpuscles, whereas the dural sheath and the sheaths of the central vessels in the subarachnoid space, during their passage through the dura, and also outside the dura, are often distinctly infiltrated with blood corpuscles.

We may thus dismiss the suggestion that blood finds its way into the eye by piercing the pial sheath of the nerve, and in that way passing through the lamina cribrosa into the eye. It must also be borne in mind that retinal hæmorrhages in these cases are not necessarily at, or even near, the disc.

It will have been noticed that the ophthalmoscopic appearances conform very much with our ideas of venous engorgement of the retina, which is seen in its most marked instance in cases of thrombosis of the central vein of the retina; namely, papillœdema, enlarged veins and retinal hæmorrhages. Since papillœdema has been seen within half an hour of the onset of the subarachnoid hæmorrhage (Uhthoff [20]), it is obvious that something has occurred within the optic nerve sheath at least soon after the subarachnoid hæmorrhage, and this is most assuredly a distension of the sheath of the nerve with blood, such as we see in pathological examination.

It has been shown by Dupuy-Dutemps [5] and by Paton and Holmes [14] that when the optic nerve sheath is distended in cases of raised intracranial pressure with papillœdema, the lumen of the vein in the subarachnoid space of the nerve is flattened, and thus much narrowed. Although experiments by injecting non-toxic and non-irritating fluids into the subarachnoid space of the brain have failed to give rise to papillœdema, owing to the rapid balancing of the intracranial pressure by dissemination and excretion, Deutchmann produced the exact appearance of papillœdema in an animal by the injection of agar-agar directly into the optic nerve sheath, thereby causing a *prolonged* compression of the central vein of the retina in the subarachnoid space.

In sections of the nerve in these cases of hæmorrhage into the nerve sheath, the œdema of the nerve is found to end abruptly at the point at which the vein leaves it, so that there is marked œdema between the papilla and the exit of the vein only, behind which point the nerve is healthy.

All these considerations point to the cause of the trouble being interference with the venous return from the retina and optic nerve. We have seen that distension of the subarachnoid space of the optic nerve causes compression of the vein (Dupuy-Dutemps [5], Paton and Holmes [14]), and this is doubtless due, not only to the raised pressure in the space, but also to the traction on the vein in the vaginal space caused by separating the dural and pial sheaths. The obstruction is sudden, thus causing appearances similar to those seen in thrombosis of the central vein, although the results are not so serious as the obstruction is neither so complete nor so prolonged as in thrombosis.

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DISCUSSION.

Mr. LESLIE PATON said he had dealt with this subject at the recent Glasgow meeting of the Ophthalmological Society, and he was sorry this paper did not coincide with the publication of his own.

At the risk of repeating himself he would like to refer to one or two points :—

It was important to remember that not in all cases did papillœdema appear within a few hours; Unthoff had pointed that out, as also had Liebrecht, in his most valuable work. In his last paper, in 1912, Liebrecht had called attention to the fact that papillœdema might not occur in these cases until some time after the onset of the cerebral hæmorrhage, and he drew a distinction between the two types of papillœdema. When it occurred within a very short time of the hæmorrhage, the correct explanation was that given by Mr. Goulden, that it was due to the direct pressure of the hæmorrhage in the nerve sheath. There was a second type of papillœdema, which was not necessarily associated with direct pressure exerted by the hæmorrhage in the nerve sheath. Therefore, though there might be hæmorrhage in the nerve sheath, it was not necessarily under such high pressure as to produce changes in the disc head. This second type was due to the intracranial hæmorrhage acting like an ordinary intracranial tumour. In many cases in which the hæmorrhage was sufficiently severe to cause a rise in intracranial pressure, the patient was dead before any papillœdema could occur. But some of the patients did survive, and the intracranial hæmorrhage then acted in the ordinary way in which intracranial tumours behaved, raising intracranial pressure and causing papillœdema several days after the onset of the original hæmorrhage.

In his paper read at Glasgow, he (Mr. Paton) had called attention to a third type of intracranial hæmorrhage, one in which the papillœdema might occur many months after the hæmorrhage. This was the form which became encysted, forming a hæmatoma, and as the hæmatoma increased in size papillœdema occurred months after the original hæmorrhage.

Another point of great interest was that concerning the origin of sub-hyaloid hæmorrhages in these cases. He (the speaker) agreed with Mr. Goulden that many of the hæmorrhages which occurred in the eye were of the type which might take place in any form of raised blood-pressure in the sheath. But there were the extraordinary cases, such as he had described at the Glasgow meeting, having a huge subhyaloid hæmorrhage without any distension of veins at all; and when the hæmorrhage had subsided sufficiently to allow the disc to be seen again, there was no sign of papillœdema, yet there were obvious rings of hæmorrhage round the edge of the disc and round the edge of the physiological pit. Though it was difficult to believe in the continuity of hæmorrhage from the sheath into the subhyaloid tissues, if Mr. Goulden had seen those cases in which the hæmorrhage tracked from the edge of the disc to the huge pool below, he thought that he would admit the possibility of some sort of direct continuity. One could understand why the hæmorrhages cleared away from the lymph channels along an axial vessel: it was the main line of outflow of lymph into the vessel. Consequently if the patient remained alive, the hæmorrhage was likely to be cleared away from those vessels more readily than from other situations; hence, post mortem, there might be no sign of corpuscles along the perivascular lymph spaces, though it might have been the course which the hæmorrhage took originally to reach the subhyaloid region.

It was difficult to dogmatize in the absence of actual specimens. He much regretted that the occurrence of the War caused the destruction or loss of the specimens he had in his original collection, for there was an important one in 1904. The last case of the kind he had seen recovered completely, and though he had had a huge subhyaloid hæmorrhage, vision was now $\frac{5}{6}$ in each eye. He found it difficult to understand why veins and arteries should appear normal if the appearances were due to changes in the disc itself. Liebrecht's original suggestion was that traumatic hæmorrhages acted on the disc, causing big intravitreal hæmorrhages. These sheath hæmorrhages were, however, not necessarily a direct continuity between the intracranial hæmorrhage and the sheath hæmorrhage, but were a slight rupturing of the vessels at the foramen in the dura at that spot; that was so even when there was no bony fracture at the foramen.

Many other points of interest were raised by the authors, but he would not deal with them now.

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Dr. RIDDOCH (in reply) discussed the clinical distinctions between traumatic meningeal hæmorrhage on the cerebral convexity and spontaneous bleeding into the meningeal spaces at the base. He agreed with Mr. Paton that recovery of vision in cases of extensive hæmorrhage into the vitreous was sometimes surprisingly good.

Mr. GOULDEN (in reply) said that the possibility mentioned by Mr. Paton of direct continuity between the subarachnoid hæmorrhage and the hæmorrhage within the eye had never been proved. Many eyes had been examined in which hæmorrhages had occurred during the course of subarachnoid hæmorrhages, and in no case had such a continuity of hæmorrhage been found. The fact that after a large hæmorrhage had cleared away there was no sign of papillœdema was not surprising when it was considered that the original swelling of the optic disc was never very severe; on the other hand if the blood reached the interior of the eye by ploughing through the fibres of the optic nerve, then we would certainly meet with profound ophthalmoscopic changes.

Section of Ophthalmology.

President—Sir ARNOLD LAWSON, K.B.E., F.R.C.S.

Two Cases of Congenital Blepharophimosis.

By ROSA FORD, M.B.

THE cases occurred in a father, aged 34, and a son, aged 2 years 9 months.

Miss IDA C. MANN exhibited on the screen drawings showing the eye at various stages of intra-uterine life. The lid folds were showing by the fifth week, and had closed with epithelial fusion by the tenth week. The exhibitor considered that if the stage of development occurring at the tenth week were to persist, there would occur at birth true ankyloblepharon. Adhesion of the lids usually remained from the tenth week to the seventh month, and then broke down. In Dr. Ford's cases (especially in the man) there was an absence of the levator palpebrae superioris, this apparently having failed to split off from the superior rectus mass, since the superior rectus was itself present.

Cavernous Optic Atrophy and its Relation to Glaucoma.

By RANSOM PICKARD, C.B., C.M.G., M.S.

(ABSTRACT.)

THIS paper dealt with seven cases of cavernous optic atrophy without increase of pressure, one case of glaucoma with tension of 30 mm. being introduced as a contrast. One case occurred in a man, the remainder in women, the ages of the patients varying from 44 to 79. The cases in many respects resembled glaucoma, but in others there were marked differences. The symptoms may be summarized thus:—

(a) In all the cases one or both optic disc cups were distinctly enlarged and deepened, but two cases showed one of the two eyes affected to have a cup of average size; (b) the visual field was concentrically limited, especially for 1/1000; (c) central vision was variously affected from $\frac{6}{60}$ to $\frac{6}{6}$; (d) the blind spots were enlarged, in some cases into an annular scotoma.

The symptoms which distinguished the cases from glaucoma were:—

(e) No increase of tension: (f) a marked diminution in the field for red as compared with that for white in the same case.

There were no general nerve symptoms. It was argued that since the pressure was not increased, any alteration in the disc cup must be regarded as due to an alteration in the nerve itself. The name "cavernous atrophy" was given because the cases in their clinical aspect appeared to resemble the pathological appearances described by Schnabel under that name.

The important bearing of such cases upon cases of apparent glaucoma with low tension was emphasized, and the necessity of ascertaining the colour fields was insisted upon. Judging from the details of the cases here recorded, it was apparent that varying degrees of this condition might be met with. Should this occur coincidentally with increase of tension, the results upon vision and fields would be great, and the relief of tension by operation would not completely arrest the downward course of the case.

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DISCUSSION.

Mr. MALCOLM HEPBURN said he would be afraid to leave these cases of cavernous atrophy without advising operation, as the signs in this condition were so extraordinarily similar to those found in cases of chronic glaucoma.

Mr. G. H. POOLEY said that he went over a large number of charts of cases of chronic glaucoma and of other cases closely resembling the condition now described, which he had seen over a period of years. The only difficulty he felt in criticizing the paper was, that no history of the progress of the cases was given; yet the whole point of differentiation between the two classes of cases was as to whether they progressed or not. He had one particular case in which the fields exactly corresponded to those shown, the scotoma almost encircling the point of fixation-spot. He watched the case for some time, and as there was steady progress he performed an operation on both eyes. After the operation there was a little further progress, and he thought the operation, though surgically successful, had been a failure. Four years later, the patient still had $\frac{5}{8}$ vision in the better eye, and the field was unchanged, although she had used no other treatment in the meantime. Was that a case of cavernous angioma, or one of chronic glaucoma in which treatment for glaucoma had arrested the progress of the disease?

Mr. LESLIE PATON referred to one undoubted case he had seen of the kind described by Mr. Pickard. It might be useful as illustrating the length of time the condition could last. The patient was first seen in the year 1909, when he had approximately 9 D. of myopia in each eye. Vision at the time was $\frac{5}{8}$ with correction, and the fields and discs were normal. He next saw the patient in 1918. The sight had been failing, especially in the left eye, during the last five or six years. The patient had seen Mr. Priestley Smith in that time, who had told him that though the case in many ways resembled one of chronic glaucoma, and though a deep cup had developed, he did not think it was glaucoma. He had also been seen by many ophthalmic surgeons both in this country and on the Continent. When he was seen in 1918, the central vision in the left eye was still $\frac{1}{2}$, but his field was limited to a small area within the 10° circle. In the right eye, where the myopia had increased to just over 15 D., the central vision had gone down to $\frac{1}{12}$; this was probably partly due to the development of a certain number of lens-opacities. There was some limitation of the field in the lower temporal quadrant. His right disc showed very little change, but the left disc was atrophic-looking, with deep cupping. Later the field in the left eye became limited to 2° round the fixation point. In 1922, central vision had gone to less than $\frac{1}{60}$ in the left eye. In the right eye, owing to the increase of the cataract, his vision had gone down to $\frac{1}{6}$.

Another case of a similar nature had reached a certain degree of cupping. The appearance of the discs were those of glaucoma, but the fields were very little interfered with, and there was no progress in it during the last seven years during which he had been under observation.

His (Mr. Paton's) impression of those cases of so-called Schnabel's cavernous atrophy of the disc was that they were very rare. From his own experience he would certainly say that they were not nearly so frequent as Mr. Pickard seemed to suggest in his paper, and one should therefore be very chary of diagnosing them as such.

Mr. PICKARD (in reply) said that it was in trying to get at glaucoma in early stages that he stumbled across the cases he had described. Some of them in an early stage he had taken for glaucoma. While he was making up his mind as to what the cases really were, he used eserine, though he did not know what good it would do. In some of the cases the course was short, in others it was long; and it was impossible to tell beforehand how a case was going to turn out. Mr. Lang's case made no progress in fourteen years. He had seen cases of the kind which had been operated upon; in one the patient's vision was reduced to perception of fingers in the operated eye, and in the remaining eye there was a typical cavernous atrophy. He assumed that the eye which had done badly was in the same condition. He did not wish to convey the impression that he thought the cases were common, but he suggested that cases which appeared to be glaucoma, but had not much tension, should be reconsidered from this point of view.

Miners' Nystagmus: its Diagnosis and Origin.

By A. FREELAND FERGUS, M.D., LL.D.(Glasgow).

THE diagnosis of miners' nystagmus does not, in ordinary cases, present much difficulty. In addition to the movements of oscillation, movements which are not constantly present, there are frontal and occipital headache, tremors of the head, vascular disturbances, photophobia, vertigo and, as a rule, a definite history of the onset of the visual disturbances. The condition suggests a wide involvement of nerve-centres more or less co-ordinated to each other.

One sign, so far as my own experience goes, seems to be all but constantly present and that is great impairment of visual acuteness, as tested by the types of Landolt or of Snellen. Photophobia in ordinary daylight is often present, but is also often absent. So far as I have observed, visual acuteness, in the strict and proper sense of the term, is almost always affected. In the large number of cases which I have seen and certified as being cases of miners' nystagmus within the meaning of the Workmen's Compensation Act, I have only had one in which the vision amounted to as much as six-ninths of Snellen's scale. Whether ultimately the diagnosis of this case as one of miners' nystagmus will stand good remains to be seen; time alone will show. A case which I shall presently record tends to throw some doubt on the matter.

The constancy of the defective visual acuteness may be of considerable service in certain cases in which the diagnosis is doubtful. Recently two cases have come under my notice which appear of importance from this point of view. The first was that of a miner, working in a district which is tolerably free from nystagmus. Some years ago this man was certified as having miners' nystagmus both by the certifying surgeon and by the referee who examined him on appeal. Quite recently the diagnosis was called in question, with the result that the matter was re-investigated. It was then found that whatever had been the matter at the previous examination the condition had become much altered. The patient walked with great difficulty, availing himself of the help of a stick: he evidently suffered from disturbance of the innervation of some of the muscles of the legs. There were no head tremors, there was no photophobia, the patellar tendon reflexes were present and distinctly exaggerated, but ankle-clonus was absent. There did not appear to be any vascular disturbances.

On examining the eyes for distant fixation and for near convergence, no movements of an oscillatory character could be detected and the positive range of convergence for the median position was excellent. On extreme upward fixation, however, the right eye diverged very distinctly outwards and there was well marked diplopia. The left eye seemed to follow the finger perfectly, but the right diverged with slight jerking movements, precisely like those of what I call *pseudo-nystagmus*. Altogether the diagnosis of impaired action of the right superior rectus was tolerably clear, for the horizontal component was heteronymous. The pupils reacted regularly, although somewhat sluggishly, to light, and there was no ptosis.

By *pseudo-nystagmus* I mean the jerking movements with which every ophthalmic surgeon is acquainted. They are familiar to us all when a test object is held rather nearer than the near point of binocular fixation, and, indeed, whenever an act of vision demands an effort just beyond the strength of a particular muscle. They are entirely devoid of definite periodicity and of rhythm.

On examining with the ophthalmoscope and ophthalmometer, I satisfied myself that there was no serious error of refraction; nor did I find, while making these examinations, any trace of tremulous movements in either eye. The fundus on each side was perfectly healthy.

On testing with Snellen's types it was found that the visual acuteness of each eye was practically six-sixths. That fact alone would seem to put this case at the time of the second examination altogether outside of the category of miners' nystagmus. As already stated, I have till now only once seen a case, and that perhaps a doubtful one, of miners' nystagmus in which the visual acuteness was not very seriously impaired by the disease. That is my own experience; but one of my objects in making this communication is to ascertain how far colleagues who have a considerable clinical experience of the malady agree with me as regards loss of visual acuteness.

It scarcely needs to be mentioned that there may be nystagmus with diminished visual acuteness which is not miners' nystagmus. Given, however, a mine worker with nystagmus and diminished vision, then the evidence of the presence of other causes of the nystagmus must be convincingly satisfactory before the conclusion is formed that the patient is not suffering from miners' nystagmus.

I only remember having seen one such case. The patient undoubtedly had nystagmus and, in addition, he had a high degree of myopia with choroidal atrophic patches. This latter factor in his case had been present for many years, but close inquiry seemed to show that the nystagmus was a recent development. I found, however, that he had ankle-clonus and his speech was distinctly impaired. I formed the opinion that the case was probably one of sclerosis and I certified accordingly. Not many months afterwards the patient died but I was unable to ascertain the cause.

At this stage I should like to point out that the diminution of visual acuteness in miners' nystagmus may be due to one of two causes or to a combination of the two. In the first place, it may be due to the fact that the rapid movements of the eyes prevents the proper stimulation of the macula; that is one way of explaining it. But it must be remembered (*a*) that in cases of nystagmus which are not miners' nystagmus there is often very good visual acuteness, and (*b*) that in cases of miners' nystagmus in which there are no movements, or in which there is cessation of movement, the defective vision is nevertheless present.

In the second place, it is to be remembered that there is in almost all cases considerable pain at the back of the head. May that not point to some lesion at the pole of the occipital cortex involving the centres for visual acuteness? This is possible—even probable. At any rate I make the suggestion for what it may be worth to colleagues who are working on this very interesting and hitherto unexplained disease.

The second case to which I will refer was one in which excellent vision was found; it had been diagnosed by one of the certifying surgeons as being of the nature of miners' nystagmus. I could not agree however with that diagnosis, for the following reasons. In the first place only the left eye was said to be affected and the workman had been off work from November 15, 1923, to March 18, 1924. The disturbance of vision at that time was of the nature of diplopia permanently present, due to an affection of the left eye. The vision of the affected organ was six-sixths of Snellen's scale. The patient had suffered from ethmoidal disease and had been operated upon for that condition. There were neither photophobia nor head tremors present; nor had he at any time, while below ground, seen lights revolving. He stated that at times he suffered from severe pain above the eyes but had never had any pain at the back of the head. In certain parts of the upper half of the field of fixation there were to be seen those jerking movements which, rightly or wrongly, have been called pseudo-nystagmus.

So far as I am able to form an opinion, miners' nystagmus has not yet been satisfactorily explained. It does not seem, as the late Simeon Snell supposed, to be due to posture or to defective illumination. I think all who have read Snell's publications will admit the excellency of his work. Nor does it seem to depend on the depth of the pit: I have seen cases in which the pit was comparatively

shallow. More recently, Dr. Robson, after a painstaking and admirable statistical research, is inclined to attribute it to the soft nature of certain kinds of coal: yet cases do occur even when the coal is hard.

One thing seems to be perfectly clear, namely, that, whatever be the cause, a considerable number of cerebral centres are involved. Thus we have the characteristic oscillatory movements, the photophobia, the head tremors, the acceleration and irregularity of the pulse. All these point to a somewhat widely spread cerebral affection. It may even be, as I have suggested, that the centres for visual acuteness are involved. For the most part the centres or nerve tracts implicated seem to be co-ordinated with each other.

On the other hand, I have never been able to associate deafness or defective speech or paralysis or paresis of any of the muscles of the extremities with miners' nystagmus. Whatever be the causal agent, it does not seem to affect any centres in the medulla oblongata or in the cord; nor does it affect the motor areas or the centres for speech and hearing in the brain or cerebral cortex.

It has sometimes occurred to me that the disease may be due to a definite and specific micro-organism. I am not aware of that possibility ever having attracted the notice of anyone competent to make researches into such a matter. That might explain why the disease is found very abundantly in some districts and not in others. Miners' nystagmus is frequently accompanied by a good deal of constitutional and even of mental disturbance; all seeming to point to a tolerably widely diffused pathological process. At the same time, as already indicated, there are many functions not impaired by its presence.

So far as I am aware, there are no records of post-mortem examinations on those who have had the disease. If such were made, they might throw a great deal of light on the subject, particularly if they were combined with competent bacteriological examinations. In the living patient there is no reason why the blood and the cerebro-spinal fluid should not be examined.

I would suggest that the Minister of Health cause investigations to be made, by the most competent public health authorities and bacteriologists whose services can be secured, into the sanitary conditions of coal-pits in those areas where the disease is prevalent.

Lastly, it is to be remarked that the fact that many functions of the nervous system do not appear to be involved seems against a micro-organic explanation of the disease. Still, definitely selective properties are characteristic of some micro-organisms, as in the case of sympathetic ophthalmitis.

I regard it as urgent that the whole facts of miners' nystagmus be submitted to reliable epidemiologists. The fact that some districts are affected and others are not would suggest quite clearly that the process of coal-mining *per se* is not the determining cause of miners' nystagmus. There are plenty of statistics available and it is to be hoped that the proper authorities will see to it that the matter is put into reliable and authoritative hands so that the question may be fully investigated.

This has become so serious an economic problem in the industry that I should like to see the disease not regarded as an ophthalmic disease at all, but rather as one of great interest to physicians, to bacteriologists, and unquestionably to neurologists. I know Dr. Percival would like psychologists to be included, but with all due deference to so great and learned an authority, I think the psychological aspect of miners' nystagmus has been somewhat exaggerated.

DISCUSSION.

Dr. FREDERICK ROBSON (Penarth) considered that the ætiology must be sought in environment and not in any personal idiosyncrasy of the workers or in lighting. He gave some evidence as to the metamorphosis of coals across the coalfield of South Wales and Monmouthshire topographically, pointing out that the decrease in the incidence of nystagmus

coincided with the decline in the hydrogen content of the coals worked in, also that in geological sequence the seams in individual pits of the same coal gave differences in percentage ratios to the number of men employed. In general the uppermost seam with most "volatile" gave a high incidence of nystagmus, but not necessarily the highest percentage, since the oldest seam at the lowest level in some pits, but not in all, more nearly approaching to anthracitization, also gave a high percentage of the disease; this was probably due to increased temperature and pressure, both of which factors were required to produce cases in harder coal, through the process of oxidation with gaseous defects. In many seams examined, however, the deepest seams had only a small percentage of increase in C/H ratio and not sufficient to counterbalance softer coal at higher levels. This indicated the necessary influence of the temperature and the pressure in the genesis of nystagmus. Some pits, and these deep pits, had no nystagmus cases, and in one group of Scottish pits only *one* seam out of all their seams gave rise to the trouble (analysis of this coal was withheld). In the South Wales coalfield the coals of the 7 class of about 15/17 C/H ratio gave rise to most "volatile" and this class of coal was found in Monmouthshire chiefly, and in this county the highest percentage of nystagmus was found. The 7 constituent of coal connoted the presence of a high percentage of hydrogen. This coal was of the bituminous class. The introduction of the electric hand-lamp in 1914 and its subsequent growth in numbers, had not, as far as figures indicated, reduced the number of cases arising year by year. Much fine coal-dust accumulated at the pit-bottom from coal ascending in the cages and from dust from the screens. Cases of nystagmus were sometimes found at the bottom of the shaft.

Dr. C. F. HARFORD appealed to members of the Section to give earnest consideration to the psychological side of this question. His interest in the subject arose from having to deal, in the Army, with a series of cases of miners' nystagmus, and especially one case whom he had under constant observation for several months. As a result of this he had written a paper, published in the *British Medical Journal* for March 4, 1916, entitled "Visual Neuroses of Miners in their relation to Military Service," but this was before he had applied himself to a definite study of the newer psychology. Mr. Inman had written a detailed criticism of the first report of the Nystagmus Committee published in the *British Journal of Medical Psychology* for January, 1923, and in the following number of the same journal an important communication appeared from Dr. Eddison as a member of the Committee. He hoped that Members would study these papers, which dealt with the psychological aspects of the case.

Mr. G. H. POOLEY (Sheffield) said it was necessary to look carefully at every feature, and regard nothing as proven. Discussions on this subject dated back a long time. Romée, in Belgium, examined all the men working in certain pits in that country in 1878, and again in 1892; and in 1908 he examined again all the men he had examined in 1892. He found that with improved lights there was a diminution in the amount of miners' nystagmus. That showed that statistics giving the number of men who claimed compensation for miners' nystagmus constituted no evidence of the rate of incidence of the disease among the workers in the pits. It was necessary to examine all the men, and ascertain how much nystagmus there was among large numbers of men. In this country no wholesale statistics had been collected. Theories which were not substantiated by accurate observations on large numbers were not worthy of notice. Position at work, defective light, gas poisoning, and now bacterial infection, had been put forward as causes. The evidence given before the Committees of 1907 and 1913 was very interesting, and made an interesting comparison possible between the state of affairs in days gone by and those prevailing now. Most work had been done on the theory of defective light. The theory of gas-poisoning had the drawback that carbon monoxide poisoning was far commoner in men who attended to gas-engines, &c., than in miners, and the nystagmus from which they suffered was very different from that of miners. In the latter, the movements of the eyeball were definitely undulatory; there were not the slow movements in one direction and then a quick return, as in labyrinthine disease and some forms of congenital nystagmus.

With regard to the bacterial idea of causation, if the infective theory was correct it was difficult to understand how all the thousands of cases of miners' nystagmus recovered completely without leaving any sequelæ if the brain were the seat of bacterial trouble.

As to the psychology of the condition, he had talked about it with the late Dr. Rivers, a psychologist and fellow member of the 1913 Committee, and since then he had

watched these cases. He believed he could divide the neuroses associated with miners' nystagmus into two definite groups. One was that set out by older writers, before compensation for the condition was instituted. The neurosis was most intense before the miner stopped work, improving fairly rapidly within about three months of his leaving his occupation. Symptoms were fairly constant, and photophobia and nystagmus were present all the time. When men had been away from work six months or more, they began to develop a neurosis of a different nature. Physical signs were all intensified when the man knew he was under observation and was being personally examined, and the longer he was away from work, the worse his condition became. He had seen cases lasting ten years in which the patients were miserable wrecks. That, he considered, was attributable to the payment of monetary compensation for the condition. Cases of that severity and duration did not occur before the compensation era. The two conditions must be differentiated.

With regard to the question of defective vision in these cases, he knew of a man with continuous eye oscillations who had no defect of vision, and played in a very good cricket team. He came for reading-glasses, and he was still working in the pit, in spite of the constant oscillation. He, the speaker, did not know of any degree of miners' nystagmus which would necessarily prevent a man going on with his work, though he did not mean by that that there was no incapacity from it. Recently he saw a man who all his life had worked at the pit-bottom, pushing the empty tubs on to the stage for lifting; he worked in good permanent electric light, he breathed the pure downcast air, and yet he had marked nystagmus. His case did not accord with any known theories. Any information which would enable the subject to be better understood would be very welcome.

Mr. T. HARRISON BUTLER (Birmingham) said he had seen several well-marked cases of miners' nystagmus with perfectly good vision, and if ophthalmic surgeons would take the trouble to correct errors of refraction—which were frequent in these cases—and put the test-types much lower than the height of the eyes, a large number would be found to see well. It was true that in many of the cases there was complaint of twilight blindness, and it was a moot point whether that was due to the oscillations, or to a definite amblyopia. He dissented from the view that lack of visual acuity could be regarded as a diagnostic sign of the disease.

Mr. R. J. COULTER (Newport, Mon.) corroborated Mr. Harrison Butler's remarks, and said that if Mr. Pooley differentiated between two kinds of neurosis, it was a pity he did not go further and differentiate between the nystagmus which did not incapacitate and that which did.

Mr. BERNARD CRIDLAND (Wolverhampton) said he agreed with both Mr. Coulter and Mr. Harrison Butler, in that he had observed a number of cases of coal-miners' nystagmus in which the visual acuity was unimpaired. Where visual acuity was affected he believed that the impairment was frequently due to spasm of accommodation. Whether nystagmus was to be considered as a general nervous disease or one peculiarly in the province of the ophthalmic surgeon had yet to be decided, but he would point out that every sign or symptom of the trouble could be referred to a disturbance of the higher and lower visual centres, or other centres directly connected with these, as, for example, the memory-centre which had a direct anatomical connexion with the occipital visual centres. He felt that Dr. Robson's observations and conclusions merited the greatest consideration, notwithstanding Dr. Haldane's views as to the effect of mine gases. It must be recognized that whereas miners' nystagmus was so infrequent in candle mines as to be non-existent for all practical purposes, it existed in lamp mines, and that between the different conditions pertaining in these two types of mine lay the clue to the ætiology of the disease. If it was not safe to use an open light in a mine it could not be considered an ideal place for a man to work in.

Mr. J. W. TUDOR THOMAS (Cardiff) said that the visual acuity was not usually materially diminished in cases of miners' nystagmus unless there was present either oscillation of the eyes, or photophobia and blepharospasm, or spasm of accommodation, which was sometimes accompanied by spasmodic internal squint. Dr. Robson had considered the relative incidence of nystagmus in bituminous coal-mines and anthracite-mines in relation to the chemical composition of the coal, but there was also a difference in the physical properties of the coal. Soft coal presented a more uniform dull black surface than anthracite, and would in consequence be likely to absorb more light and reflect less. In the American mines where electric cap lamps were used, illumination at the coal face was very much better than it was when ordinary miners' lamps were used.

Dr. FREELAND FERGUS (in reply) said that practically every patient who came under his auspices as referee had his refraction carefully examined, by the ophthalmoscope, by testing with glasses, and by the ophthalmometer. He did not think defective refraction had anything to do with the production of miners' nystagmus. But in all the cases he examined of miners' nystagmus there was a considerable diminution of visual acuity. He was glad to hear that such patients recovered; he had not been sure they did; they came back with complaints of frontal and occipital headache years afterwards. Part of that might be due to the psychological condition which had been spoken of. As medical referee, he would be sorry to return a man to his work if, every now and again, he had violent fits of headache and was unable to get along the street alone; he would not send such a man to work even on the surface of the pit. If it could be managed, he would like such a man, when he felt fit, to go in for light agricultural work, or something similar, and thus gradually get back to his normal condition. It was pleasing to hear there was no nystagmus in America and South Africa, but he was not satisfied with the explanation, because at home there was also nystagmus among workers in shallow pits. It was, however, more common in deep pits. He did not admit this was purely a disease of the eye, therefore he would be willing to see it pass into the hands of pathologists and physicians and neurologists. It was not due to defective light, but it might be due to gas-poisoning, though there were considerable difficulties in the way of accepting that explanation. Dr. Robson's statistics were very valuable, and various specialists ought to combine to make a thorough investigation of the cause.

Mr. BASIL LANG, F.R.C.S., showed cases of:—

- (1) Implantation Cyst after an ordinary advancement.
- (2) Congenital Pigmentation of Retina.
- (3) Monocular Syphilitic Choroido-retinitis.

Mr. A. H. LEVY, F.R.C.S., showed two cases of Acute Macular Disease.

Section of Ophthalmology.

President—Sir ARNOLD LAWSON, K.B.E., F.R.C.S.

Cyst of Optic Sheath or Hole in Optic Disc (?).

By RAYNER BATTEN, M.D.

PATIENT, a male, aged 52, by occupation a driver. Vision : right $\frac{6}{6}$, left $\frac{6}{1\frac{1}{2}}$. Pupils = active, Tn.

The question is whether this is a knob or a hole. It is behind the scleral ring, and I think it is in front of the disc. It is a rounded swelling, with a vessel passing over it, and after disappearing from view it appears again. On the other hand, some Members believe it is a hole, with a vessel at the bottom. Light is reflected from the top of it. The only other suggestion I have to make is that it may be a clear-walled cyst, so that vessels can be focused at the front and the back of it.

Discussion.—Sir ARNOLD LAWSON (President) said he thought it was a hole; there was a very strong parallax, and a vessel seemed to run across the bottom. He had never seen a similar condition before, and he did not know how it was produced.



Mr. E. TREACHER COLLINS said the appearance reminded him of the crater-like holes in the disc which were described by the late Mr. George Coats; these were mainly filled with neuroglial tissue, which was transparent. If this were so, the vessel might appear to bridge across the gap in the way depicted.

Mr. JULER agreed that this was a hole in the disc. In a paper he published in collaboration with Miss Mann two years ago, he had collected the literature about these cases (*Trans. Ophth. Soc.*, 1922, vol. xlii, p. 96). The hole was almost always near the temporal edge of the disc, and sometimes appeared to be pigmented. Coats had examined microscopically a case.

which he thought was one of *crater-like* hole, but which had not been seen with the ophthalmoscope. He found a defect in the lamina cribrosa, with a pocket in the nerve partly lined with pigment epithelium and filled with loose tissue, suggesting retinal elements and neuroglia. He was of opinion that the pigment and retinal cells had developed from the optic stalk in place of neuroglia.

Tints and their Value.

PRESIDENT'S ADDRESS

By Sir ARNOLD LAWSON, K.B.E., F.R.C.S.

I HAVE had considerable difficulty in choosing a suitable subject for my address this evening, and when mentioning this to Sir Anderson Critchett some time ago, he reminded me that I was somewhat in the position of the boy who had a plate of strawberries given him, and having eaten them all and wanting more, was reduced to starting business on the stalks. I could find no fresh strawberries for you to-night, so I am dishing up some stalks, in the hope that, though only stalks, they may still retain some of the savour of fresh fruit.

The subject of tints has become rather a prominent one of recent days. A great deal has been written—some reasonably, some unreasonably—about ultra-violet rays and their destructive effects on human eyes. Mainly, I think, owing to the energy with which the optical trade has run excursions and alarms, the general public has become to a large extent imbued with a haunting fear of daylight, and the wearing of lenses which eliminate the ultra-violet rays has become the fashion. Like all fashions, the reasonableness has been largely lost in unreasonableness; the desirability of protection for some has become merged in a totally undesirable wish for protection by a large body of the healthy public, a desire fostered by ignorance, and maintained, to a large extent, by advertisements calculated to bring monetary profit to those who advertise. Further, a great deal of confusion seems to have arisen with regard to tints themselves. They seem to me, in a very large number of cases, to be prescribed in a kind of indiscriminate and indefinite way, without any special reference to anything in particular. Especially, I suppose, is it due to the bugbear of ultra-violet rays, which seem to have induced, in some minds, an uncontrollable desire to order Crookes' glass for all sorts and varieties of patients, whether healthy or unhealthy, and no matter what the condition of their eyes may be, simply because the tint is said to produce a feeling of comfort and refreshment.

For these reasons I think this is a good opportunity to make an attempt to clear up the matter of the value attaching to the wearing of tinted glasses.

I fear that the subject, as I shall present it, may give rise to a certain amount of controversy, but at any rate I am, like you, a seeker after truth, and if my version of this estimable virtue as regards the use of tinted glasses leads to a clearer outlook, that is as much as I can or do expect.

What is the real truth about the human eye and its susceptibility to ultra-violet rays? To examine this point, I must traverse, briefly, some well-known ground. Assuming the acceptance of the wave theory of light, it is known that the visible spectrum extends, roughly, from waves of light of 700 $\mu\mu$ wave-length at the red end, to 400 $\mu\mu$ wave-length at the violet end, or, in some people, to 380 $\mu\mu$. Beyond the visible spectrum are the infra-red (or heat) rays, which produce a rise in temperature: whilst beyond the violet end extend the ultra-violet rays, which cause harmful chemical action. Now, whilst there is no direct protection from the luminous and infra-red rays, which pass almost unimpeded to the retina, there is, on the other hand, a great deal of direct protection given by Nature against the ultra-violet rays. It has been found by experiment that this protection is furnished by the cornea and by the crystalline lens, and especially by the latter. The cornea absorbs all waves of shorter wave-length than about 290 $\mu\mu$, whilst the lens is impervious to

ultra-violet rays of a shorter wave-length than $350\ \mu\mu$. Thus, only ultra-violet rays of wave-lengths between $400\ \mu\mu$ and $350\ \mu\mu$ normally reach the retina. Now in various ways the ultra-violet radiation has been traced, in the laboratory, to waves of about $20\ \mu\mu$ wave-length, so it is obvious that only a very small proportion of possible ultra-violet radiation can reach the retina under normal conditions. Ordinary sunlight—that is, the sunlight of the lower altitudes—is, as a matter of fact, rather poor in ultra-violet rays, and contains few of shorter wave-length than $350\ \mu\mu$. All the ultra-violet waves of shorter wave-length than these are stopped by vapour and the density of the atmosphere. Thus, the eye is constructed so as to allow the ultra-violet rays of ordinary sunlight to pass unimpeded to the retina, whereas all excess of ultra-violet radiation beyond what is present in ordinary sunlight is excluded by the natural protective qualities of the eye. So again, protection from the heat radiation proceeding from the red end of the spectrum is to some extent afforded by the absorptive properties of the pigment lining the back of the iris and choroid, and by the contracting power of the pupil, hence the dark eyes of the Southern and Negroid races, who are subjected to sunlight containing excess of heat radiation.

Now, it must be conceded that in all human races the eye is self-adapted to endure, without harm, the daylight common to the part of the world from which any particular race springs: the point is sufficiently obvious and need not be laboured. And it therefore follows, as a necessary corollary, that ultra-violet radiation of wave-lengths between $400\ \mu\mu$, the end of the visible violet spectrum, and $350\ \mu\mu$, are also perfectly harmless. Consequently again, therefore, there is no need to protect healthy eyes from the ultra-violet radiation of ordinary daylight for which the eye is self-adapted under ordinary circumstances.

The same applies to the luminous rays of the visible spectrum and to the infra-red radiation. The eye is normally adapted to endure them without harm.

Practically, however, there are several possible subsidiary factors, which complicate the situation and make it less straightforward than it appears at first sight. The most important of these factors are: (1) The intensity of the light; (2) the duration of the exposure; (3) the presence of glare; (4) occupation or trade.

(1) *Intensity of light.*—The intensity of the light may react on the eye in one or both of two ways. Intense degrees of sunlight imply an atmosphere abnormally charged with ultra-violet and infra-red radiations, and this is well illustrated in the sunlight of mountains such as the Alps, where, on account of the rarefaction of the atmosphere and the comparative absence of vapour, the spectrum is extended at both ends. And again, extreme degrees of sunlight imply an actual increase of dosage, apart from the consideration of the wave-lengths of the penetrating light. If the light in a room is suddenly doubled, the respective wave-lengths of the spectrum are not altered, but only the total amount of light is changed. It is obvious that the effects of increasing the intensity of the light must be cumulative in the eye, but these cumulative results do not seem to react equally in every direction. They are almost certainly much more concerned with the luminous and infra-red portions of the spectrum than with the ultra-violet. As the light is increased, the luminous and heat rays are increased in proportion, and also our consciousness of them, because they all pass through the pupil unimpeded. The ultra-violet radiation must also be increased in quantity, but the eye is not so conscious of it, because of the natural protection afforded by the cornea and lens. Although, by increasing the light, there is a cumulative effect with regard to those ultra-violet rays which can pass to the retina, any actual increase in the ultra-violet radiation is nullified to a very large extent by the construction of the eye. The natural protection from the luminous and infra-red radiations does not appear to be nearly so efficient; so that any danger arising from submitting an eye to intense sunlight would seem to be chiefly with regard to the red end of the spectrum, and not with regard to the violet end.

(2) *The duration of the exposure to high light.* Here again the question of cumulative effect must very much modify our conception of the self-protectiveness of the eye. Under certain conditions of health, prolonged exposure to even moderate degrees of light may be harmful, because the natural resistance may be lowered. We see, of course, the same thing throughout the whole range of disease in the sense that a general lowering in the power of function is a natural sequence when the nutrition is lowered, from whatever cause. Consequently when we begin to talk about what, if any, protection from light is needed in any particular instance, the question has not only to be considered from the narrow point of view of the intensity of the light, but also from the broader standpoint of the length of time the eyes are to be exposed, and the individual's own capacity to endure the light without harm.

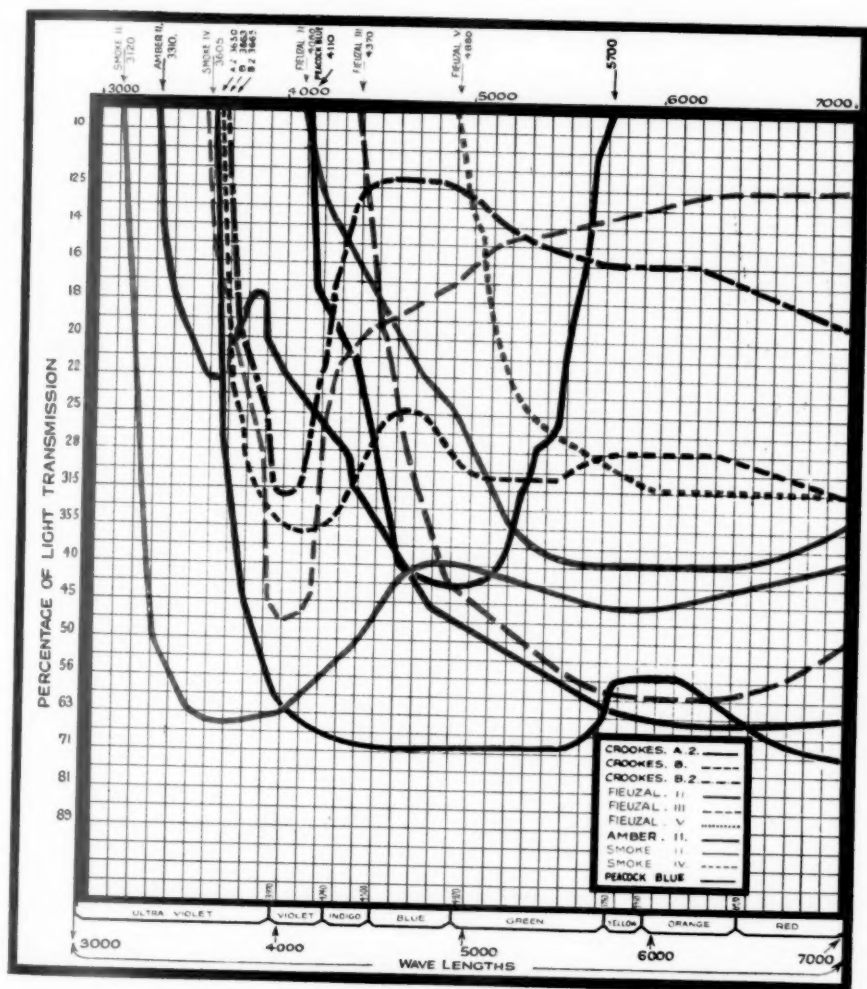
(3) *Glare.*—The presence of glare may complicate the matter very considerably. Glare is an expression which denotes the unpleasant effect produced on the eyes by an excessive amount of reflection from below. It generally proceeds from snow, desert sand, rock, water or chalk; and it owes its disagreeable effects to two causes. First, and chiefly, because we are accustomed to proceed with our eyes directed straight before us or slightly lowered, and the reflected light or glare strikes the eye in an unusual and unaccustomed manner, which distresses us on that account: and, secondly, because glare is something over and beyond what we normally experience, and which must be added, as it were, to the effects of sunlight. Thus, it is estimated that snow reflects about 70 per cent. of the existing sunlight, and it is possible, therefore, in situations where there is much glare, to experience the effects of intense light even though the direct sunlight may be of quite a moderate intensity. For the reasons just stated, the effects of glare would seem to be exerted rather in the luminous and infra-red directions than the ultra-violet, and on this account one is led to the conclusion that snow-blindness is not nearly so much due to excess of the ultra-violet radiation as to excess of exposure to the luminous and infra-red radiations. The distressing conjunctivitis that accompanies snow-blindness is, according to this point of view, due to direct scorching of the conjunctiva similar to that which occurs in the retina in cases of eclipse blindness.

(4) *Occupation.*—Some trades involve long and dangerous exposure to particularized forms of light. Thus, on the one hand, acetylene welding exposes the worker to an intense ultra-violet radiation, whilst glass-blowers and workers in foundries are subjected to much heat radiation. Trades of these kinds are abnormal conditions of life, which must receive special consideration over and beyond that accorded to those not so occupied.

CONSIDERATIONS AS TO THE RELATIVE VALUE OF VARIOUS TINTS.

In order to present the matter as accurately as possible I asked for help from Messrs. Hamblin, who most kindly took the greatest interest in the matter and arranged for the necessary examinations. To them and to Dr. Judd Lewis, who undertook the experiments, and whose name is a sufficient warranty for the accuracy of the work, I tender my most hearty thanks.

For the purpose in hand I selected London Smoke Nos. II and IV, Amber Nos. I, II, and III, Fieuzal Nos. I, II, III, IV, and V, Crookes' A, A2, B, and B2, and Peacock blue. Dr. Judd Lewis was asked to produce spectrum photographs of each of these tints in two grades of thickness, one of 0.5 mm. and the others of 2 mm. thickness. These photographs are incorporated in figs. 1 and 2. Comparison between these two series of photographs will at once emphasize the enormous difference which the thickness of the glass makes to the spectroscopic value. In order to illustrate the matter more completely Dr. Judd Lewis undertook to demonstrate the spectrum photographs of the 2 mm. tints in a series of absorption curves. The majority of these curves have been incorporated by Messrs. Hamblin in a chart (*see plate*) in which each tint is represented by a separate colour. For the sake of clearness some of the shades have been omitted, those



THE ABOVE ABSORPTION CURVES SUPPLIED BY
THEODORE HAMBLIN LTD.
 DETERMINED SPECTROPHOTOMETRICALLY BY MEANS OF A JUDG LEWIS SECTOR PHOTOMETER AND A QUARTZ SPECTROGRAPH



included being sufficient to emphasize certain points which I wish to demonstrate to you this evening. I did not think it was worth while to examine the spectrum photographs of the 0.5 mm. tints because the photographs were sufficient to bring out the point on which I desire to lay stress, that surgeons should realize how much the value of any prescribed tint is affected by the thickness of the glass supplied by the optician. To this point I shall refer yet once again a little later on.

If you look at the chart of absorption curves you will note that each tint starts steeply at zero, at the violet end of the spectrum, where visible light ceases, and then trends away in various degrees of curvature to the red end where it passes away into the little known infra-red portion. At the extreme limit of the red end all the

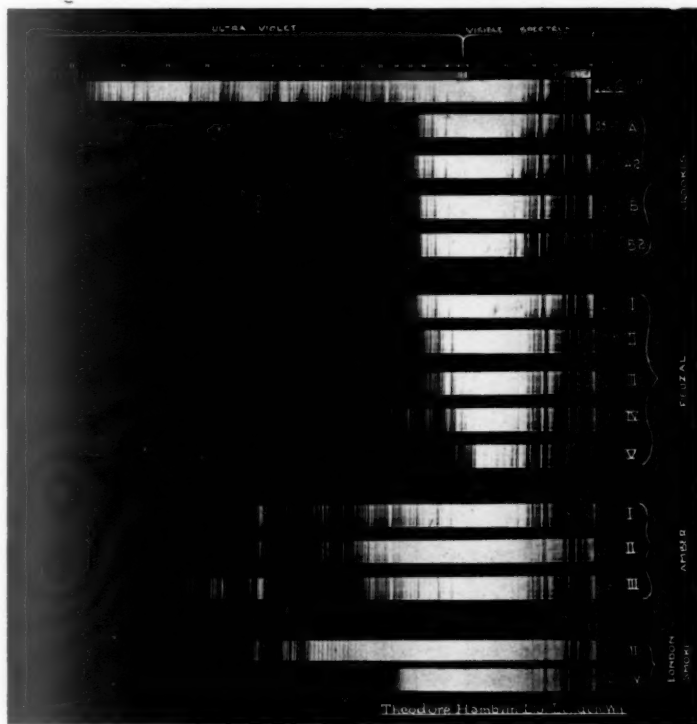


FIG. 1.

tints are still allowing a certain amount of light transmission with the exception of the peacock blue which absorbs all light of wave-length longer than $570 \mu\mu$.

Looking at the violet end of the spectrum the deeper Fieuzal tints have the greatest absorption value, being slightly more powerful than peacock blue and considerably more so than Crookes' glass.

One is further struck by the fact that only in the case of Crookes' glass is there any sort of uniformity in the position of the zero point at the violet end. All the various shades of Crookes' glass have practically the same protective power with regard to the ultra-violet radiation, whereas all the other kinds of glass submitted for

examination show an extraordinary variation in this respect, the ultra-violet protection seeming to depend very largely on the depth of the tint, being less marked in the lighter shade and becoming progressively deeper as the shade is deepened. This can, I think, be only explained by the composition of the glass employed. In Crookes' tints the glass is absolutely standardized and the tints, as it were, superimposed, whereas in all the other tints the various shades are the main factor, and their effect is influenced by being incorporated in a glass which is not standardized. In other words the ultra-violet protective value of Crookes' glass is not much affected by the density of the tint, whereas the value of all the other tints is very much so influenced.

It would seem, therefore, that although Crookes' glass is not the most powerful

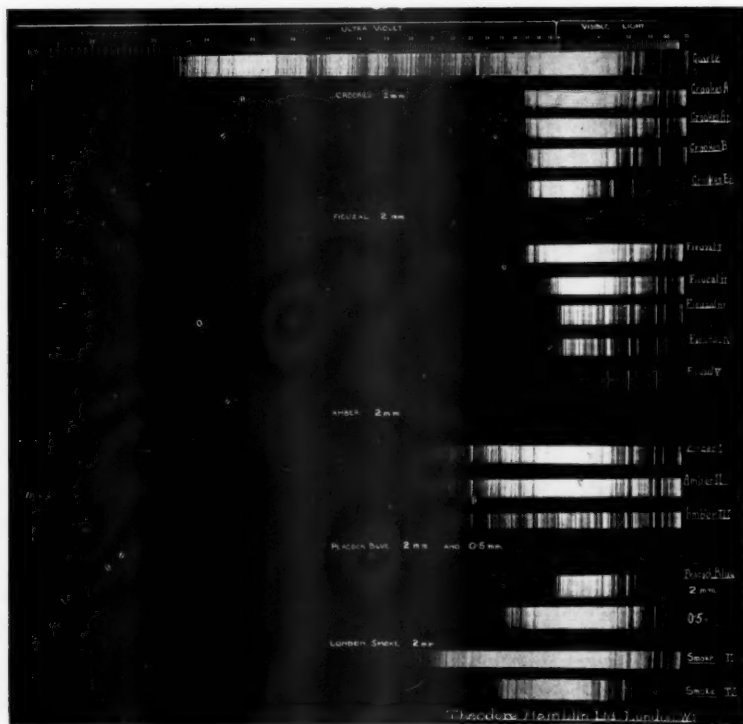


FIG. 2.

agent we possess for shutting off the blue end of the spectrum as well as the ultra-violet rays, it nevertheless has this most valuable property, viz., that we know exactly what we are getting when we prescribe it, and if protection from ultra-violet radiation was the only point to be considered, Crookes' A would be just as valuable as Crookes' B2.

However, as I have already attempted to point out, protection from excess of ultra-violet radiation is only one factor in the situation. In the majority of cases when a tint is ordered it is chiefly from the point of view of general shade effect, whilst in others protection from red and infra-red radiation is at least as important

as protection from ultra-violet, and sometimes more so. If we look upon the tint simply as a means of obtaining shade, all the various types of glass in common use are approximately of the same value, the main factor to be borne in mind being the amount of shade desired which is regulated by the density of the tint and by the thickness of the glass.

There is, moreover, a second, subsidiary factor to be remembered, which is the effect of the tint on the patient's comfort. Some tints exercise a more depressing effect than others and some interfere more than others with the general colour scheme. Thus the shades of London smoke and Crookes' B and B2 are less pleasant in these respects than amber or Fieuzal tints; but if you refer to the absorption curves reproduced on the chart, you will see that to produce a given shade effect a somewhat deeper shade of Fieuzal and of amber is needed than if London smoke or Crookes' glass is employed.

With regard to the particular protection from the red and infra-red radiation, peacock blue is obviously far and away the best tint to employ. However, it cuts out such a tremendous lot of colour that the effect on the patient is extremely unpleasant and depressing and its uses are, therefore, very much restricted. There is not a great deal to choose between the other tints. The deeper shades of Crookes' and London smoke produce curves which are very similar and exclude rather more than the corresponding shades of Fieuzal. All the amber spectrum photographs were so inefficient as regards all the points just considered, when compared with the other tints, that I have only reproduced one amber curve on the chart. The example shown of a medium amber shade curve sufficiently proves the inferiority of this tint to all the others in general use.

After these remarks on tints in general I will pass to a short analysis of the absorption curves shown on the chart. To prevent the confusion necessarily arising from a great many intersecting lines only a certain number, which were calculated to bring out the special points just mentioned, have been included.

The most interesting curve of all the tints is the peacock blue, which absolutely excludes all rays shorter than $400\text{ }\mu\mu$ or longer than $570\text{ }\mu\mu$, which means that all the ultra-violet and some of the violet rays are excluded at the one end and all the yellow, orange and red rays at the other. It thus affords an infinitely larger range of protection than any other tint.

All the tints compounded with Crookes' glass shut off the ultra-violet rays of a shorter wave-length than $350\text{ }\mu\mu$, which means that they give good ultra-violet protection, but they are not nearly so efficient with regard to the blue and violet end of the visible spectrum as the corresponding shades of Fieuzal. The charts show quite conclusively the point, which I do not think has been generally noted before, that the deeper shades of Fieuzal Nos. 3, 4, and 5 effect the most powerful protection of all known tints with regard to the violet end of the spectrum, much more so even than peacock blue. It will be seen that Fieuzal 5 shuts off nearly all the blue and every ray of shorter wave-length than $490\text{ }\mu\mu$.

London smoke IV (dark medium), which is of a little lighter hue than Crookes' B2, produces an interesting curve. It affords very nearly as much protection from the ultra-violet radiation as does the Crookes' B2, but it allows a good deal more of violet and blue radiation than does the Crookes' B. On the other hand it shuts off a good deal more of the orange and red radiation than Crookes' B2.

With regard to the protection from red radiation, Crookes' B and B2 and London smoke IV are better than any of the Fieuzal shades, but both are markedly inferior to peacock blue.

As an efficient shade from high lights without general reference to ultra-violet and infra-red radiation, London smoke of medium tint in the form of Crookes' B is very satisfactory. Crookes' B2 and London smoke IV exhibit very similar curves and are both excellent when deep shade is required, Crookes' B2 being preferable

because of its superior protection from ultra-violet radiation. The Fieuzal tints are all rather disappointing in this respect.

Crookes' A2 is a remarkable glass in that it affords substantial protection from ultra-violet radiation without seriously impeding the luminous rays. It is in this respect infinitely superior to a light shade of London smoke and much better than amber. In this way is explained the refreshing qualities in conditions of mild glare such as prevail in theatres and cinemas.

The most uniform curve is obtained from Crookes' B. This glass gives good protection throughout the whole length of the spectrum, transmitting at one point no more than about 35 per cent. of the radiation. It is on these grounds the most satisfactory of all the tints examined and seems to be the best for general use.

Finally, two more points arise from the consideration of these tints. Both have been previously mentioned; but I should like to draw a little more detailed attention to the desirability of obtaining a greater uniformity of shade and spectroscopic effect than that which obtains at the present time. It is quite obvious, I think, that nobody does know or can know what is really provided when a tint is ordered, unless Crookes' glass is selected.

Take, for instance, the three shades of Fieuzal given. The three pieces of glasses from which the spectrum photographs and the illustrating curves were made were precisely similar in thickness and only apparently differed in shade. Yet you at once see that the composition of the glass must be very different because they give such extremely varying effects spectroscopically, and, most curious point of all, Fieuzal 3, which is a darker shade than Fieuzal 2, allows more transmission of the red, orange and yellow rays than does Fieuzal 2. This question of standardizing the glass is the first point, the second being the thickness of the glass employed, to which I have already alluded. I do not suppose that any ophthalmic surgeon has ever interfered with what may be regarded as purely opticians' work, to the extent of suggesting the thickness of the glass to be used when ordering a tint; but yet it is often a matter of the greatest importance, and especially so, perhaps, when prescribing a tint to be overlaid on a minus lens.

As a result of this lack of standardization in the quality and thickness of the glass employed, the whole supply of tinted glass, with the exception of Crookes' glass, is in a most unsatisfactory state. Nobody can write a prescription for the supply of a tint, and have the slightest idea of the true value of what will be supplied to his patient, unless he has some private arrangement with a firm of opticians, and has a special box of tints of uniform thickness which correspond exactly with what the optician supplies. Even then, neither he nor the optician has any control over the manufacture of the glass, and, as I have pointed out, both may be utterly deceived in consequence.

Perhaps these considerations are of the greatest importance when it is a question of supplying special protection in dangerous trades, or for those undertaking journeys or exploration in countries where they will be exposed for long, continuous periods to great glare, as for example in the recent expeditions to explore Mount Everest. I hope that this question of standardizing different tints will now receive the consideration which is its due, and I think it is a question which might with great advantage be brought before the Council of British Ophthalmologists.

My personal belief is that if Crookes' glass were combined with Fieuzal shades as well as with London smoke, we should then possess with peacock blue a series of highly standardized tints which would meet all and every requirement. It should further be made clear that the glass, unless there is a special indication to the contrary, is always provided of a certain standard thickness, and that variations from this standard must be compensated for by ordering a lighter or darker shade, as the case may be.

THE WEARING OF TINTS IN HEALTH AND DISEASE.

(1) *The Wearing of Tints for Healthy Eyes.*—As harmful ultra-violet radiation does not exist under ordinary circumstances of daylight, protection by special ultra-violet glasses is quite unnecessary. When extraordinary circumstances are apt to be encountered, the main factors to be considered are the luminous and the heat radiations, rather than the ultra-violet. As already explained, these circumstances are present when the eyes are exposed for prolonged periods to the effect of high sunlight and glare, especially when these factors are present in rarefied atmospheres of the higher altitudes such as the Alps on the one hand, or when the eyes are exposed to the sun of the plains in the Tropics on the other hand. Although the effect of the Fieuzal tints seems rather to diminish towards the red end of the spectrum, I believe that these would, if the glass were properly standardized, prove to be, on the whole, much the best shades to employ in high glares, with the exception of peacock blue, which is the most protective of all.

One often hears of Crookes' glass A or A2 being ordered for reading and work. Very often, I think, it is because the prescriber has the idea that artificial light is stronger in ultra-violet radiation than is sunlight, which is an entirely erroneous view. Dr. Leonard Hill, in his recent work on sunlight and fresh air, states that in the case of an oil lamp 97 per cent. of the energy is in the infra-red region, and of electrical incandescent lamps, 95 per cent.; so that if it is considered desirable to protect the eyes by some tint in artificial light, Crookes' glass possesses no special advantage from a medical point of view. Crookes' A or A2 glass has, however, this superiority, namely, that it affords a general shade without materially disfiguring the appearance or altering colour values, and, as a matter of practice, it is, on these accounts, very useful and often very much appreciated by sensitive patients.

Then we must consider a large class of cases consisting of patients who have perfectly healthy eyes but who are physically or mentally abnormal. In physical disease and in neurasthenia the general function of the eyes is, very commonly, impaired. Thus we note that the physical invalid prefers, as a rule, that the blinds should be, to some extent, drawn; and, similarly, mental invalids suffer to some extent from photophobia. Whilst, therefore, undue protection is to be deprecated in health, we are bound to give help in cases of lowered health, from whatever cause, and whether for the purpose we prescribe yellow, smoked, or Crookes' glass, is immaterial. The choice of tint rather depends upon the effect that the tint has on the individual. Smoked glass is to-day as valuable as it ever was, but it has the disadvantage of being somewhat depressing in its general effect, and Fieuzal, which is as effective without interfering so much with the colour scheme, is often preferred. Crookes' glass is excellent, too, especially that variety which combines a smoke tint, and is known as Crookes' B, or Crookes' B2. In any case, however, we should disapprove of too much protection in this way, and should not only advise the gradual discontinuance of the tint as the general health improves but should, in nearly all cases, strongly discountenance what has become a very common and pernicious habit, viz., that of wearing some sort of tint continuously, whatever the light and whatever the occupation.

We should always remember that the habitual wearing of shades is apt, like all other habits, to become fixed, and to produce not only a dislike of conditions of light which are perfectly harmless, but a positive inability to endure them unaided. When such a condition is reached the person suffers a great deal of inconvenience, and, of course, it detracts very much from her or his personal appearance.

(2) *The Wearing of Tints for Diseased Eyes, or as a Prophylactic Measure against Disease.*—It has been shown quite definitely, I think, that cataract arising in glass-blowing and allied trades is due more to harmful infra-red radiation than to ultra-violet rays. Tints such as Fieuzal 4 or Fieuzal 5, or peacock blue, afford the best prophylactic. In all cases of cataract, from whatever cause, tinted glasses to wear

in bright sunlight or in glare should be ordered. I have no doubt in my own mind that there is nothing more harmful in the early stages of cataract than exposure to bright lights of any kind, and I am most careful to order efficient protection to all my patients with that condition. Glare by artificial light should be shunned as much as glare in daylight. I think, in this variety of cases, where a habitual tint is indicated, there is nothing more pleasant or comforting to the patient than Crookes' glass, though other tints are just as effective. I often make a point of ordering two degrees of shade, one for moderate and one for extreme degrees of light. Reading glasses of Crookes' A are quite efficient and have the advantage of not cutting off so much of the luminous rays as to interfere with the patient's comfort. After the extraction of cataract the eye is deprived of its most powerful protector against undue ultra-violet radiation. Further, the high plus lens, usually necessary to correct the refraction, acts to some extent as a burning-glass. Consequently aphakic eyes need special protection—a point which, it seems to me, is not insisted upon as much as should be the case. There are practical difficulties in the case of hospital patients, who are usually supplied with large bi-convex lenses, on account of their extreme thickness, and also, to a slight extent, on account of the increased expense involved in making the glasses tinted. In the more modern and expensive "luxe" type of cataract lens the matter is easier. Crookes' A glass or the lightest shade of London smoke or Fieuzal are the most suitable tints to employ, and personally I much prefer the Crookes' A, as being the less disfiguring and the most suitable for the purpose. I feel strongly that all aphakic patients should have some protection outdoors, except on dark and cloudy days, and if the lenses are made out of Crookes' A the same glasses will do for indoors as well as outdoors, without materially interfering with the patient's comfort. Reading lenses may be similarly constructed and should indeed be so made if the patient is wearing lenses made out of Crookes' A glass at all other times.

In active inflammatory disease of the retina and choroid I much prefer, and always order, peacock blue. By it all ultra-violet and infra-red radiations are entirely shut off, and a very large proportion of the luminous rays as well. It has a peculiarly soothing effect on the patient and forms a most valuable kind of treatment. There is, to my mind, no other tint comparable with it.

Discussion.—Mr. HAROLD GRIMSDALE said that there was one point he had intended bringing up as indicating a possible use for tinted glasses under certain circumstances. All had met, from time to time, patients, mostly those past middle life, who found difficulty with their eyes in keeping pace with moving objects; they found, in sport, that the bat no longer met the ball squarely in the middle. In a certain number of them the explanation was to be found in Pulfrich's sign or phenomenon. Two years ago Pulfrich described a series of experiments showing that the time of perception varied inversely with the intensity of the stimulus, and therefore if, by any means, the stimulus reaching one eye was weakened, e.g., by putting, as in his experiments, a tinted glass in front of it, the position of the moving body was falsely projected owing to the fact that the stimulus received by the weaker eye was appreciated later than that received by the normal eye. When an eye was weakened by disease, for example when the light perception was a little reduced owing to the media being turbid, or when after retrobulbar neuritis, although the form perception had been recovered perfectly, the light perception had remained defective, the patient saw the moving object in the wrong position. And he saw Pulfrich's two pointers revolving, instead of keeping in the same plane. In the case of some of these people, if one reduced the luminous perception of the normal eye, they at once saw moving objects normally, and some of them had been able in that way to regain their efficiency at games. He threw that out as a suggestion, to be borne in mind as a possible use for a tinted glass.

Mr. ERNEST CLARKE said that he remembered the early days of electric light when the naked carbon point caused intense glare, and he believed that the fear of glare of light became fairly common then. It was a great contrast to the colza oil lamps with the green shades which our grandparents used. He said that some years ago he invented a new word to express the fear of glare of light, to supplement the word "photophobia." With the approval of Sir

Cooper Perry the word he coined was *photaugiaphobia*. This fear of glare was now becoming less, as designers of lighting systems for buildings sought to do away with the glare by casting the rays of light upwards to a white ceiling, from which the rays were diffused as a soft light. As a rule only patients obviously suffering from glare should be given Crookes' or tinted glasses. He had found the Crookes' A and A2 of enormous benefit to patients with a condition bordering on neurasthenia, namely one of pronounced hypersensitiveness. Even ordinary glass cut off many harmful rays, and sometimes glasses supplied for correction of distance defect served the purpose. The great point about Crookes' glass was that it did not cut off the luminous rays. Crookes' B he had found very efficient for use in Switzerland and the tropics.

Mr. MALCOLM HEPBURN said that he was greatly interested in the question of correcting glasses for snow-blindness. Thirty or forty years ago the blue glass was in general use for this purpose, and from what the President had now said there was no doubt that was scientifically the right glass. Still, the wearing of peacock blue glasses on the mountains was not comfortable; the glasses had to be worn during the whole time, whether one was traversing rock or snow, and the effect of peacock blue was so to dull the illumination that the wearer could not judge distance when stepping from snow to rock or *vice versa*, and this might easily result in a false step which might prove dangerous. During the last few years that he had climbed he had used green glasses, and he found them much more comfortable; the intensity of illumination was not interfered with as was the case with peacock blue, and therefore there was not the same difficulty in correctly judging distances.

Mr. G. H. POOLEY said that for those engaged in association with steel furnaces, where the light was very intense, peacock blue glass seemed to be far more comfortable than any other; it enabled the men to carry on efficiently. The spectrum showed clearly that this glass cut off the ultra-red rays to a considerable extent. He (the speaker) pointed to the fact that the ordinary healthy white man took no harm from the high degree of glare to which he might be exposed in the tropics or on the big lakes. He had long believed that the healthy human eye could adapt itself to the greatest intensity of ordinary light or artificial illumination, and therefore ophthalmologists should set their faces against healthy people having recourse to various tinted glasses. With regard to the damaging effects of very bright lights, some time ago he was appointed Official Referee over a large area in the north of England, in addition to his former area; in this area many people were subjected to the intense glare of molten metal but no case had yet been brought to his notice of injury so caused to the lens. He did not think the idea that infra-red rays caused changes in the lens had been sufficiently substantiated by evidence. Referring to early cataract, he had had a large number of cases under observation for many years, and he was not able to say that any one thing—whether subdued light or chemicals—was responsible for the rapidity with which the crystalline lenses changed.

Mr. BERNARD CRIDLAND (referring to Mr. Ernest Clarke's statement that Crookes' glass was not really a tinted glass) said that he had been informed by the makers that the tint of Crookes' glass had nothing to do with its optical properties, and that the tint was merely put in because the public would not have the glass if it were not tinted; they did not think it worth the extra money. If that was so, was it necessary to regard the deeper tinted form as of any different value by reason of that tint than the paler form?

Mr. ERNEST CLARKE (answering Mr. Cridland) said he wrote to Sir William Crookes before that scientist died, asking him to give some particulars of the "Crookes'" glass. The reply to that was, that the slight purple tint was due to the presence of the red metal, didymium, which could not be separated, commercially, from the cerium salt. "Crookes' A" was No. 187 in his series and the composition was cerium nitrate 17 per cent., fused soda flux 83 per cent. It cut off 27 per cent. of the heat rays and practically all the ultra-violet rays and transmitted 99 per cent. of the light.

Mr. CYRIL WALKER said that many years ago a confrère of his spent a winter in Labrador and experienced much discomfort from snow-blindness. He tried glasses of various tints to lessen this discomfort, but while wearing them he could not shoot. He (the speaker) then ordered him two pairs of smoky amber-tinted glasses, which were found to be a perfect success. He believed that the wearing of these glasses was common among people who shot in snow.

Mr. TREACHER COLLINS said he had personally prescribed amber-tinted glasses rather largely. He was first induced to do so by what the late Dr. Bowles once told him of his experiences as a climber. When, with several others, he was going on a climb among snow

and glaciers, he appeared at breakfast in the morning with his face coloured yellow, and he received a good deal of chaff in consequence. On their return in the evening, the only one of the party whose face was not burning and red was that of Dr. Bowles. Sir Arnold had compared snow-blindness to sunburn; both were due to the effect of ultra-violet rays.

Many years ago he had advised an officer from India to take with him some amber-tinted glasses. He did so. A few years later he came back, and said he had been to Tibet, where many of the men had snow-blindness, but that he had not suffered from it, as he had worn glasses. He (Mr. Collins) was told by a doctor who had been with Shackleton's expedition to the South Pole, that all his party to the South Pole wore amber-tinted glasses as a protection against snow-blindness, but that the ponies got snow-blindness and had to have amber-tinted glasses constructed for them.

It was true that the crystalline lens absorbed and cut off many ultra-violet rays. There was a good deal of evidence that erythropsia occurring in aphakia was due to exposure to ultra-violet rays which gained entrance to the eye. Dr. Rivers had attributed the erythropsia to hyperemia of the retina so produced.

On certain occasions he had himself prescribed peacock blue glasses, and his purpose was then to cut off the heat rays. He found that cooks and others exposed to heat rays obtained great relief from wearing them. What coloured glasses were worn had often been a matter of fashion or vogue, and what the President had just said in his address would do much to place the subject on a scientific basis. He would have been glad if the research had been extended to other forms of glass. In America amethyst glass was a vogue for a while, and in this country at one time pebbles; it would be interesting to have spectrum tests applied to these forms of glass also.

Mr. ARTHUR GRIFFITH suggested that the only value of Crookes' A glass came from the presence of didymium. When Sir William Crookes set out to make a protective glass which should be colourless, he took advantage of the properties of the rare earth metals which gave positive absorption spectra. From a solution of didymium salt, or from a didymium glass, one obtained two absorption bands in the yellow, and two fainter ones in the green. If Crookes' A glass possessed any value, he (the speaker) thought that it was owing to these absorption bands in the yellow, which reduced the luminosity, for yellow was at the centre of the diffraction spectrum.

He (Mr. Griffith) said he had made a number of spectrograph studies at one time, and had found that Crookes' glass did comparatively little in the way of absorbing ultra-violet light, indeed it was in this respect little superior to ordinary crown glass.

Sir ARNOLD LAWSON (President) in answer to Mr. Hepburn, said that peacock blue was not desirable to wear in the ordinary way, because of its unpleasant effects. For snow-fields, Fieuzal 4 and 5 were just as effective. In answer to Mr. Collins, who advocated the use of amber-tinted glasses, he (Sir Arnold) said that, as he had indicated, he had found them generally ineffective and he had demonstrated curves on the diagram in support of that statement.

Cataract Extraction in Egypt.¹

By A. F. MACCALLAN, C.B.E., M.D., F.R.C.S.

(ABSTRACT.)

THE author first described the preparation of the patient preliminary to operation for cataract, and then gave details of the method of operation which he usually prefers to adopt, which however depends on the case; in all cases a capsulotomy is done. By whichever method the capsule is opened a number of cases will require dissection of capsule subsequently, the author preferring to do this as soon after the operation as is safe. He described his method of recording the results of his own operations and the importance of following up the cases after discharge from hospital. A number of unusual conditions met with during operation was then described in illustration of the importance of experience in dealing with emergencies.

The operation of extraction of cataract from eyes which are affected with primary glaucoma was discussed, the prognosis being on the whole satisfactory. Reference was made to a form of cataract found in children, frequently in association with anæmia, described by Waddy (*Bull. Ophth. Soc. Egypt*, 1914).

¹ This paper will be published in full in the *Lancet*.

Section of Ophthalmology.

President—Sir ARNOLD LAWSON, K.B.E., F.R.C.S.

Bitemporal Hemianopia due to Fracture of the Skull.

By EGERTON GRAY, L.R.C.P., M.R.C.S.

PATIENT, a male, aged 44. On September 9, 1911, fell out of a tree on to the road, a distance of about 12 ft., upon head, striking left frontal, causing depressed fracture and injuring nose and upper lip. Unconscious for ten days; during part of that time had severe epistaxis. When conscious he immediately complained of his sight, which had previously been perfect.

He attended Shrewsbury Eye and Ear Hospital under the care of Dr. Russ Wood as an out-patient for over twelve months. Returned to work six months after accident. Had no paralysis of limbs or speech.

Thinks he put on weight for twelve months after accident. Had to sleep in sitting position up to three years ago, now sleeps with head high—two pillows and a bolster. Has had a good deal of severe headache, not now so severe as it was, but sharp at times. No vomiting. Sees double in certain position. Cannot read, for unable to follow successive lines. Is sure he is no worse now than ten years ago. Lost sense of smell same time as sight. Right vision = $\frac{6}{9}$. Left vision = $\frac{6}{12}$. Pupils equal and active, left sluggish. No scotoma for colours. Right disc: pale temporal, blurred nasal. Left distinctly pale with sharp outline. When reading distant type, head turned towards side of eye being tested. Movements of eyes good. When reaching limits of abversion and up-and-down movements, occasionally gets nystagmoid movements of head. Crossed diplopia with right eye. Fields show bitemporal hemianopia and contraction of nasal field to 25° circle or less in left eye and to 40° circle or less in right, with 10 m., white test object. Has always had a very dry skin. Hair does not come out. No loss of sexual function.

Urine: Specific gravity, 1015. Albumin, sugar, *nil*. No polyuria. Sugar tolerance not increased.

Noted at his business for possessing an exceptionally good memory.

Mr. J. HERBERT FISHER remarked that this was a very exceptional kind of case. It was not a pituitary body case, but a traumatic case, with a bone lesion. There had been a depressed fracture which even now could be traced in the left fronto-temporal region. The fracture must have run across the roof of the left orbit and involved the cribriform plate and olfactory lobes, as well as implicating the optic chiasma; and it had thus produced the bi-temporal hemianopia. The sella turcica of this patient was normal in outline, but how the fracture produced damage to the chiasma it was difficult to conceive. We were apt to think of the chiasma as lying in the groove which was on the anterior part of the body of the sphenoid process, but that was not its situation. It was located on the anterior segment of the diaphragma sellae, where it would be less accessible to direct bony laceration than if it lay in the bony groove. There was a little enophthalmos on the left side and also diplopia, presumably attributable to mechanical impediment of movements of the left eyeball.

Mr. RAYNER BATTEN, M.D. showed a Sister and Brother Born Blind, without Obvious Ocular or Central Cause.

Mr. P. G. DOYNE, F.R.C.S., showed a case of Microphthalmia with Retinal Cyst and Pedunculated Growths along the line of the Orbito-nasal Fold.

Inflammatory Pseudo-Tumour of the Orbit.

By F. A. WILLIAMSON-NOBLE, F.R.C.S.

(ABSTRACT.)

INFLAMMATORY Pseudo-tumour is a condition clinically resembling an orbital tumour, but found pathologically to be due to inflammatory changes in the orbit. The condition is somewhat rare, and Benedict and Knight could find only five cases reported since Birch-Hirschfeld's original paper in 1910.

Case I.—Male, aged 25, who attended hospital with marked proptosis of the right eye, and conjunctival injection. Movements of the eye were normal, and rhinological examination was negative. Mercury and iodide in the usual doses had no effect, though administered for over six weeks. An exploratory incision revealed a hard mass within the orbit, which was therefore exenterated. Macroscopically, the orbital contents were seen to consist of a large white mass lying within the cone of the muscles, and on microscopic examination this mass showed the histological characteristics of a gumma. A similar case has been reported by Hine. According to Harrison, a negative Wassermann in a case of this kind is of little value in excluding syphilis.

Case II.—Patient, a middle-aged man, who developed proptosis and limitation of movement in the left eye. The orbit was exenterated and the contents showed infiltration with lymphocytes, the infiltration being much more marked than were any other evidences of inflammation. The condition was diagnosed as a lymphoma, the excessive formation of lymphatic tissue being brought about by a preceding chronic inflammatory change.

Case III.—There was a small mass of young connective tissue formed outside the eye as the result of irritation induced by the presence of cholesterin crystals, these crystals having been produced by previous hæmorrhages.

[February 13, 1925.]

Case of Recovery from Pseudo-tumour of the Orbit.

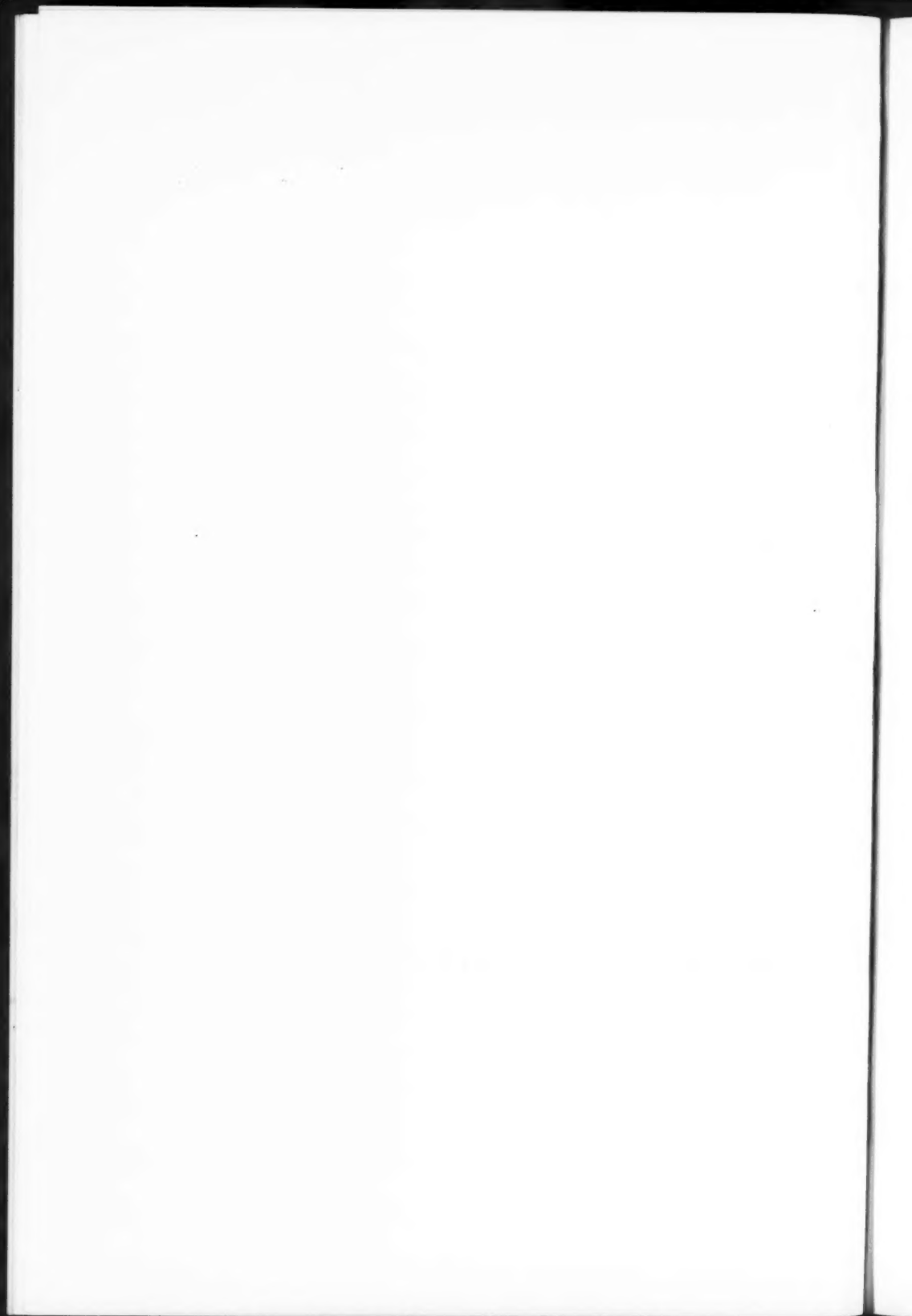
By R. LINDSAY REA, M.D., F.R.C.S.

I FIRST saw this patient last June and I ask you to contrast the condition now with what it was then (see Plate). At our last meeting I mentioned this case saying it was a gummatous infiltration. Examination of the surrounding sinuses by transillumination showed the left antrum to be opaque. The left antrum, the left orbit, ethmoidal region and right orbit were all affected. The disease was blood-borne by means of the anastomoses of the ethmoidal branches of the ophthalmic artery with the alveolar branches of the internal maxillary. Fibrosis had not taken place, and therefore when the patient was given rapidly repeated injections (each fifth day) of novarsenobillon, chiefly 0.45 grm., with mercury inunctions and iodide of potassium, 25 gr., doses thrice daily mostly, within three months the left eye, which had been blind, recovered its sight, and to-day there is $\frac{5}{6}$ vision in each eye. Also each eye is back in its proper position.

This communication will be published in full in the *British Journal of Ophthalmology*.



REA: Case of Recovery from Pseudo-tumour of the Orbit.



Case of Optic Nerve Tumour.

By HUMPHREY NEAME, F.R.C.S.

THE essential features in this case are the following. The patient is a man aged 45. In 1918 his doctor noticed that one eye was more prominent than the other. He was sent to the Prince of Wales Hospital, Tottenham. He states that then the sight of his right eye, the good one, was sufficient to enable him to read the sixth line of letters, and that the sight of the bad eye was such that he could read the third line. Another observer noticed the proptosis. During the six years that have elapsed the proptosis has gradually increased, and it is now 7 mm. Vision in the proptosed eye is only perception of light. There is practically no lateral displacement or rotation of the eye, and while he looks straight in front the left eye shows a pupil displaced upwards about 2 mm. Movements are full in all directions. The left eye is hypermetropic to the extent of 6D., the right eye to 1D. Lenses do not improve the vision of the left eye. On ophthalmoscopic examination the left eye shows a prominence; I think his disc is 4D. more prominent than the surroundings at five disc-diameters away.

With regard to X-ray examination, Mr. Norman Flemming said, eighteen months ago, that X-ray examination and the Wassermann were negative, and it is the same now. Mr. O'Malley made a nasal examination, and that also proved negative. The patient himself told me that in 1918 he underwent eighteen months of treatment with medicine containing potassium, so I presume he had potassium iodide for eighteen months on the assumption that the condition was similar to that in the last patient. The proptosis steadily increased.

The chief difficulty lies in diagnosing the type of optic nerve tumour, whether it is gliomatosis or an endothelioma, or possibly a fibromatous state, which is more rare. His age is in favour of its being endothelioma. In Mr. Hudson's paper in the *Royal London Ophthalmic Hospital Reports, 1912*, 75 per cent. of the gliomatosis cases were said to occur within the first decade of life, and of the endotheliomata 50 per cent. of the cases were over 30 years of age. In gliomatosis visual defect is usually early, and proptosis comes on later than the visual defect. In this man the proptosis is the more marked feature. Vision was found to be defective, but not very seriously so until recently, as far as can be ascertained.

There is generally more limitation of the eyeball movements in endothelioma than in gliomatosis, because the endothelioma is generally more anterior. But in this case the movement of the left eyeball is free; it may be that the endothelioma is further back. Hudson says that pain, which was a symptom in this case, is more common in endothelioma.

Dr. G. W. KENDALL exhibited a case in a male of "Detached Retina with Angioma of the Retina."

The Susceptibility of Nocturnal Animals to Ultra-violet Radiation.

By SIDNEY RUSS, D.Sc., in association with Sir ARNOLD LAWSON, K.B.E., F.R.C.S. (President).

I.—Professor SIDNEY RUSS.

THE observations which your President and I are bringing to your notice this evening arise out of some work of a different character which was done during the war, and to that work I will first of all briefly allude.

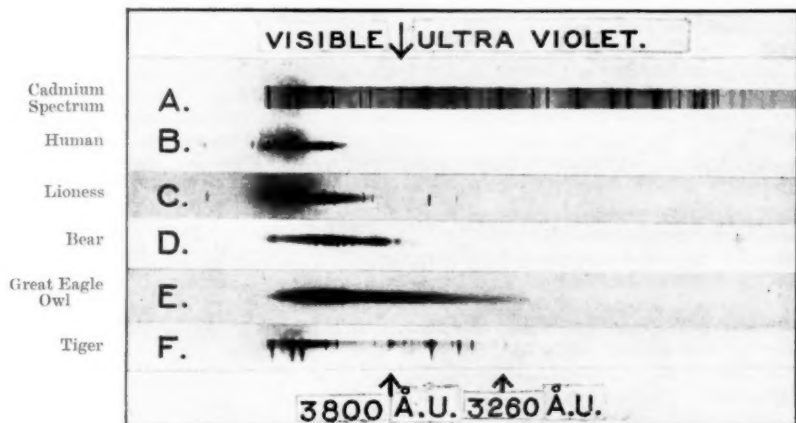
The problem which was put to Dr. Mottram and myself was this: How could

the night vision of naval officers and other ratings be improved? It was of vital importance that their night vision should be as acute as possible. Under ordinary conditions of navigation all the lighting was cut down to a minimum, and that, in itself, aided night vision. But the naval authorities wanted something more—some experiments to ascertain whether artificial assistance could be given to that end. The suggestion was made as to whether, by giving the eye certain fractional doses of light, we might increase the sensitiveness of the eye to very dim lights. I do not want to go over those investigations in detail, but the result of them was this: that if to a comparatively rested eye you gave comparatively small doses of light in the blue and violet region, the eye became somewhat more sensitive, more acute in its perceptions of distant and dimly-lighted objects, when they were lit by light of longer wave-length than that used to sensitize the eye. We arrived, in the course of these experiments, at certain quantitative results, which showed that you could increase the acuity of vision somewhere about 15 per cent. We did not know exactly what reliability to place upon this figure, but it was rather interesting to learn subsequently that other experimenters at different stations had found a similar result and a like figure. The experiment consisted in flooding the eye with very small intensities of blue and violet light and then training the eye on distant objects which were difficult to distinguish. When the first Zeppelin was brought down in this country it was found, I believe, by Dr. Mottram that the gun-sights, scales and compass charts were found to be illuminated with this variety of light.

That experiment led to interest being directed to the question of acuity of vision under ordinary night conditions. I then approached your President, and the question arose as to how transparent the eye is to different regions of the spectrum. He was very ready to help us in the way of providing material. Human eyes are sometimes available for experimental purposes. The experiments which we then continued together were of a direct character and consisted in taking the fresh specimen, cutting a little window at the back of the retina, and cementing a quartz plate on to that, so that, for all practical purposes, we still had the eye, but we had a method of transmitting radiation through it, and subsequently analysing it and seeing to what extent the radiations were absorbed by the cornea, the various humours of the eye, and the lens. I need not go into detail as to how that source of light was arranged; either an arc light of tungsten or a mercury lamp does very well for the purpose, and a collimating lens. The light, before entering the slit of the quartz spectrograph, had to pass through this eye with a quartz window. If you take an ordinary spectrum of these sources of light, you get all the constituents of the light separated on the plate, but, interposing the eye in front of the slit, you get the slit still focussed in such a way that all the transmitted lines are suitably dispersed by the prism in operation.

The first experiment with the human eye showed that the human eye is very absorbent of anything beyond a wave-length of about 3,800 Å.U.; that is a very frequent limit which is set to human vision. The question which arose in my mind was the following: Is acuity of vision to dim lights dependent on the transparency of the eye for radiation beyond such a wave-length, that is to say, of a shorter wave-length than 3,800 Å.U.? If so, it should show itself by means of a study of the same process in nocturnal animals. We then got into touch with the Zoological Society, and the officials very kindly supplied us with the eyes of some nocturnal animals. The result of similar measurements, as you will see from a picture on the screen, was to show an extraordinary difference in the degree to which these violet and ultra-violet radiations were transmitted by the whole optical apparatus. We tried a few domestic animals, such as the ox, the cat, the owl, and then the tiger, and one or two other specimens. It was very interesting to find that the nocturnal animals had, in general, a very marked transparency to radiation which apparently was not at all transmitted through the human eye. The degree to which this was so will be made

much clearer by the picture than by words of explanation. The top spectrum is the analysed radiation from a cadmium vapour lamp, one which gives a very good range of radiation, consisting of closely-packed lines. It extends well into the ultra-violet radiations, up to about 2,200 Å.U. On interposing the human eye in front of the source of light before it is analysed, you will see that all radiations beyond *this* point (shown) are completely absorbed. I want to draw attention to this point, because I hope your President will lay further stress upon it; viz., that there is considerable absorption of radiations within the range of normal vision. It may possibly have some bearing on the extent or change of normal vision in different people; it may not be so much a matter of the retina as of those other components of the eye.



Here I show a typical photograph of the eye of one of the nocturnal animals I have mentioned; contrary to what happens in the case of the human eye, we get radiation transmitted beyond the range of normal vision. There is to be seen the interesting case of the night owl, in which a very considerable range of radiations are capable of reaching that retina transmitted through the various media, and this does not obtain in other cases. Here is an illustration showing the case of the tiger, an instance in which the range, though not so large as in the case of the night owl, is yet considerable.

There are one or two features of interest in connexion with this question of the nocturnal animals—and the term is usually restricted to animals which hunt their prey by night. One feature is that animals such as those which belong to the tiger tribe hunt partly by scent and partly by sight, whereas, I understand, in the case of the night owl the hunting is entirely by sight. It seems to me rather likely that this range of radiation entering the eye does contribute very considerably to the vision, because we have to remember that these shorter wave-lengths will give much sharper images than will other parts of the spectrum.

The observations are not necessarily conclusive on these points, because it may be questioned whether the conditions of experiment are such as to prove absolutely that there is really a selective absorption of the radiation. But I think the next slide will show that by a series of increased exposures of the photograph there is no appreciable increase in the extent to which the radiations can get through, and that is the test of a true absorption.

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Your President thought this demonstration might interest the Section of Ophthalmology and there are a number of points bearing upon vision, some elucidation of which may be afforded by these experiments.

II.—Sir ARNOLD LAWSON (President).

There are a few interesting speculations which occur to me with regard to these experiments.

I think it is permissible to assume that they furnish some explanation of the power of nocturnal animals to go about their business in a light utterly impossible for diurnal creatures to use.

It appears to me that there are three possibilities:—

(a) That the extra susceptibility to ultra-violet radiation actually increases the consciousness of light, so that the owl, or the fox or the cat really hunts by means of a light more or less invisible to us, but which provides an ample medium for the necessities of those animals. One would thus conceive that a dark night to a fox or owl appears no darker than that time of the day which we term twilight.

(b) It may be that the effect is shown rather by a greater power of image-formation than by an actual increase in the light itself. I mean that the nocturnal animal may actually visualize an object in very low degrees of illumination, not because it is conscious of more light by which to do so, but because it has an actually keener sight, an extra keenness which is conveyed to it by its longer visible spectrum.

(c) Or thirdly, the effect may perhaps react in both of the above directions.

So far as I am aware nothing has ever been discovered in the anatomy or physiology of the eyes of nocturnal animals to explain their strange power, and I think that the susceptibility to ultra-violet radiation affords a reasonable basis for further investigation and is the best explanation that so far has been offered on the point.

These experiments, too, although they have not been carried nearly far enough to render any dogmatic assertion possible, do suggest an explanation as to why people with apparently similar emmetropic refraction differ so widely in their power of image formation. Hitherto we have been content to accept the broad fact that emmetropia is simply an artificial standard, and that, for example, the person who can see $\frac{4}{4}$ is just an exception to the general rule. We accept the fact that he has a keener sense of image formation, but we cannot explain it. We know, too, as a fact that there is a very large difference in the length of the visible spectrum at the ultra-violet end with different people, and that some people are conscious of much shorter wave-lengths than others. It is possible, then, that in those persons who possess an extra keenness of vision we should find an extra susceptibility to ultra-violet radiation, and that the various gradations of keen sight may to some extent be explained in this way.

This is, of course, pure surmise, but it appears to me that a fair field for research is opened up, and that it is a field from which a great deal of valuable truth may in time be extracted.

Conversely, these experiments open up the question of night-blindness. May it not be that those who suffer from this symptom may in fact be suffering from a lack of ultra-violet appreciation? The facts just recorded by Professor Russ seem to warrant the possibility of such an assumption.

Discussion.—Sir JOHN PARSONS, F.R.S., asked whether Professor Russ had made experiments on the absorption by individual parts of the eyes investigated. Many years ago he (Sir John Parsons) did some experiments on rabbits' eyes, and the work was extended by Mr. E. K. Martin, in Professor Fowler's astrophysical laboratory at South Kensington. He did not think the results agreed very well with those arrived at by Professor Russ, at any rate in regard

to the human eye. One of the great difficulties experienced in finding the absorption to various wave-lengths by the lens or the cornea, for instance, was the refractive effect of the tissues themselves. In these experiments the tissues were perfectly fresh and were placed in a quartz cell and more or less squeezed out, so as to eliminate, as far as possible, any refractive effect due to the tissues themselves. It was very difficult, experimentally, to make a window in an eye like this and still get a satisfactory exposure and a satisfactory spectrum of the absorption by that means. He did not know of experiments having been previously done on nocturnal animals especially, but from Martin's work it appeared that there was ultra-violet radiation between 3,500 Angström units and the violet end passing through to the retina. But if ultra-violet radiation entered an eye, it ran the gauntlet of all the tissues anterior to the retina. One of the most important facts was that much was absorbed by the lens, and much was altered by the production of fluorescence by the lens; some fluorescence was also produced by the retina, by those comparatively long ultra-violet rays which succeeded in getting to the retina in the phakic eye. The question of the aphakic eye was different. He thought there must be some fallacy about the great absorption at the violet end in the visible spectrum for the human eye.

The sensitization of the eye produced by applying short wave-lengths of light was a very interesting experiment, and Professor Russ would no doubt agree that further investigation was needed. Some very fine work had recently been done by a physicist in Winnipeg, Professor Frank Allen, who had found what he termed reflex effects; i.e., that stimulation of one part of the spectrum increased sensitivity to stimulation by other parts of the spectrum. But was it quite certain that if this short wave-length radiation did get into the eye, it would increase sensitivity and acuity of vision? He thought that was doubtful. The eye showed chromatic aberration, and one could not accurately focus both the long wave-lengths and the short wave-lengths. In the case of the eye one focussed for some intermediate wave-length, which, according to Hartridge, was in the brightest part of the photopic spectrum, namely, the yellow region. If one was dealing with monochromatic illumination, it was a different matter altogether.

The research opened up a number of extraordinarily interesting questions, and he was delighted to know of the work, though he would like to hear more of the actual conditions.

Professor HOBDAY said that in Palestine during the war certain Australian horses were particularly selected for night work, because they did not suffer from night-blindness. He suggested that Professor Russ's further work might advantageously be prosecuted on eyes from horses which had been killed whilst in the wild state, on either the pampas of South America or the big farms of Australia, and he did not think there would be any insuperable difficulty in procuring such eyes. This subject was of keen interest to veterinarians, but unfortunately in the past they had not had the means of investigating it enjoyed by Professor Russ—means which he had used with such scientific precision.

Professor RUSS (in reply) said that in his desire to be brief he omitted mention of some points which Sir John Parsons' remarks showed were important. After the general run of experiments had been carried out, the eye was taken to pieces, and the cornea, the aqueous humour, the vitreous humour, and the lens were separately studied in regard to their absorption, and it was found that, of the four, the lens was the most absorbent, next came the cornea, and there was not much difference between the other two humours. The degree to which the cornea cut off radiation always showed itself as giving a little longer spectrum than did the whole eye, but it was seen that the absorption spectrum of the lens coincided with the limits of the whole eye. This showed that that sharp limit was set by the lens, not by the cornea.

The difficulty encountered in getting true absorption spectra had exercised the minds of others, including Dr. Martin, and Dr. Hartridge, of Cambridge; but it would have been noticed, in the photographs he projected on to the screen, that the ordinary lines of the spectra were somewhat converged, forming more or less a point image. Martin and Hartridge had produced spectra in which the line images were restored.

Another question was, how was it that this absorption was so marked by the lens? If observations of a rather different character were made, a ready explanation would be forthcoming in a very marked fluorescence of the lens. Fluorescence argued absorption, and so the matter tallied beautifully. An experiment could be done in the following way:—

An arc of mercury, or an ordinary carbon arc-lamp, was taken, and before utilizing any of the radiation, a piece of Professor Wood's black glass was interposed—this was made in this

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country by Messrs. Chance Brothers—and then one obtained a beam of practically pure ultra-violet radiation. The beam was transmitted through this black glass with practically no visible radiation. He said “practically” because if a slab a centimetre thick was used there was very great difficulty in seeing the source of light, even though it were a brilliant arc lamp; there would be a slightly tinged red light. Though it was absorptive over the whole range of visible radiation it had a transmission band in the ultra-violet. And, if one did not mind being the victim of the experiment, one looked straight at this light and got the observer to tell what he saw. He saw the lens of the eye of the subject under observation fluorescing a beautiful lavender colour. There was also a slight fluorescence of the cornea visible.

Therefore, though it was difficult to understand how these particular elements of the eye could be so absorptive, yet when we saw them fluorescing so vividly, we became reconciled to the view. The whole eye seemed to be filled with light.

(Sir JOHN PARSONS: What was the wave-length limiting the absorption of the lens?)

In the human eye (Professor RUSS replied) it was a rather longer wave-length than 3,800.

He had been much interested in the remarks made by Professor Hobday on the night vision of horses. He felt sure there was something at the back of these observations; the extraordinary differences which existed, not only among people, but in the animal world, in the fitness to go about in dim lights, were very difficult of explanation except on the basis of a great difference in the transparency of their eyes. After all, it must be largely a question as to how much energy was getting into the eye. If the eyes of nocturnal animals had an extra range of transparency, the difference seemed comprehensible.

With regard to chromatic correction, to anybody who had got their eye more or less corrected for bright lights this would be of no assistance, i.e., to have their eye flooded with extreme violet. It would be completely out of focus, and would not be of any help. He suggested that in animals which shunned daylight there was a natural degree of correction for that range of radiation for which they were better receivers.

Section of Ophthalmology.

President—Sir ARNOLD LAWSON, K.B.E., F.R.C.S.

Massive Exudate in the Retina.

By RAYNER D. BATTEN, M.D.

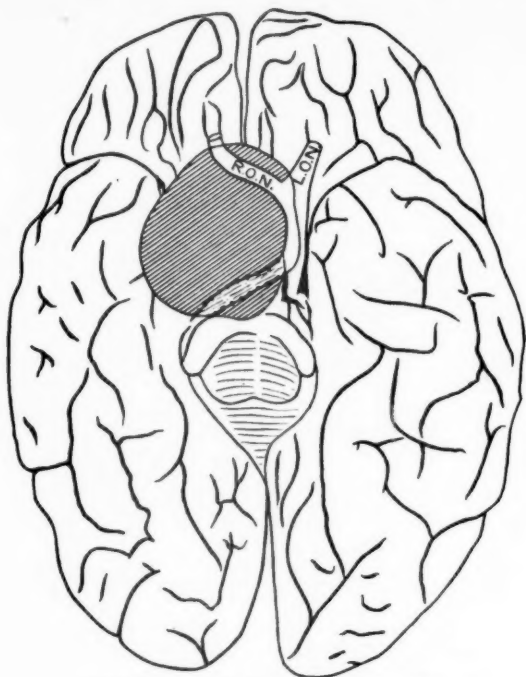
PATIENT, a man aged 64, has a massive exudate in the right retina and some exudative changes in the left. He has been under observation since October, 1922, when he had an extensive vitreous hæmorrhage in the right eye, and retinal hæmorrhages and white patches in the left macular area. His blood-pressure was then only 135/85 mm.; there was no sugar or albumin in the urine, the central nervous system was normal, and the Wassermann reaction negative. The patient did not exhibit the changes in the cardio-vascular system commonly associated with retinal disease. In 1923 the vitreous hæmorrhages had cleared up and the condition was as now seen. The changes in the left eye have been steadily increasing, both as to the area involved and the height of the swelling. The top of the swelling in the right eye measures + 6 D.

Massive exudates resembling those in the case shown are fully described and illustrated by H. Coppez and M. Danis in *Archives d'Ophthalmologie*, 1923, xl, p. 129.

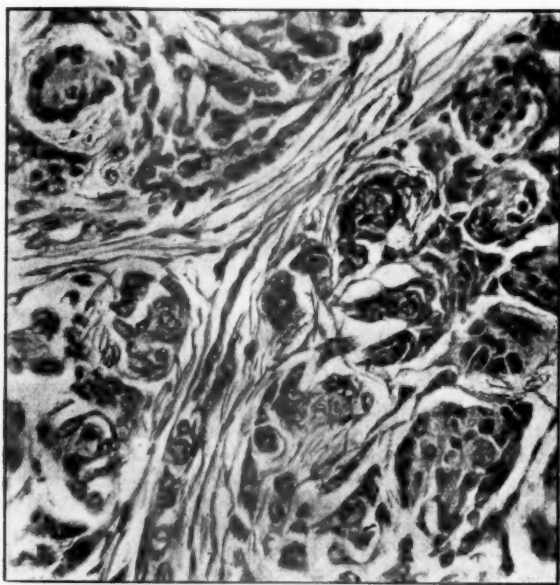
Cerebral Tumour displacing the Optic Tracts, Chiasma and Nerves.

By R. LINDSAY REA, M.D., F.R.C.S.

THE patient, a man aged 51, came to me on February 5, 1925, complaining of blindness of the right eye. The history was that the sight of the eye had failed somewhat suddenly just before Christmas. On examining the eyes nothing abnormal could be found except that the right disc was very slightly blurred, the edge was not quite sharply defined, but a definite papillœdema was not present. There was no vision in the right eye; vision in the left eye was normal. The field of the left eye was normal also. There was no change in the appearance of the vessels that would suggest a cause for the failure of vision in the right eye. The Wassermann reaction of the blood-serum was negative, so also were the examinations by the rhinologist and neurologist. One thing I omitted to do, namely, to make an X-ray examination of the skull. On March 9 the right disc appeared slightly pale. The general health was perfect, and the patient's usual work was done each day. On May 11 I noticed slight blurring of the left disc, and three days later there was a definite papillœdema. The patient was at once admitted to the West End Hospital for Nervous Diseases. A radiogram was made which showed a very much flattened sella turcica; the shadow of the pinna caused some confusion, making it appear as if a partly ossified tumour occupied this region. A decompression operation was performed, but the patient died two days later from meningitis extending from the base of the brain. The post-mortem examination, including that of the brain, was made by Dr. Carnegie Dickson. The actual specimen shows a large tumour, forty millimetres across, occupying the pituitary region. The tumour has flattened out the fossa. Extending upwards on the outer side of the right optic tract, it has pushed the tract across to the left side. The right optic tract and optic nerve are stretched and flattened, lying on the tumour. The left optic nerve and tract are not stretched nor pressed

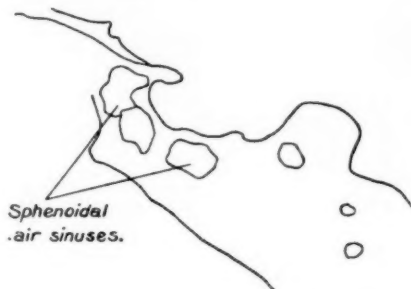


Base of brain with tumour (shaded) *in situ*.



Microscopical appearance of sections of tumour. (Carnegie Dickson.)

upon. The anatomical arrangement thus produced provides an explanation of the blindness of the right eye. It was the right optic nerve which suffered from stretching due to the growth of the tumour. The vision and field of the left eye remained normal throughout. The entire absence of symptoms suggestive of a cerebral lesion was remarkable. The patient sang a solo at his church on the Sunday before his admission to hospital.



The pituitary fossa enlarged, due to erosion by tumour.

I must thank Dr. Carnegie Dickson for doing the post-mortem examination and making the drawing of the brain.

Discussion.—Dr. W. E. CARNEGIE DICKSON said he thought that this tumour probably arose from the pituitary, but the specimen was recent and had not been fixed or cut. The pituitary was attached. If the tumour did arise from the pituitary, a curious point was that it had produced distortion of the optic tracts, chiasma and nerves from the right to the left, instead of coming between and opening them up. The nature of the tumour remained to be ascertained.

Mr. F. A. JULER asked whether there had been changes in the pupil. (Dr. FEILING: No when I saw him.) It was a long time for the optic atrophy to take to become apparent at the disc. In ordinary cases in which the optic nerve was ruptured in the optic foramen, three to four weeks was the usual interval before the discs showed atrophy.

In one case which he (Mr. Juler) had seen, the patient was suffering from total ophthalmoplegia due to some condition affecting the sphenoidal fissure. All the nerves of one eye were involved, including the optic nerve, and the patient was blind. That case had been shown by a house surgeon at a congress, who was asked how he explained the non-appearance of optic atrophy at the disc, although the patient was blind. His answer was that the blindness was of recent origin, and that probably there had not been time for the atrophy to develop. The present case seemed to show that pressure causing blindness might not, in a descending degeneration, affect the optic nerves and cause atrophy at the disc until four or five months afterwards.

Mr. J. H. FISHER said that if it was a pituitary tumour he thought the growth would have extended first to the right side. The right optic tract was displaced to the left side; there was no gross erosion of the sella turcica, and, possibly, there was some unusual anatomical arrangement of the cavernous sinus on the right side which enabled the growth to extend without much opposition in that direction. There was a gross stretching of the right optic nerve, and it would be the stretching of that nerve which induced the early blindness, rather than the compression of those fibres between the tumour and any bony structure at the base of the skull. As soon as papillædema revealed itself in the left optic disc, the diagnosis would be fairly obvious, namely, that there was implication of the intracranial portion of the right optic nerve, obstructing the conveyance of the raised intracranial pressure, but with sufficient pressure present to set up papillædema in the left eye. Whether it was a pituitary tumour remained to be decided. There were growths in the interpeduncular space which

were not of that character. He remembered a case of Mr. Lawford's in which there was an interpeduncular tumour of chondromatous character, and it produced symptoms analogous to those of a growing pituitary body tumour. It did not produce lateral displacement of the visual pathways in the way the tumour in the present case did.

Mr. LINDSAY REA (in reply) said that the optic nerves and tracts were originally in their proper anatomical relationship, but the sella turcica had been eroded. The tumour had flattened out the bone and, growing upwards and on the outer side of the right optic nerve and tract, had pushed these across to the opposite side. This stretching had produced the physiological want of continuity which had resulted in the blindness of the right eye. The left eye had remained normal from the end of December until the middle of May, when the papilloedema was first seen.

Mr. A. HUGH THOMPSON asked whether there had been any hemianopia in the left eye.

Mr. LINDSAY REA (in further reply) said there had been no disturbance of the visual field in the left eye. If he (the speaker) had taken a very minute tracing he might have discovered some defect, but hand perimetry did not show any. The patient did not complain, there was not the least suggestion of hemianopia, and vision had remained at $\frac{2}{3}$ to the last.

Mr. FISHER (in further comment) said that Mr. Lindsay Rea's last remarks confirmed his (the speaker's) contention that the causal mechanism was traction. The optic chiasma, instead of occupying a transverse position in front of the sella turcica, was displaced to the left side. Therefore fibres passing from the left optic nerve had no traction on them, and even those which were passing to the displaced right optic tract from the left nerve would exercise little or no pressure upon them.

Postscript by Dr. Carnegie Dickson.—Since the above discussion the tumour has been cut and examined. It was found to be an endothelioma intimately connected with the walls of the cavernous sinus, a small central mass being actually within the sinus, surrounding a small nerve trunk, probably the sixth cerebral nerve. The tumour consists of more or less undifferentiated sarcoma-like cells, i.e., cells which have a hyalomatous or "pulp-cell" arrangement, but with a tendency at parts to be arranged in "alveoli" or little rounded whorls. The cells are mostly irregularly rounded to polyhedral. The nuclei are small, pale and vesicular, with multiple nucleoli. There is little or no intercellular material.—W.E.C.D.

Acute Pulsating Exophthalmos: Ligature of Internal and External Carotid Arteries.

By A. E. A. LOOSELY, F.R.C.S.

PATIENT, a man aged 62, first seen on February 13, 1925.

History.—Diplopia of about six months' duration.

Examination.—V. = $\frac{6}{6}$ right and left. Paresis of left external and inferior recti. Nothing else abnormal.

February 27, 1925.—Eye proptosed, lids brawny, congested conjunctiva protruding through palpebral fissure, cornea could not be seen. Pulsation felt. Bruit, subjective and objective. No perception of light. This condition had developed in less than a week.

March 2.—Ligature of internal and external carotid arteries by Mr. Kerr. Pulsation and bruit ceased.

Conjunctiva partly sloughed, cornea ulcerated and perforated, and lens escaped. Eye then quieted down, with some symblepharon.

Case of Epibulbar Sarcoma.

By GEOFFREY VINER, F.R.C.S.

PATIENT, a female, aged 31, noticed a black spot in the eye nine months ago. This has been increasing in size. Her sight is not affected.

Operative Treatment of the Lacrymal Sac.

By G. H. POOLEY, F.R.C.S.

THOSE who, like myself, have to deal with ocular troubles in a large industrial area, have always a great number of cases of dacryocystitis to deal with. The condition is more frequently met with in women than in men, in the proportion of four or five to one. A considerable number of these patients live from ten to twenty or more miles away and cannot attend regularly. It is therefore important to find some quick and efficient method of dealing with them radically. The defects we have to deal with may be classified as:—

(1) Stenosis of the puncta; (2) stenosis of the canaliculi just before they join the sac; (3) obstruction in the sac itself without retention or regurgitation of inflammatory products; (4) obstruction with retention of inflammatory products which, on digital pressure, pass down into the nose; (5) obstruction with retention of inflammatory products which, on pressure, pass up through the puncta into the conjunctival sac.

(In all these cases the inflammation appears to be confined to the interior of the sac.)

(6) In a large and far more serious group of cases the tissues round the sac become more or less acutely inflamed, in many cases there is abscess formation and death of bone, and many present large red swellings, some of which have burst externally before we see them.

(1) and (2) I do not propose to dwell upon the treatment of stenosis of the puncta or of the canaliculi, which in each case should be that of stretching; the puncta may need incision or a three-snip operation.

(3) Obstruction in the sac without regurgitation may be kept quiet without operation; if not, it can be dealt with easily.

(4) Distension of the sac, the contents of which can be squeezed down into the nose, simply requires repeated digital pressure by the patient with the use of antiseptic lotions.

(5) Distension of the sac, the contents of which regurgitate on pressure, but apparently without external inflammation, may be treated by excision of the sac after injecting it with an antiseptic dye such as methyl violet and brilliant green (as I described in the *Ophthalmic Review*, 1913, xxxii, pp. 325-328). Even here a permanent drain into the middle meatus of the nose prevents epiphora.

(6) The acute suppurative cases give most trouble. For a long time I was content to incise and drain in these cases, following the drainage by fomentations until the discharge had ceased, when I excised the sac, removing also the lacrymo-ethmoidal cells and any dead bone; the results were fairly satisfactory, although there were a good many subsequent abscesses probably due to dead bone. The drawback was the length of time required before the treatment was completed; this was irksome to the patient and threw an unduly heavy strain on even a big eye department like that of the Sheffield Royal Infirmary, where I have sixty-six beds under my own control. Most of the cases that were not acute had to be placed on the waiting list, which became unduly large.

I next tried incision of the inflamed sac, removal of the lacrymo-ethmoidal cells and scraping of the nasal duct; this gave fairly good results, but there were some recurrences. Some cases were free from epiphora, but in a greater number it gave trouble. This fact set me thinking, and I came to the conclusion that there was no epiphora in those cases in which a free opening had been made into the nose when removing the lacrymo-ethmoidal cells or dead bone.

About the end of 1923 I began making an opening through the sac into the middle meatus of the nose, scraping out the sac and any granulations and firmly

packing the nose with ribbon gauze, which was left *in situ* for three days, after which it was removed. Two fishing-gut sutures, which had been inserted at the time of operation but left unfastened, were tied and the edges of the skin brought together; the operation only took about three minutes to perform, the results were satisfactory and the convalescence rapid. The patient could go home about ten days after the operation; fluid was syringed through the lower punctum into the nose on the seventh and each subsequent day till the patient went home.

Quite recently I have tried a further modification of technique as follows: I make a curved incision, about half an inch long, through the skin, beginning just below the tendo oculi and going right through into the sac; I make sure that the membrane which extends across from the ridge on the nasal process of the maxilla in front to the ridge on the lacrymal bone behind, and which covers the sac, is divided as far forward and as thoroughly as possible, then, taking Lang's sharp spoon and dissector I pass the dissector down the nasal duct so as to be sure of my position, and, using the spoon of the same instrument, I scrape out the epithelium and contents of the lacrymal sac and nasal duct using knife or scissors if necessary. Next I plunge the dissector into the cribriform part of the lacrymal bone and scrape out the lacrymo-ethmoidal cells. I then take a sharp gouge, four millimetres long on the cutting edge, and clear up the edges of the opening in the lacrymal bone and extend the opening upwards and inwards—to include that part of the nasal process of the maxilla behind the ridge—and nearly if not quite as high as the opening of the canaliculi into the sac. The opening is about six millimetres long from above downwards by three millimetres wide. I plug this with a bundle of thick thirty-day catgut, which keeps open the aperture between the site of the sac and the middle meatus of the nose. I suture the skin with two ophthalmic fishing-gut sutures. If there is any dead bone I remove it, also any sinuses in the skin. After the operation I foment the wound in septic cases and syringe through the lower punctum on the seventh and subsequent days. The patient can usually go home on the tenth day, but the syringing should be done twice a week for three more weeks.

The operation takes from five to ten minutes. I have now performed this operation on sixty patients; in some cases on both sides. In the last eleven cases I have used catgut plugs.

The results on the whole are most encouraging, particularly since I have used a gouge and catgut plugs. I have had a few cases of recurrence of inflammatory trouble; in three cases the catgut plugs have presented and been removed through the incision, but that is not surprising considering the severe sepsis; the end results are good, namely, little deformity, little epiphora.

The ages of the patients varied from eighteen months to seventy-six years.

There were two deaths, one from myocarditis during recovery from the anæsthetic, the other one from septic meningitis which had commenced before operation.

One must be careful to avoid injuring the nasal septum when performing the operation.

Discussion.—Mr. M. W. B. OLIVER suggested that instead of a catgut plug Mr. Pooley might try skin-grafting. Mr. Gillies had done a similar operation to Mr. Pooley's, but he used dental wax, and this was then covered with a skin-graft. He (the speaker) was not able to say whether it had proved satisfactory.

Mr. A. D. GRIFFITH said he was surprised that Mr. Pooley found he could do the operation so quickly with the gouge. He (the speaker) had found it tedious to get away the piece of maxilla. It had been easier to him since he used the Citelli punch.

Mr. J. H. FISHER said that here was a septic field in which Mr. Pooley was ready to operate, and it was necessary to operate and drain in these cases. Mr. Pooley's thorough method was to exenterate all the septic sac, and he even invaded any septic cells which might be adjacent to the sac. The nasal duct was obliterated, and there could be no inferior drainage from the granulating wound. The operator made sufficient and efficient drainage into the middle meatus by perforating the vertical plate of the lacrymal bone.

He (Mr. Fisher) saw no chance of the operation having any effect on the epiphora, and there could be no permanent drainage through the puncta and canaliculi. The drainage Mr. Pooley provided was a temporary one for the septic wound in which he was working.

With regard to Mr. Oliver's suggestion to use a piece of wax and make a graft upon it, that seemed to be a very unlikely improvement on Mr. Pooley's method, as a graft must perish under the septic conditions to which it was exposed.

Mr. OLIVER (in reply to Mr. Fisher) said he believed it was incorrect to say that a graft would not "take" on a septic surface; one could skin-graft on a septic surface, and it would take quite well.

Mr. E. D. D. DAVIS asked whether Mr. Pooley had had experience of West's operation for these cases, and if so, what were the results he obtained. He (the speaker) had done a great many dacryocystotomies through the nose, and the most successful were those in which there had been attacks of suppuration and dilatation of the sac. The more chronic cases, in which the canaliculi were slit up, or an external operation done, apart from excision of the sac, had been a failure. There was a possibility of a fistula between the nose and the face after Mr. Pooley's operation. Cases of dilatation of the sac of recent origin seemed to do better after West's operation.

Sir JOHN PARSONS said he thought there was some ambiguity as to what was the type of case upon which Mr. Pooley operated, e.g., whether he operated on mucocele as well as lacrymal abscess. These conditions were different, and required different modes of procedure. The mucocele did not require this type of operation at all.

He (the speaker) had been surprised to learn that the President penetrated into the lacrymal and ethmoidal cells in every case in which he removed the sac. He himself had not done it except where there seemed to be some definite necrosis of the bone, setting up a condition which was different from the ordinary mucocele due to obstruction of the duct. In the ordinary way one cleared out the upper part of the duct with a sharp spoon, without interference with the bone other than at the upper part of the duct, and this procedure gave good results. The excretory channels having been obliterated, one must expect these patients to have epiphora on abnormal stimulation of the conjunctiva by wind, &c., afterwards, and in emotional states. Most of the intranasal operations seemed to be unsatisfactory. The object of West's operation was to establish a communication between the interior of the sac and the nose. In many of the cases the opening rapidly closed up, and the patient was no better off than before. Toti's operation seemed to have given better results. But in one case in which the operation was done the patient had a good communication with the nose, and the general result was expected to be good, but she still complained of much discomfort. The result of the removal of the sac in cases of mucocele was much better.

With regard to lacrymal abscess, if Mr. Pooley's operation was intended to cut short the treatment of lacrymal abscess it stood on a better foundation when time was an important factor. He (the speaker) found no fault with the ordinary procedure of opening the abscess, packing it to the bottom and then leaving it for a considerable time before doing anything else, even if there was a fistula. The pathology of the cases of lacrymal abscess was different from that of mucocele, for in the case of mucocele the trouble was confined to the interior of the sac, whilst in lacrymal abscess there was a cellulitis, and if one interfered by operation, without providing good drainage, the result was unsatisfactory. Mr. Pooley had introduced a method of getting good drainage into the nose, but it appeared to be a somewhat risky procedure. If one was not overwhelmed with cases it was better to take some time over the matter, and, when the inflammation had subsided, use the fistula as a guide to the source of the trouble, namely, the remnants of the sac which were left at the bottom. He doubted whether in many cases the lacrymal trouble was combined with ethmoiditis.

Mr. R. AFFLECK GREEVES said he would have thought that an opening into the nose was to be avoided rather than otherwise. He had seen two cases in which, after making such an opening, there was a general infection of the ethmoid cells, which resulted in a chronic fistula. This was not put right until the whole ethmoidal area was cleared out.

Mr. POOLEY (in reply) said that his experience of operative work during the war had decided him against the use of dental wax; this blocked up the wound and was not pervious, so that drainage could not be carried on through it.

With regard to the operation by means of the gouge, as opposed to other instruments, though, as he had said, he had sixty-six beds for eye cases under his care, he was still getting

more patients than he could deal with; they allowed these conditions to reach a late stage before seeking advice; instead of coming when they had a mucocele, they waited until an acute abscess had developed.

He agreed with Mr. Fisher's remarks. He did not care about burying skin, particularly in septic cases. In many cases without subsequent epiphora he thought that epithelium did grow into the opening made, so that fluid got from the canaliculi into the nose. In some of the cases, when syringing a long time after operation, the silver needle would pass quite freely into the cavity of the nose; therefore there must be a permanent opening. He could not say how many years that opening would last, but his syringing and after-treatment was undertaken with the object of establishing it as early as possible. He used a bundle of coarse catgut in order that fluid should drain into the nose between the strands of catgut; the catgut should stay there some time, to give a chance for a lining to form before it was absorbed. More would be known of the results at a later date, and every effort would be made to improve the technique. He was impelled to read this communication now, because the method saved much of the operator's time and was economical from the hospital standpoint.

Where one could excise a mucocele, the junction of the canaliculi with the sac, or part of the canaliculi with the sac, and, in a clean case, suture the skin, the result was excellent, with no very tiresome epiphora. Probably the lacrymal gland did not secrete more tears than necessary, except in a strong wind or under dusty conditions. In those cases he thought the excision of the mucocele gave very good results. The question was whether there were not some cases near the border-line—cases of mucocele which were about to become externally inflamed, and in which to make an opening into the nose might be an advantage. He had not been able to discover whether epithelium grew from the canaliculi into the lining.

With regard to West's operation, an able rhinologist had done this operation many times in Sheffield. He (the speaker) understood that the operation, even in the most skilled hands, was a rather tedious one, and in less skilled hands there seemed to have been a somewhat high mortality, from pneumonia, &c. If the operation he advised were done from the outside, through a small oblique incision, no deformity resulted, and the condition could be dealt with more easily and thoroughly than through the nose.

With regard to the occurrence of a fistula between the nose and the face, in these cases if one took away the edges of any sinuses which existed they usually healed, and he had not so far had a single case of fistula as an after-result.

He had wondered in how many cases the trouble began in the sac, and in how many it commenced behind the sac, in the bone, or in the lacrymal-ethmoidal cells. On the whole, his conclusion was that there were more cases of trouble in the bone than he at first thought, and in many probably some dead bone was the *fons et origo* of the trouble.

The old method of dealing with these cases, from an external incision and packing them to the bottom, was slow, and caused more scarring than his (the speaker's) present method. One must be thorough as well as quick; sometimes he had completed an operation on a double sac in five minutes.

With regard to an opening into the nose being left, and causing ethmoiditis, if a small and incomplete opening was made there might possibly be something of the sort, but if the edges were clean cut to give a large opening he did not think this would happen; he had never seen anything like it.

The Safety Limits of the Jullundur Operation for the Intracapsular Extraction of Cataract.

By MONTAGU HARSTON, M.D. (Hong Kong).

WHEN Colonel Smith, I.M.S., of Jullundur, first introduced his operation to ophthalmological surgeons everyone was impressed both by the beauty of the operation and by the skill and ingenuity of its originator. When successfully performed it constitutes one of the most brilliant operations of surgery. It naturally followed therefore that he had imitators of the method who have not always acknowledged its original source. Filled with enthusiasm, I immediately proceeded to perform the operation in cases of cataract occurring in Chinese in Hong Kong; many of these patients had journeyed from the interior of China or had come down from the city of Canton.

I noticed that, although I followed Colonel Smith's minute and detailed directions as implicitly as I could, certain cases, for no obvious reason, were unsuccessful and prolapse of the vitreous occurred. At first I was puzzled by the lack of success, and, in searching for the cause, determined to ascertain the ocular tension in every case immediately before operation, i.e., while the patient was on the operating table. I found that I could always operate successfully within the normal limits of ocular tension, namely, 18 to 25 mm. of mercury as tested by the Schiotz tonometer, and that these limits must be rigidly adhered to. A tension of 26 mm. threatened disaster, and, on the other hand, so did a tension of 17 mm.

Sir JOHN PARSONS said that he had had no experience of this operation beyond that of watching the late Dr. Green, of Dayton, Ohio, perform it at St. Louis; and the results in the two cases he had seen were not such as would make one desire to emulate that surgeon. He (Sir John) thought "show operations" were the greatest mistake; no one was justified in doing these operations under the conditions which obtained in unfamiliar surroundings. He had seen the Barraquer operation performed at Portland, Oregon, and he had also seen a modification of it done in a strange hospital by the Greens, and he had not been impressed by the results. It was a mistake to operate in public; under such conditions it was difficult to assess the value of an operation. It was increasing the dangers for a questionable advantage. In operating for cataract, at any rate on Europeans, one should select that operation which gave the reasonably best result with the minimum of danger. He (Sir John) believed that these intracapsular methods must necessarily be accompanied by increased danger to the patient's eye, and hence were not altogether justifiable.

At a Clinical Meeting held at the Royal Westminster Ophthalmic Hospital, December 12, 1924, the following cases were shown:—

Mr. ELMORE BREWERTON: (1) Post-operative result of Conical Cornea; (2) Absence of Central Retinal Vessels on Optic Disc.

Mr. GEOFFREY VINER: (1) Bilateral Uveo-Parotitis; (2) Bilateral Brown Pigmentation of Chorea.

Mr. W. H. McMULLEN, O.B.E.: (1) Loss of lower half of Field of Vision in one Eye; (2) Choroiditis and Retinitis in a Woman, whose child had an eye excised many years ago; (3) Recurrent Retinal Hemorrhages of unknown origin.

Sir RICHARD CRUISE, K.C.V.O.: (1) Gradual Disappearance of Cysts of the Iris; (2) Modified Motais' Operation; (3) Result of Flap Sclerectomy.

Mr. FRANK THOMAS: Marginal Dystrophy of the Cornea (two cases).

Dr. GORDON HOLMES, C.M.G., C.B.E.: Loss of Voluntary Lateral Movement of the Eyes, with preservation of Reflex Movements.

Mr. A. LEVY: Papilloedema in a High Myope (Cerebral Tumour).

Miss IDA MANN (for Mr. LESLIE PATON): Leber's Atrophy.

Mr. MONTAGUE HINE: (1) Pituitary Tumour; (2) Unusual Form of Nodular Keratitis; (3) Obstruction of Central Retinal Vein.

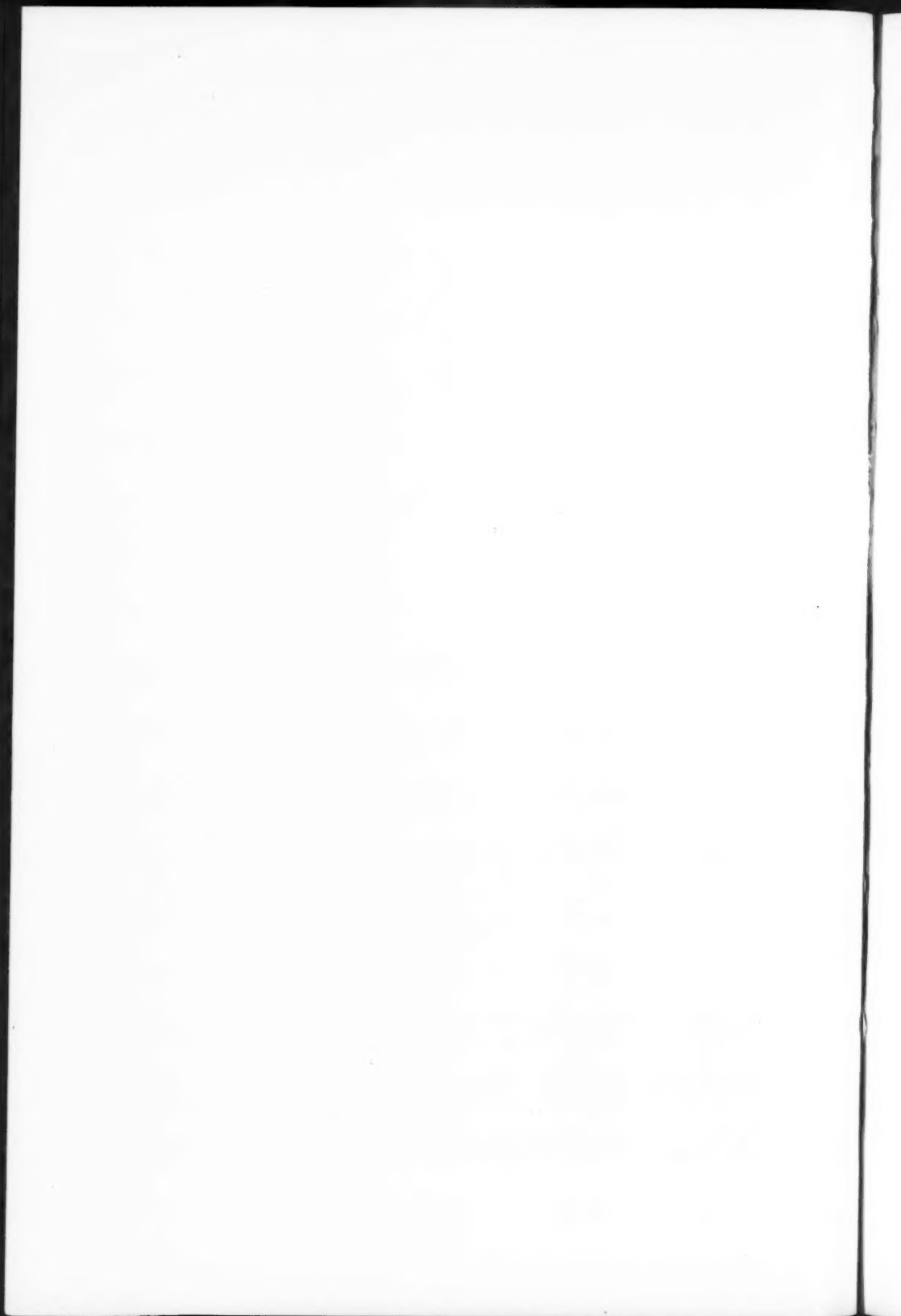
At a Clinical Meeting held at Guy's Hospital, March 13, 1925, the following cases were shown:

Dr. H. C. CAMERON: (1) Bilateral Facial Paralysis (Moebius); Nuclear Atrophy; (2) Cyclical Vomiting, with Ptosis.

Mr. A. W. ORMOND, C.B.E.: (1) Glioma of the Retina, treated with deep X-ray Therapy; (2) Detachment of the Retina, with "Hole" in Retina; (3) Arachnoidactyly; (4) "Hole" in Macula: Traumatic.

Mr. O. G. MORGAN: (1) Fibrous External Rectus; (2) Congenital Lagophthalmos; (3) Recurrent Third Nerve Paralysis in a Child.

Mr. J. HERBERT FISHER: Mikulicz's Disease.



Section of Orthopædics.

President—Mr. H. A. T. FAIRBANK, D.S.O., M.S.

Some Affections of the Epiphyses:

PRESIDENT'S ADDRESS.

By H. A. T. FAIRBANK, D.S.O., M.S.

It is my duty and privilege to open the session with an address, and as my subject I propose to take "Some Affections of the Epiphyses," showing radiograms of some of the less common conditions due to trauma and other causes. I shall begin with three traumatic cases, all of boys aged 16, all sent up from the same school by the same doctor, Dr. A. M. Amsler. These cases seem to me of importance since the lesion in each is easily missed unless perfect radiograms are available. They emphasize the importance of X-ray examinations in all cases, however trivial, in which it is possible that a lesion of a bone has occurred.

The first is an early slight separation of the femoral head, i.e., an early adolescent coxa vara. A common history in such a case is that of a series of two or three minor accidents—e.g., falls in the football field—leading in each case to a limp which finally becomes persistent, and increases. It is of the greatest importance to recognize this condition in its early stages and to take steps to prevent its increase, but it is rare in my experience for the surgeon to see a case before considerable displacement has occurred.

Case I. A boy aged 16, in September, 1922, fell on the right knee without hurting himself much, and on return to school he fell twice in the football field and hurt his right hip; after the second fall the hip continued to give pain; the pain was in the groin and on the outer side of the hip; a trip increased the pain.

When seen a month or so later he was limping, and the pain was increased by walking or even moving the hip when sitting. On examination the hip showed no deformity, but there was slight fullness in the groin, with tenderness there and over the trochanter. Abduction and internal rotation were limited; flexion and extension were only slightly limited. Shortening amounted to three-eighths of an inch, and there was slight wasting. There was pain on jarring the heel and also, though less marked, on jarring the trochanter. The

skiagram (fig. 1) shows widening of the epiphyseal line and definite, though slight, depression of the right femoral head on the neck. Weight-extension was applied and the leg put up in abduction and internal rotation, these two being increased day by day till extreme limits were reached, when the limb was fixed in plaster of Paris. It was hoped that the small amount of displacement of the head of the femur might be corrected, but unfortunately this result was not achieved. The plaster cast was left on for three months and a walking caliper worn for a year. No increase of the deformity occurred.

Case II. Also that of a boy, aged 16, who, after falling once or twice in the football field without hurting himself particularly, noticed pain in the left



FIG. 1.—Male, aged 16. Partial separation of head of right femur with slight displacement (early adolescent coxa vara).

hip while running, and found he could not kick with the left foot. After this game he played twice only as on each occasion he had experienced pain after five minutes' play. In an hour or so the pain had disappeared, and he only felt the hip on twisting or jumping. Walking caused no pain and there was no limp. Physical examination was negative when made three weeks after the onset, but some thickening of the trochanter had been noticed and this had been reduced with massage. The radiogram shows partial separation of the great trochanter, the space at the site of the epiphyseal line of the great trochanter being definitely wider on the left than on the right. Abstinence from all games for a couple of months or so effected a cure (fig. 2).

Separation of this epiphysis is usually complete and the result of direct violence. Although it might be expected that muscular action would be a not uncommon cause, the lesion usually results from direct trauma. A few cases due to muscular contraction have been recorded, but they are not very convincing. The commonest period for its occurrence is from the thirteenth to the sixteenth year. The displacement is usually not great, and the diagnosis is difficult. In the recorded cases, mostly reported some years ago, a surprising number seem to have been complicated by infection of the hæmatoma. Of



FIG. 2.—Male, aged 16. Partial separation of left great trochanter with slight displacement.

eleven cases (Poland¹) six were followed by suppuration and five of these died. Some seem to have been primarily cases of infective osteomyelitis with secondary separation of the epiphysis.

Case III. This showed an even rarer injury. A lad, aged nearly 17, complained that, while playing football and running all out, the left hip suddenly hurt him and gave way so that he fell, another boy falling over him. He finished the game, but with pain. When seen five days later, he said he had only a little pain on walking, but any attempt at running caused considerable

¹ "Traumatic Separation of the Epiphyses," 1893.

pain. He had no limp, and there were no physical signs. The radiogram shows separation of the epiphysis of the anterior inferior iliac spine, the centre for which appears at the fifteenth year or later. Games were forbidden for a few weeks (fig. 3).

This would seem to be a very rare injury. Poland gives a few cases in his wonderful treatise on epiphyses. This injury usually gives rise to greater disability than in my case, the hip being flexed, and it is difficult or impossible for the patient to extend the hip and the knee. Soft crepitus may be obtained. The avulsion usually occurs in sprinters in the violent effort of starting a race.



FIG. 3.—Male, aged 16½. Separation of left anterior inferior iliac spine. In stereoscopic films a clear area is seen on the left at the site of this iliac spine, while on the right the clear area is almost filled by the normally situated opaque epiphysis.

The reflected head of the rectus, which is not torn, prevents great displacement.

We may now pass on to consider the tubercle of the tibia. Whilst there is some doubt as to whether the so-called "Schlatter's disease" is necessarily traumatic in origin, though certainly aggravated and kept up by the pull of the ligamentum patellæ, there is no doubt that all types of cases are met with, from the complete traumatic avulsion of the tubercle to the type in which no displacement is present, and trauma seems to play no part in the onset.

The following case is at one end of the series, and is, I think, exceptional in that the plane of fracture passed up through the epiphysis into the joint, the anterior part of the articular surface of the tibia being displaced. In a boy, aged 14, attempting to jump a rope, something "gave" in the knee as he took off with the left foot. The skiagram shows the displaced fragment to consist of the anterior third of the articular surface of the tibia as well as the tubercle. At operation the fragment was forced back into place and fixed with a beef-bone screw, the drill hole being tapped to fit the screw. The result was entirely satisfactory (fig. 4).

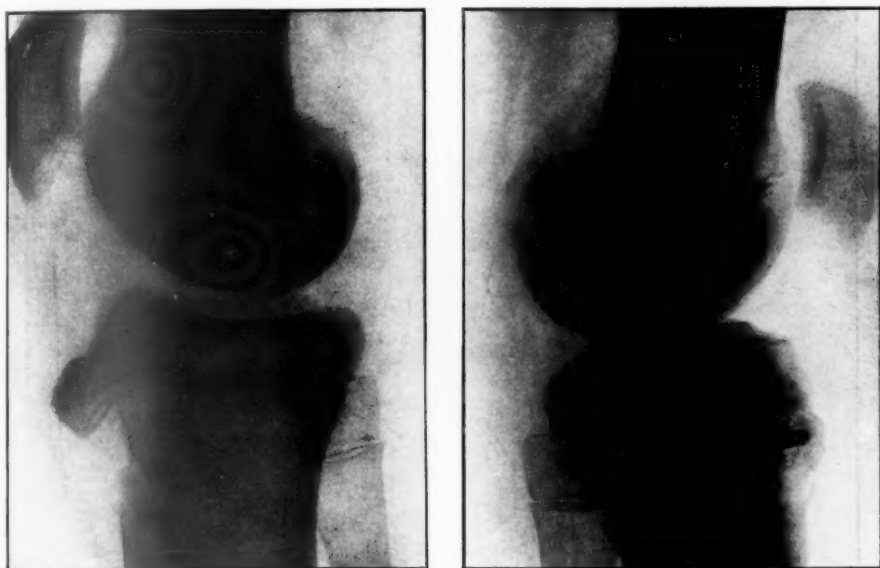


FIG. 4.—Male, aged 14. Avulsion of tibial tubercle, fracture running up into joint. Before and after operation.

The next skiagrams of "Schlatter's knee" are only shown for the sake of comparison with those of similar affections of other bones, of which I propose to speak. The first is of the knee of a boy, aged 14, who had had symptoms for four months. The radiogram shows the typical fluffy condition of the tongue-like process forming the tubercle and of the bone beneath it, with some irregularity of the intervening cartilage (fig. 5).

The next is the condition seen in a boy aged 13½, and the last of this type is from a boy of about the same age. Abstention from all games for a time has been sufficient in most of my cases to relieve symptoms, though the possibility of a recurrence cannot be said to have disappeared till fusion of the tubercle with the shaft of the bone has occurred.

I now want to refer to a less known, though not less common, affection of a similar nature, viz., osteo-chondritis of the heel epiphysis. This, like the affection preceding it, and that which is to follow, is an affection of an epiphysis to which a strong tendon is attached, so that trauma would naturally be expected to play a part in the causation. While in "Schlatter's knee" trauma plays a more prominent part than in the others, it is difficult to get away from the impression that a mild infection must play a part in all, and more particularly in the cases of the heel and of the navicular tubercle, to be mentioned later.

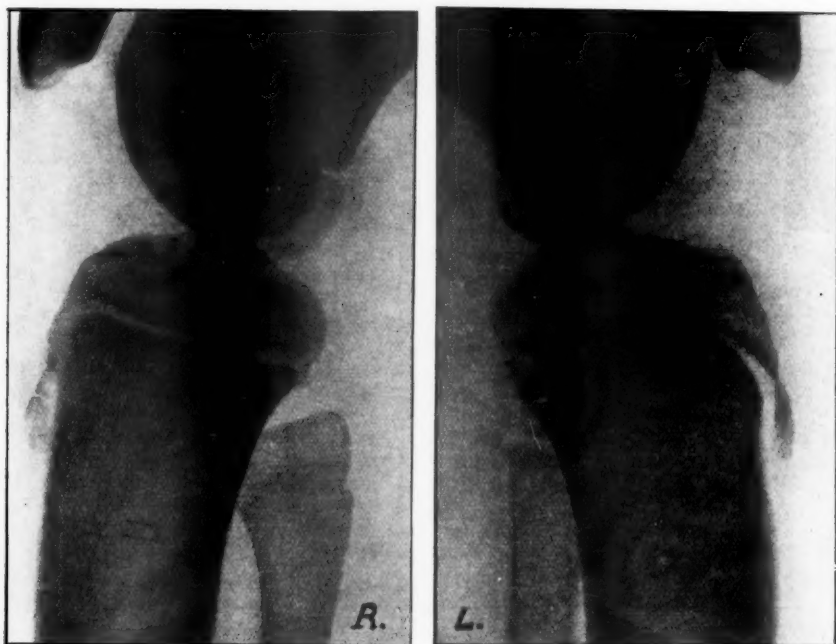


FIG. 5.—Male, aged 14. Osteo-chondritis of tubercle of right tibia, so-called Schlatter's Disease. Left leg normal, for comparison.

This infection and resulting inflammatory change is necessarily aggravated by the pull of the tendon attached to the affected epiphysis, and there is no doubt that violent exercise does increase the symptoms and that rest is followed by partial or complete relief. Whilst in the case of the tibial tubercle, as already mentioned, definite traumatic displacement, partial or complete, does occur as a result of violent action of the quadriceps, I know of no convincing case of separation of the epiphysis of the os calcis or of the navicular tubercle, though I have one case to show you of fracture of the os tibiale externum. The great majority, if not all, of the cases of supposed tearing off of the navicular tubercle are

really explained by the presence of a separate centre of ossification in this tubercle, the so-called *os tibiale externum*. Although in the affection under consideration an infective focus is probably present, it is not, however, easy to find as a rule. More than one writer has drawn attention to the similarity of these affections to pseudo-coxalgia and to Kohler's disease. In the case of the former, the femoral head being an epiphysis subject to considerable strain, the similarity is striking, but in Kohler's disease the radiographic similarity between the affections is not nearly so convincing, nor in this case is the bone—the tarsal scaphoid as a whole—subject to an unusual amount of strain.

Osteo-chondritis of the heel affects boys rather more commonly than girls and is more commonly unilateral than bilateral, the left heel being affected in my small series of fourteen cases much more commonly than the right. The ages of the patients varied from $8\frac{1}{2}$ to 15 years. In only one, in whom the affection was started by a jumping competition, was anything in the way of unusual trauma discovered. The diagnosis depends on local pain and tenderness and the X-ray changes. The pain is definitely in the heel and not beneath it, and is aggravated by violent exercise; the tenderness is on one or both sides at the margin of the epiphyseal line, and occasionally at the back of the heel: swelling may be present, but is often absent. Flat foot may be present, and occasionally there is some shortening of the calf muscles. Radiograms show a fluffy, eaten-out or fragmented epiphysis with unusual irregularity of the adjacent posterior surface of the body of the *os calcis*. Great irregularity of this surface has been met with, in a large number of radiograms specially examined, only when symptoms of the disease had been present at some time or other in the case. It must be remembered that the epiphysis may ossify by more than one centre and, as in the case of Schlatter's disease, radiograms must be examined in conjunction with the clinical evidence unless the changes are very marked or unilateral. This is apt to be a chronic complaint, and while only occasionally is complete rest, even in plaster, indicated, abstention from all violent exercise may have to be enforced for several months. Cure, as in the case of the tibial tubercle, takes place when fusion of the epiphysis with the rest of the bone occurs, but, fortunately, symptoms may subside long before this. The later the affection occurs, the better, therefore, the prognosis. One case of a relapse after an interval of two years was met with. I now show radiograms of three cases:—

(a) A girl, aged $8\frac{1}{2}$, with pain on the inner side of the left heel of some months' duration.

(b) A boy, aged 12, with pain in the right heel for two months. Irregularity of the sub-epiphyseal surface of the body of the bone is well-marked. Later the other heel developed symptoms (fig. 6).

(c) A girl, aged 13, with pain in the right heel of two months' duration. Complaints of pain and tenderness after running or jumping. Flat foot. Radiogram shows fragmented, fluffy epiphysis on the right side.

Precisely similar in nature is an affection of the tubercle of the tarsal scaphoid, to which is attached, of course, the *tibialis posticus* tendon. I have seen both the scaphoid and the *os calcis* affected in the same foot. It may be as well to say that I am not speaking of Kohler's disease, or isolated disease of the navicular, but of an entirely different condition. Here we are dealing with an affection of the navicular tubercle only, and not of the whole bone. A child or adolescent complains of painful flat foot, the navicular tubercles being unusually prominent, painful and tender, the tenderness being usually on the posterior aspect of the process rather than on the tip, which might well be

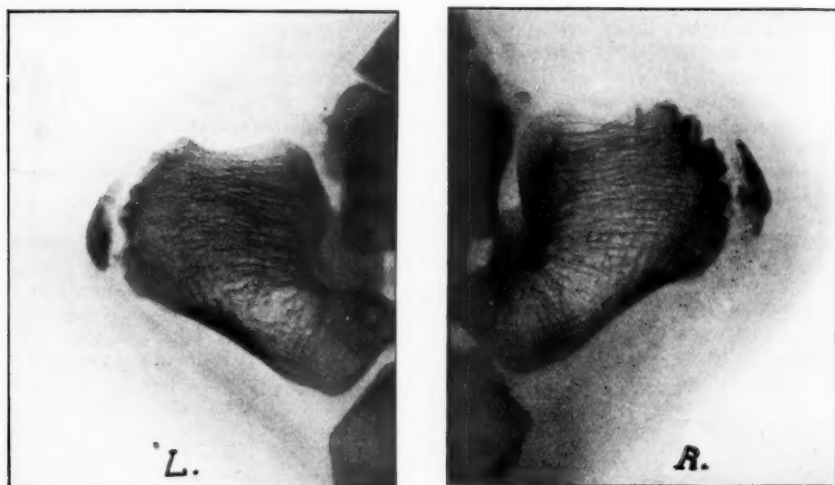


FIG. 6.—Male, aged 12. Osteo-chondritis of heel with symptoms in right only, but appeared later in left. Note irregular ossification of epiphysis and great irregularity of adjacent surface of body of os calcis.



FIG. 7.—Girl, aged 14. Osteo-chondritis of left navicular tubercle. Os tibiale externum present on both sides, that on left somewhat irregular.

rubbed by the boots. An interesting fact, and one I have not seen mentioned, is that almost invariably a separate centre of ossification is present in these cases for this tubercle, the so-called *os tibiale externum*. In eleven cases, with bilateral symptoms in seven, making eighteen feet in all, one or more centres of ossification for the tubercle were present in thirteen, in two others there is a questionably minute fragment of bone, while in one double case radiograms are not available to decide the point. The presence of an epiphysis at this point makes the anatomical conditions similar to those in the heel and the tibial tubercle. In only one or two has there been any suggestion of fluffiness of this epiphysis if we may so call it. Girls are nearly twice as commonly affected as boys, in contrast to the heel cases, the ages varying from $9\frac{1}{2}$ to 16 years in my series, the girls being rather younger. The sides are equally affected. The undue prominence of the tubercle, which is of course extremely common in painless flat feet in children, is often equally great on the two sides even though signs and symptoms are unilateral, while the *os tibiale externum* is usually bilateral. It is not known, so far as I know, what is the usual fate of this bone. It certainly may remain as a separate fragment throughout life, while the appearances seen in some feet strongly suggest that a separate bone had been present and had united to the navicular. The feet are almost invariably flat, and in rare cases the calf is contracted. These cases are not quite so obstinate as the heel cases. Treatment should aim at supporting the arches, and a varying degree of rest should be ordered according to the severity of the symptoms. Plaster may be advisable for a time. Exercises, &c., for the cure of the flat foot must be delayed till all pain and tenderness have disappeared. Radiograms from the following cases show typical examples:—

(a) Boy, aged 13, with local pain and swelling—for ten days only—of left navicular tubercle. Slight valgus. *Os tibiale externum* on both sides but larger on the left.

(b) Girl, aged 14, with bilateral flat foot. Prominent navicular tubercle on left. Tender at back. Pain for some weeks on walking much. There is some irregularity of the *os tibiale externum* on the left (fig. 7).

(c) Boy, aged 13, with prominence of both navicular tubercles, but only the right was tender at back. Three fragments of bone are seen in the region of the navicular tubercle (fig. 8).

This radiogram is from the case of a woman, aged 51, who had turned her right ankle in 1923, and had had pain in the foot ever since. An *os tibiale externum* is present on both sides, but on the right the bone is apparently fractured. I have not met with a similar radiogram. A month's rest was followed by return of pain as soon as she got up again. Removal of the bone was recommended.

The next radiogram shows a separate centre of ossification for the internal malleolus. This is a very rare condition, apparently unknown to many anatomists. Curiously enough I have met with five cases. In three the condition was bilateral, in two unilateral. Four had severe flat foot with pain, one also having peroneal spasm; the condition was only revealed by routine X-ray examination. Only two showed any local signs, and in these the tenderness, and in one also some swelling behind and below the malleolus, rather suggested teno-synovitis of the *tibialis posticus* tendon, but this could not be connected, directly at any rate, with the presence of this separate epiphysis. The radiogram shown is from the case of a girl aged 10, who complained of pain and flat foot. She had been kicked on the right ankle five months previously and when seen there was a puffy, tender swelling

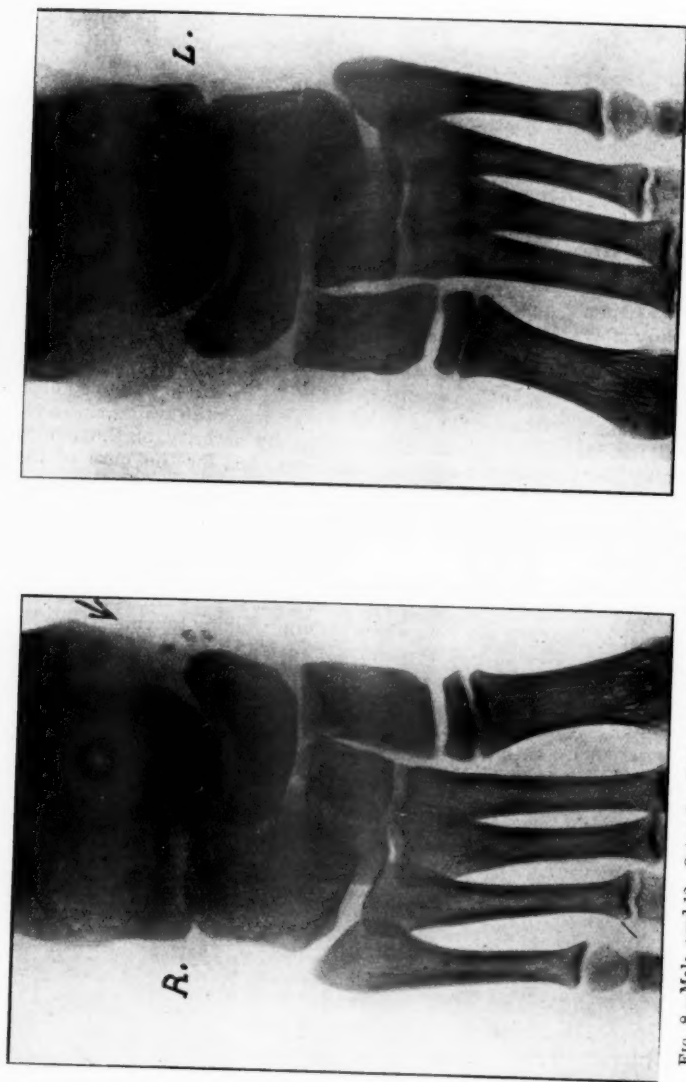


FIG. 8.—Male, aged 13. Osteochondritis of right navicular tubercle. Right navicular shows three separate centres for tubercle.

below the internal malleolus on this side. The separate epiphysis is bilateral. It is extremely unlikely that the existence of this epiphysis had any relation to the symptoms. (Fig. 9 is taken from another case; unilateral.)

Lastly, I show you a case of dislocation of the elbow with separation of the epiphysis of the internal epicondyle. During reduction by the doctor, by some chance, the separated internal epicondyle was caught between the humerus and ulna. At operation we were able to free the detached piece of bone, and

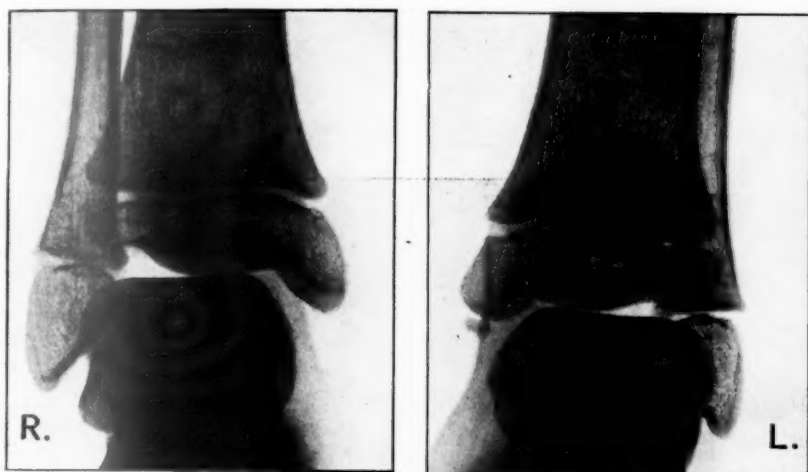
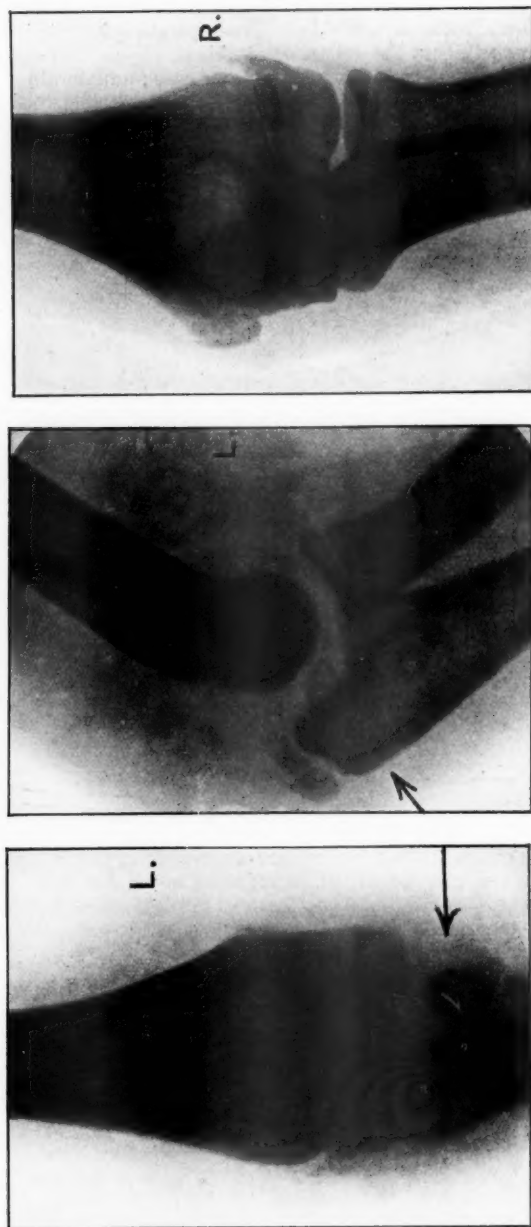


FIG. 9.—Girl, aged 8½. Extra centre of ossification for left internal malleolus.

after drilling it, to 'stitch it back somewhere near its correct position. This appears to be a very rare condition, though, as often happens with rare affections, four such cases have been met with in the last two years: in each the diagnosis has been confirmed by operation. It seems advisable to keep these cases in mind, particularly when recovery is delayed after reduction of a dislocation of the elbow (fig. 10).

My sincere thanks are due to the radiologists responsible for the radiograms of these cases, and particularly to Dr. Knox and Dr. Shires.



Left
 FIG. 10.—Girl, aged 12. Displaced internal epicondyle caught between bones on reduction of dislocation of left elbow.
 (A. P. views are reversed, and show view from behind of both elbows.)
 Right (normal)

Section of Orthopædics.

President—Mr. H. A. T. FAIRBANK, D.S.O., M.S.

A Case of Destruction of the Os Calcis, with Deformity of the Foot.

By E. LAMING EVANS, C.B.E., F.R.C.S.

A. D., AGED 10, male.

History.—At age of 4, suffered from multiple foci of osteomyelitis, affecting both humeri, right femur, both tibiae, and left os calcis. The original infection is said to have started in the left os calcis.

Patient first seen in August, 1922, when there was a discharging sinus over the right tibia. The left foot was then in calcaneus. Pending healing in the right tibia, the left foot was held in a moulded leather splint at right angle to the leg.

Present Condition.—Right tibia now firmly healed, no evidence of active sepsis. Left foot still in marked calcaneus, and useless as a lever. X-ray examination showed loss of posterior half of os calcis, with ankylosis of ankle-joint, and fails to disclose any epiphyseal cartilage at lower end of left tibia. Free mobility at subastragaloid and mid-tarsal joints. Triceps suræ active, but normal attachment of tendo Achillis is replaced, on account of loss of posterior half of os calcis, by thin fibrous union to fibrous scar attached to remains of os calcis.

The aim of correction must be to bring the foot to a right angle with the tibia, in which position the triceps suræ will become active as a plantar flexor and a functional lever.

I suggest that the most hopeful method of operation will be the removal of a wedge from the tibia, the base of the wedge being directed posteriorly.

DISCUSSION.

Mr. H. A. T. FAIRBANK (President) said he was of opinion that there was some movement at the ankle, and suggested Whitman's operation.

Mr. R. C. ELMSLIE suggested a wedge operation above the ankle-joint if it was found movable at the time of operation.

Mr. D. McCRAE AITKEN said he agreed with the President that there seemed to be some movement at the ankle, and suggested a horizontal section through the astragalus.

Locking of Elbow due to Large Loose Body.

By PAUL BERNARD ROTH, F.R.C.S.

JOHN H., aged 45, an engine-driver on the Southern Railway. When seen on July 3, 1924, stated that for last twelve years he had had something the matter with his right elbow. Something would catch in it; it would become painful suddenly when bent, e.g., as in washing his neck, and he would have to seize hold of the wrist with his other hand and twist it free. This had happened "thousands of times," but during the last week it had been worse than ever, and so he decided to come to hospital.

He is employed on a section of the line where the engine levers are all worked with the left hand, and so he has been able to do his work satisfactorily. But his

14 Roth: *Locking of Elbow*; Bristow: *Achondroplastic*

elbow has frequently become locked when he has been driving an express. As far as he knows, he has never injured the elbow.

On examination, the right elbow was found slightly larger than the left, and slightly hot. It could be flexed to 60° and extended to 155° , pronated fully, and supinated $\frac{4}{5}$. No grating on movement. X-ray showed a large loose body in front of lower end of humerus, and a small one tucked in between head of radius and lesser sigmoid cavity of ulna, there being a depression for it in the latter. Marked signs of osteo-arthritis present.

Patient advised to have bodies removed.

At operation on July 8, 1924, the joint was approached from in front, between supinator longus and tendon of biceps. The large body was found to be quite loose in the joint: it could be pushed up in front of humerus, or down into a depression between coronoid process and neck of radius. It was in a pouch formed by anterior part of capsule of elbow-joint: when this was opened the finger was able to explore the joint and extrude it. No other loose body was found. It was a flattened oval, $\frac{3}{4}$ in. by $\frac{1}{2}$ in. by $\frac{1}{8}$ in. about, and pitted all over.

When seen on October 24, 1924, patient stated he was relieved of all his symptoms, and the amount of extension was increasing.

Achondroplastic Dwarf.

Shown by W. ROWLEY BRISTOW, F.R.C.S.

PATIENT, a female, aged 22, 2 ft. 6 in. in height.

Shape, size of head, hair, skin and breasts normal; mentality normal.

The limbs are very short, and the fingers splayed.

The trunk is distorted by a severe scoliosis, which renders standing and walking painful.

The range at nearly every joint of the limb is limited, owing to the abnormal shape of the ends of the bones.

The case is of interest as the hands are atypical, and the X-ray shows considerable rarefaction. The scoliosis, which is the prominent symptom in this patient, is not usually a feature in achondroplasia.

So-called Rickety Dwarf.

Shown by W. ROWLEY BRISTOW, F.R.C.S.

THIS boy was admitted to St. Thomas's Hospital in June, 1923, when aged $5\frac{1}{2}$, for rickets, and was treated by correct diet, fresh air, artificial sunshine and cod-liver oil. In October, 1923, the X-rays still showed signs of active rickets and he was accordingly transferred to the country hospital, where he has been up and running about for the past year.

Three series of radiograms are shown: (1) Taken June, 1923, when first seen; (2) taken October, 1923; (3) taken October, 1924.

The interest of the case lies in the diagnosis. The original radiogram of the wrist suggests rickets. Patient presents deformities such as one would expect if weight were borne on soft bone, but the X-rays of the present date do not show the changes that one is accustomed to see in healed rickets. The radiograms rather resemble those seen in cases of diaphyseal aclasis: but there are no cartilaginous or bony nodules to be felt or seen.

The renal deficiency has not been tested, but clinically there is nothing to show that the kidneys are not working satisfactorily. His general health is excellent.

The case is entitled "continued rickets," but it is shown because the X-ray changes are atypical. It would seem to be a type of bone deficiency resembling osteomalacia in some respects.

Case of Traction Injury of the Brachial Plexus.

By H. A. T. FAIRBANK, D.S.O., M.S. (President).

D. O., GIRL, aged 8½. On August 3 she was knocked down by a motor-car, receiving a scalp wound over the left eye. She was unconscious for a week. No further history, except that paralysis of left arm dates from the accident.

On admission to hospital on October 18: No sign of superficial injury except scar on scalp. Left arm almost completely paralysed. No voluntary power below shoulder except in the triceps, which contracts feebly. No power in the deltoid, but arm can be raised, in combined flexion and abduction, apparently by the spinati, which are certainly acting. The pectoral is paralysed, and also, apparently, the rhomboids, but this is not quite certain. The serratus magnus acts feebly, but the latissimus and teres muscles are paralysed.

Sensation.—Complete loss of sensation to touch and pin-prick except over upper half or so of the deltoid area, and on inner side of arm nearly as low as elbow.

Passive movements of limb free, except for some limitation of external rotation at shoulder-joint and of supination of forearm.

Electrical Reactions.—Some response to faradic current in triceps, biceps and deltoid only. "The galvanic response is sluggish in all the forearm muscles, and in those supplied by the median difficult to obtain."

Left pupil persistently smaller than right. No difference in palpebral fissures.

X-ray examination negative.

Diagnosis.—Traction injury to the brachial plexus ("whole arm" type) with injury to the cervical sympathetic. It is likely that the lesion is intrathecal, the nerves having been partially or completely pulled out of the spinal cord.

As the prognosis in the "whole arm" type is so bad, it is thought advisable to explore the plexus without further delay, though the chance of finding a lesion or lesions amenable to surgical treatment is admittedly small.

Note.—At operation the whole brachial plexus was found to be surrounded by fibrous tissue, but the fibrosis did not extend into the substance of the individual nerves. The fifth and sixth cervical nerves were intact, but the seventh and eighth were found to end above, just at the intervertebral foramina. The seventh cervical showed a small filament of nerve tissue passing in through the foramen but the eighth cervical was completely torn. The proximal ends of these roots could not be seen. It seems probable that the lesion in the case of the fifth and sixth, and possibly also of the other nerves must have been close to the cord, if not actually where the nerves emerged from the cord. Nothing could, of course, be done to remedy the damage.

Case for Diagnosis: Multiple Arthritis (? Type) and ? Spastic Diplegia.

By ST. J. D. BUXTON, F.R.C.S.

M. M., FEMALE, aged 18, an only child. Mother alive and father deceased.

She has been crippled as long as she remembers. She has been under my observation five and a half years, and when first seen could only walk by holding on to a chair

16 Buxton: *Case for Diagnosis*; Heath: *Ganglion on Wrist*

and with a scissors gait. The condition of the joints has altered little during this time, but the legs are less spastic.

Present condition: Incomplete ptosis on right side. Pupils react to light.

Arms: No inco-ordination, no tremor, no paresis, no spasticity.

Legs: Spastic, with limited movement. Knee- and ankle-jerks active. Plantar response: tendency to extensor.

Joints: Spine, slight scoliosis; shoulders, normal; elbows, slightly limited extension and flexion.

Wrists: Fluid present on the dorsum; restricted movement.

Hips: Thighs adducted and abduction is limited.

Knees: Almost normal.

Ankles: Equinus deformity of feet, with tight tendo Achillis.

Wassermann reaction negative.

Right tendo Achillis was lengthened by Mr. Fairbank in 1920, and she was kept on abduction apparatus for nine months, and then re-educated in walking. No source of infection which might have been the primary focus of the joint infection was found. No glandular enlargement; spleen not palpable. No cardiac disease found.

Skiagrams of affected joints exhibiting the appearance of rheumatoid arthritis are shown.

Discussion as to the nature of the disease is invited, and the following questions are asked: (a) Has the patient one disease? If so, what is its nature? (b) Has she an arthritis of unknown origin and a spastic diplegia? (c) Is the ptosis a manifestation of the same condition that has caused the diplegia?

Ganglion on the Dorsum of Wrist associated with a Cystic Condition of the Os Magnum.

By P. MAYNARD HEATH, M.S.

MISS E. W., aged 34, first noticed a swelling on the back of the left wrist in 1923. It was incised on two occasions by her private doctor. The swelling recurred and in April, 1924, it was excised under local anaesthesia. It proved to be a simple ganglion surrounding the extensor tendons and attached to the back of the carpus. Soon after this it was noticed that there was a bony swelling on the back of the wrist and that the wrist was painful. An X-ray photograph showed an appearance of cysts in the os magnum. The wrist was put at rest in a plaster splint, but the condition has remained unchanged for six months. At no time has there been any appearance of inflammation about the wrist.

Two Cases for Diagnosis: ? Pseudocoxalgia.

By G. PERKINS, M.C., F.R.C.S.

Case I.—Patient, a girl, aged 9, with pain in the right knee on and off for nine months. There has been no treatment except for a brief period of confinement to bed.

When first seen a week ago the right hip was held in a position of 45 degrees flexion, 10 degrees adduction, and 15 degrees external rotation. There was no tenderness or thickening of the head or neck. No movement was allowed at the hip-joint owing to protective muscular spasm. Slight muscular wasting only was noted and there was no true shortening. The radiogram reveals: slight widening and shortening of neck of femur, rarefaction below epiphyseal line, absence of

marked bony atrophy; lateral displacement of epiphysis, the ossific centre of the epiphysis of uneven density, some parts showing diminished and others increased density; normal cartilage covering the articular surfaces of femur and acetabulum, as judged from the joint space; and an asymmetry of the upper part of the pelvis due to asymmetrical exposure, and also of the lower part of the pelvis due to persistent flexion at the hip-joint.

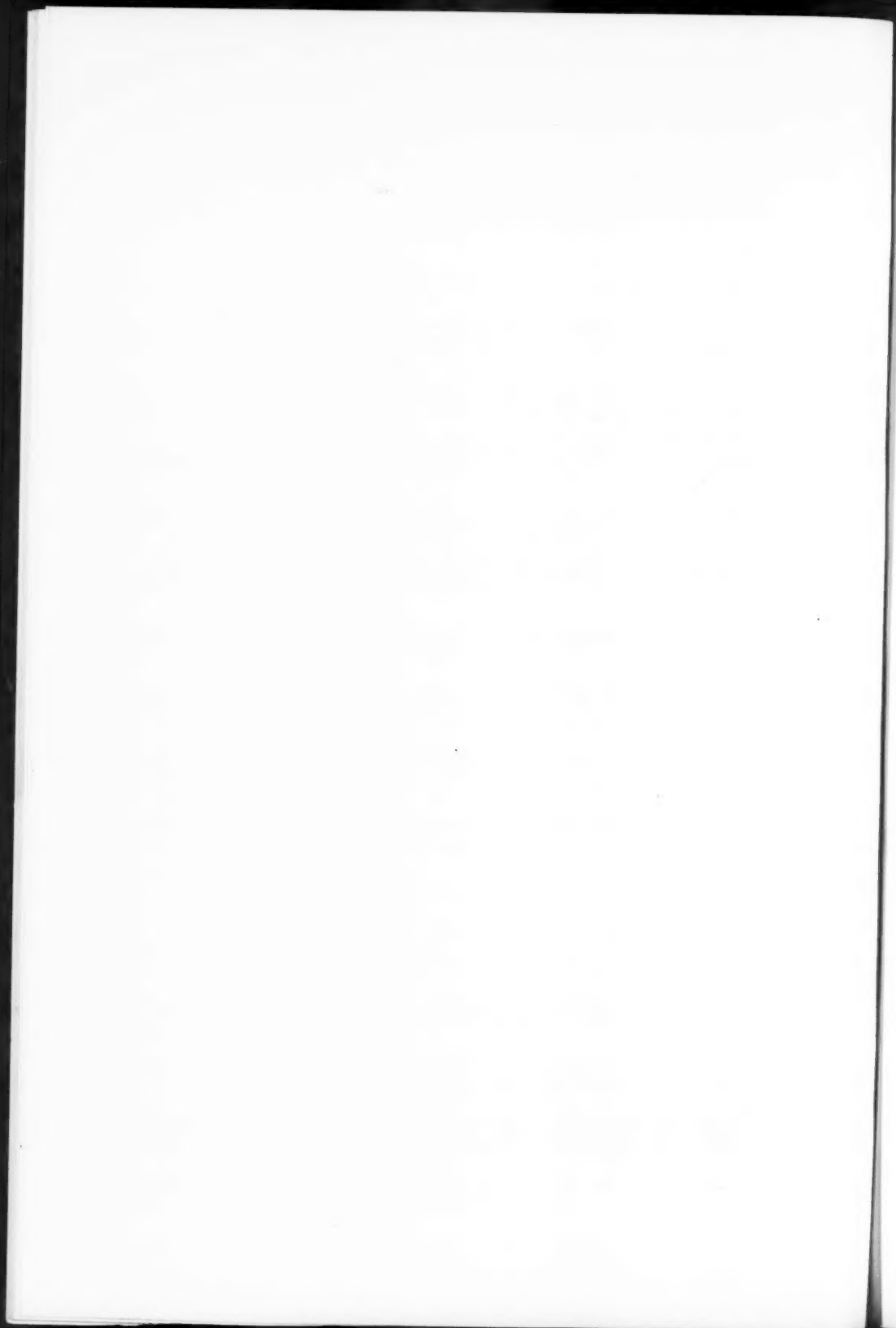
Patient was put to bed and traction applied to the limb five days ago. The deformity of the hip quickly disappeared. The adhesive traction was removed last night and the hip joint has immediately again acquired a flexion-adduction deformity.

I suggest that this is an early stage of pseudocoxalgia.

Case II.—Patient, a boy, aged $13\frac{1}{2}$, with a history of five months' limp. No pain; no trauma.

When first seen last week, there was a slight flexion-adduction deformity of the right hip, and all movements of the joints were prevented by muscular spasm. Since then he has been resting in bed, without splintage. The movement at the joint is becoming more free, but there is considerable limitation in abduction, adduction and rotation, and slight limitation in flexion and extension. Muscular wasting is slight. The great trochanter is prominent, and there is, perhaps, a slight thickening of the neck of the right femur to be palpated. The limbs are of equal length.

The radiogram shows: slight flattening and spreading of the ossific centre of the head of the femur, and an alteration in its normal uniform density; no rarefaction, and a normal joint-space. I suggest that this is a late stage of pseudocoxalgia.



Section of Orthopædics.

President—Mr. H. A. T. FAIRBANK, D.S.O., M.S.

DISCUSSION ON THE DIAGNOSIS AND TREATMENT OF AFFECTIONS OF THE SACRO-ILIAC JOINTS.

Mr. P. JENNER VERRALL.

THERE are still many branches of orthopædics which are imperfectly understood, and afford opportunities for investigation; the sacro-iliac joints are not the least important of these. I am opening this discussion in a spirit of extreme modesty and hope that considerable light will be thrown on a difficult subject.

When one considers that the whole weight of the superincumbent portion of the body must be borne by the sacro-iliac joints, and that the force directed upon them is not a direct compression but a resolved force involving a shearing strain, it is a matter of wonder that disabilities of these joints are not more common. It might appear at first as if nature would have done better to form a bony ankylosis here, but primarily the claims of parturition, and secondarily the need of shock-absorbers, have led to the development of the existing mechanism. It has been stated by high authorities that disabilities of these joints are very rare but I cannot help feeling that this is not the case. American surgeons have studied the subject more exhaustively than we have, and this is a disadvantage which we should hasten to remedy. Incidentally, perhaps, physio-therapists have more accurate knowledge of this matter than the majority of surgeons. It is the duty of the opener of a discussion to give some indication of the lines on which the discussion should proceed, and I feel that most attention is due to the question of differential diagnosis. In the sacro-iliac joint we are in the curious position of finding it more difficult to decide whether the joint is at fault than to decide in what way it is at fault and to treat it. Except in the case of serious and advanced disease X-rays will not help us and we must rely on clinical observation.

The joint is so tortuous that the antero-posterior view does not show minor displacements with any accuracy. Whatever the affection of the joint may be, the same symptoms occur to a greater or less degree, and it is the recognition of a certain clinical picture that is our best guide.

The patient stands or walks in a characteristic way, obviously seeking to spare the affected joint. Smith Petersen, in a recent paper, observes that in mild cases there is a tendency to deviate the body towards the affected side, owing to involuntary muscle spasm tending to hold the weak joint together, while in bad cases the deviation is away from the affected joint owing to the throwing of the body weight on to the sound side. This is reasonable, but my own observations would suggest that the latter condition is by far the commoner, being associated with an upward tilting of the pelvis on the affected side. Most characteristic is the manner in which a sacro-iliac patient goes upstairs. He mistrusts his bad side. Holding the bannister he carefully and painfully composes himself on his bad side until he feels safe, rapidly places his good leg on the step above, throws his weight on to it with obvious relief, and draws the bad leg after him. My description is inadequate, but there is a definite difference between this obvious careful seeking after a painless posture and the gait of, say, an arthritic hip case, where the joint is painful in all positions. Consideration of the history will elicit an account of sudden unilateral strain, of pain following pregnancy, or of gradual unexplained onset.

The symptoms of which the patient complains are usually characteristic, their severity depending naturally on that of the disease. Sacro-iliac affections for some reason or other seem productive of a high degree of neurasthenia, which naturally leads to exaggeration of complaints. After frequently questioning such patients as to what it was that upset them, I have almost invariably been told that it is the feeling

of extreme instability, of "falling to pieces," that worried them. This is the most typical symptom of sacro-iliac disease. Pain will be felt over the joint, and as the joint is supplied by all the nerves near it—lumbo-sacral cord, first and second sacral, superior gluteal, and (according to Smith Petersen) the obturator—the pain may be, and commonly is, referred to the peripheral distribution of these nerves, and complaint will be made of sciatica, pain in the buttock, &c. This pain is increased by standing or walking, and especially by any sudden jar, as in going downstairs, though this symptom is less pathognomonic than the text-books would have us believe.

Examination should first be made with the patient standing. The typical gait and method of climbing stairs will be noticed and also the deviation of the lumbar spine and pelvic tilting. In the recumbent position we shall look first for swelling or abscess either over the joint, in the buttock, over the lumbar spine, *per rectum*, or along the iliacus. Palpation will reveal deep tenderness over the joint. It is very important to remember that it is deep tenderness which is symptomatic. Superficial tenderness over the joint is common and indicates referred pain from some other source—the spinal, intra-abdominal and especially the intrapelvic regions (as from uterine displacement). Pain felt over the joint on compression of the iliac crests or of the os pubis is conclusive evidence. Its absence should make one very reluctant to diagnose a sacro-iliac lesion. This is the most valuable test of all. On testing movements with the patient recumbent we find that hip movement is practically normal so long as the knee is flexed and the hamstrings consequently relaxed, but with a straight knee, pain is felt over the joint as soon as the hamstrings begin to tilt the pelvis. Smith Petersen points out that this is well shown by the relative ease of active spinal flexion when the patient is sitting on a chair with bent knees. There is no true psoas spasm as in hip and spinal cases, and this, combined with free hip movement, with flexed knee and the absence of rigidity in the lumbar spine, will serve to differentiate these areas.

Strains of the muscles and ligaments in the region of the sacro-iliac joints are common and are due to the same causes which affect the joints themselves. Local tenderness, change of posture and resistance to pelvic flexion may be present, but the characteristic gait, feeling of instability, and above all pain on compression of the ilia will be absent. Sciatica of different origin may be excluded in like manner.

Referred pain is the commonest pitfall, while affections of the lumbo-sacral joint may in general be recognized by noting that both sides of the body are equally affected, by the manifestation of greater pain and deformity, by the location of tenderness, and by skiagram. On these points I hope to hear the opinions of others.

Having decided that the sacro-iliac joint is at fault our next query is as to the nature of that fault. (1) *Tubercle* of this joint is fortunately rare as it is a very severe and fatal disease. Skiagraphy in the early stages will tell us nothing, but as soon as the disease is well established, evidence of destructive arthritis and of abscess is forthcoming. Abscess is a common complication, and this when found in any of the above-mentioned situations will clinch the diagnosis. It must be remembered that sacro-iliac tubercle, unlike tubercle in other joints, is essentially a disease of adolescence and early adult life, and also that the disease may begin without giving rise to any symptoms whatever, abscess being the first sign. Next in order of severity comes (2) *gonococcal arthritis*, this joint being generally attacked in common with other joints; occasionally, but rarely, alone. (3) *Toxic arthritis* is quite common, commencing either insidiously, or following a chill or minor injury. A word of warning here may not be out of place. Inexperienced radiologists often report the presence of arthritis of this joint when none is present, basing their diagnosis on lipping of the joint margins at the lower end. This apparent lipping is due to a groove in which a small vessel runs and which is bridged by a ligament, described by Derry, of Cairo, and used by him as a sex indication in investigating remains from certain pre-dynastic cemeteries in Nubia, the groove being better marked

in the female, especially after many pregnancies or if congenital relaxation is present. However, skiagrams do show arthritis, which is generally of the atrophic variety. Frequently, of course, such arthritis is associated with similar disease elsewhere, and it must be remembered that arthritis may exist without causing bony change sufficient to be shown in a skiagram.

Undue laxity of the sacro-iliac joints is by no means uncommon. It may be present as a congenital abnormality which increases with repeated strain as life goes on, but the commonest cause is parturition. The fact of relaxation of these joints in the later months of pregnancy has been known to cowherds from time immemorial and has been recognized by obstetricians for years.

Restoration to normal stability takes place *pari passu* with the involution of the uterus. Should strain be thrown on the joints by premature re-assumption of the erect position after labour, this restoration may fail, and chronic pain result. It has been said that it is hard to demonstrate such undue mobility, and this is true, but its existence may be assumed by inference and therapeutic tests. Variation in the structure of the normal joint surfaces is great, and congenital abnormalities must play a large part in deciding whose sacro-iliac joint will be easily hurt, and whose will not. Other predisposing factors will obviously be the existence of a stiff hip or a stiff spine, an abnormal degree of strain being thus imposed on the sacro-iliac joints. Strain may be due to trauma or to habitual bad posture. In the latter case we shall notice such extraneous influences as flat foot, genu valgum, lordosis, &c. Nutter, of Montreal, points out that acute sacro-iliac strain takes place generally when the back is bent and the patient is stooping, torsion thus occurring in a direction to force the upper end of the sacrum back on the ilium. This is probably true for acute strains, but in chronic strains the sacrum tends to slip either directly downwards or downwards and forwards. This statement may appear unduly dogmatic, but my view is based on the apparent movement of the parts when reposition is attempted.

Finally, there is the question of treatment. In severe arthritis, tuberculous or otherwise, prolonged immobilization will be needed, with or without some form of internal splinting. It is obviously necessary to immobilize the joints above and below the lumbar spine and the hip. This can, of course, be effected by a Thomas's frame or a plaster bed. It will be interesting to hear the experience of others on the subject of internal fixation. In the case of tubercle the classical procedures consist of (a) open erosion or partial resection of the joint, and (b) Smith Petersen's fixation by means of a countersunk block of ilium. I have no experience of either. I have attempted fixation by sliding a graft from the ilium across the joint like a match-box opening and I have placed a tibial graft across the joint. In these methods the joint is opened, and possibly an abscess.

Some months back I showed at this Section¹ a man upon whom I had operated at the suggestion of my assistant, Mr. Fleming, by introducing a tibial graft across the sacrum under the erector spinæ muscles from one posterior superior spine to the other. I have also done this operation for chronic intractable strain. One patient in whose case I have carried out this operation is here to-night. The results I think are good, the principle of the operation being the insertion of a tie-beam, after the fashion of the iron bars inserted into old houses, with the purpose of resisting strain in exactly the place it occurs, also the avoidance of opening the joint and possibly an abscess.

Manipulative surgery has a true place in the treatment of strains and subluxations. Where the sacral displacement is backwards, full flexion of the spine will often reduce it, and again this can be done (after Nutter's method) by placing the patient in the prone position and forcibly lifting the ilium on the affected side by means of the hyperextended thigh, while counter-pressure is applied over the upper end of

¹ *Proceedings, 1923-24, xvii (Sect. Orth.), p. 12.*

the sacrum with the other hand. This method has the added advantage that by altering the counter-pressure the ilium can be rotated forwards on the sacrum in cases where the reverse displacement is present. In the few cases in which I have done this the perceptible and audible click in the joint has been accompanied by movement in this latter direction. Nutter uses plaster fixation after reposition, but says that his patients complain vigorously. Rest in bed with lumbar support is generally adequate. Temporary or permanent support for the joint by means of apparatus is not always easy to contrive. Braces may be used with a pad to press upon and support the affected joint.

In the case of women, at any rate those with tender and relaxed joints, there is nothing so good as an elastic support, somewhat similar to those appliances now used to reduce the hips at the bid of fashion, but made of porous, elastic material like an elastic stocking. The best of these are of French make.

Dr. JAMES MENNELL

expressed his regret that this subject was being raised in the Section of Orthopaedics rather than in the Section of Neurology, or even in the Section of Medicine, as the majority of sufferers from this complaint rarely consulted anyone interested in orthopaedics.

After relating personal experiences, both in the United States and at home, he said he had come to the conclusion that in the treatment of sacro-iliac strain there was place both for manipulation and for support.

He showed a series of supports, the first being typical of the usual American support; and, a second, one which had been presented to him at the Mayo Clinic. Experience had led him to believe that these patterns missed a most important point—namely, prolongation of the abdominal plate in the form of two bars which fitted under the anterior superior spines.

He showed the various types of back-plate used when forward or backward pressure was required, and he explained how they could be adjusted to a corset. A third pattern was shown for the use of those who were too stout to wear an ordinary abdominal plate. The length of the back-plate depended entirely on the condition of the lumbar spine. When lordosis was a possible cause of the continuation of the strain, the back-plate had to be elongated so as to rest above on the highest part of the thoracic convexity, while, below, all patterns finished two fingers' breadth above the tip of the coccyx.

With regard to diagnosis, he called attention to John Baer's sacro-iliac point, which is situated on a line from the umbilicus to the anterior superior spine, 2 in. down from the former point. He contrasted this with McBurney's point, and drew the moral that it was possible to explain on these lines, persistence of pain in the right iliac fossa after an appendix had been removed.

Insufficient attention was often paid to the position of patients in bed during prolonged illness and during anaesthesia. He hoped that the condition would soon be more generally recognized, and therefore more generally treated on correct lines.

Dr. EDGAR F. CYRIAX.

The sacro-iliac joint is a potential joint, inasmuch as it has a synovial membrane and an articular cartilage, and though it possesses no normal movements, it can readily assume abnormal ones, and subluxations result. These are usually of the nature of mal-rotations. These subluxations are much commoner than is generally supposed. They are often accompanied by a compensatory twist in the fifth lumbar vertebra, and these tend to react disadvantageously on one another.

As regards the symptoms enumerated by Mr. Verrall, I should like to add the following: (1) alterations in the relative positions of the anterior and posterior superior spines; (2) increase of the pain due to attempts to move the bone as if to increase the displacement; (3) decrease of the pain due to the attempt to move the bone in the opposite direction.

Skiagrams often show a splayed joint and bony irregularities, but radiographers usually dismiss these as having no significance, from the fact that "nobody is quite symmetrical."

Subluxations of the ilia are often, in my opinion, the starting point of tubercle and arthritis, and with a large proportion of sacro-iliac strains subluxation is associated. Many medical men absolutely deny the possibility of sacro-iliac subluxations. And many of those who do admit them recommend rest in bed, strapping, splintage, ionization—in short, everything excepting the one radical cure, namely, reposition. The reason for this is not clear. I have replaced about 1,500 subluxated ilia at periods varying from a few hours to about thirty-five years after the onset of the injury. Failures have been very few; recurrences still rarer. The method of reposition is simple and perfectly painless; children often appear amused by it. An anæsthetic is never used because there has never been the slightest indication for its use. The after-treatment is simple; sometimes none at all is needed, as in a case published in the *British Journal of Radiology*, 1924 (p. 135). At other times I employ mechano-therapeutics. I have never used any bandage or fixation in the after-treatment.

MR. PAUL BERNARD ROTH

said that *sacro-iliac strain* (or "painful sacro-iliac joint") had interested him for many years, and he could rejoice with Dr. Mennell that its existence was at last becoming recognized. Members might care to refer to his (Mr. Roth's) paper, published in the *Proceedings*¹ in 1913. He could not agree with the opener's description of this condition: he himself held that before a diagnosis of sacro-iliac strain could be made, three things were needful: (1) a history of a sudden strain; (2) pain referred to the back of one or other joint; (3) deep tenderness over the back of this joint. How the patient stood or how he walked was immaterial.

Treatment of the condition consisted in manipulation, and then support. Manipulation consisted in forcibly hyperextending the thigh on the trunk, with the patient lying prone: if the strain were due to a displacement of the joint surfaces, this manœuvre caused them to go into place with a click, with instant relief of pain. After manipulation his rule was to apply a 2 in. wide band of strapping tightly round the pelvis, immediately below the anterior superior spinous processes, the two ends overlapping each other behind for at least 6 in. This supported the pelvis, prevented any further slipping, and allowed the traumatic inflammation to subside. If care were taken, this could be worn for two or three weeks, and at the end of this time the patient was cured. He regarded sacro-iliac strain as a perfectly clear clinical entity, not to be confused with any other condition.

MR. R. C. ELMSLIE.

Mr. Verrall has taken great care in going over the symptoms and diagnosis of sacro-iliac conditions, but I think that his opening remarks and the remarks which have followed only make it additionally clear that there is still a great lack of knowledge about the diagnosis, and great difference of opinion as to what is and what is not sacro-iliac defect. Mr. Verrall says that he requires all the symptoms and signs to be present before he will diagnose sacro-iliac strain. When someone of Mr. Roth's experience records a paper which he wrote on the experience of nine cases, and when Dr. Cyriax talks of the 1,500 cases he has treated, it becomes quite obvious that there is a difference of standard as to what does constitute a sacro-iliac strain or displacement, and until we can get a greater uniformity of view on this matter, it is difficult to see how progress can be made. We all have to deal with many of these cases of pain in the lower part of the back. The actual location of the pain varies greatly with different people, and, personally, I feel that the practitioner who has a tendency to diagnose sacro-iliac strain will often find that the tender spot is over the sacro-iliac joint, whereas someone else who is prejudiced in favour of a muscular cause of the trouble will make out that it is situated in one of the muscles.

¹ *Proceedings*, 1913-14, vii (Sect. Surg., Sub-sect. Orth.), p. 54.

24 Elmslie—Cyriax—Fairbank: *Affections of the Sacro-iliac Joints*

We are all aware that many of the cases are relieved by manipulation and exercising, and that others are relieved by support; the difficulty is to diagnose what is the exact cause in any case and to be sure which treatment is going to be adequate for it.

Mr. Verrall has omitted to mention one part of the deformity of a severe sacro-iliac condition, i.e., a flattening of the lumbar spine and sacrum, so that the latter assumes almost a vertical position. This, and the sacro-iliac tilt, are of course not confined to cases of trouble in the sacro-iliac joint but occur also in lumbo-sacral disease and in anything involving the lumbo-sacral cord. If a mere strain thrown on the sacro-iliac joint is productive of pain, is it not remarkable that cases of low lumbar scoliosis are practically never associated with any sort of sacro-iliac pain?

Dr. CYRIAX

said he had given a wrong impression and did not mean that he had treated 1,500 cases of sacro-iliac strain by reposition of the ilium, but that he had treated 1,500 cases of displacement by reposition. A large number of these displacements had no subjective symptoms but only objective ones of spinal curvature and asymmetry. The proof of having effected reduction was chiefly based upon the removal of the abnormalities in the position of the anterior and posterior superior spines.

Mr. H. A. T. FAIRBANK (President)

said there was no doubt that another discussion on this subject at a later date was indicated. There were many further points that were worthy of discussion. For instance, there were occasional cases met with that required excision of the joint, e.g., those with sinuses and a mixed infection. What was the best method of exposure of the joint? Should the posterior portion of the ilium be removed *in toto* or should the posterior part of the crest and the posterior superior spine be left intact and the bone removed in front of it? He said he agreed with Mr. Verrall as to the advisability of having an undoubted combination of signs before diagnosis of sacro-iliac strain or disease. Compression of the ilia often caused no pain, but rather relieved it. The essential of any apparatus must be the fixation of the ilia. In tuberculous disease he doubted whether abscess was more common on the anterior aspect of the joint as was often stated. In his rather limited experience abscess was only too common posteriorly, and this interfered with a bone-grafting operation. As to the differential diagnosis he emphasized the great importance of good radiograms, and mentioned a case with great pain and tenderness at the back of the sacro-iliac region on one side, where X-rays showed an abnormal joint between a large transverse process of the fifth lumbar and the ala of the sacrum, with marked osteo-arthritic changes in this joint. He was very interested in Dr. Mennell's exhibition of belts and asked for more information as to when the pad reaching higher was used, and when the ordinary sacral pad. He had only seen one or two cases of strain which seemed to him to call for operative treatment. Mr. Verrall's method of bone-grafting was undoubtedly an improvement on Smith Petersen's method.

Mr. JENNER VERRALL (in reply)

emphasized that it was essential to try both lateral iliac and pubic pressure as the effects of these were diametrically opposite. He said he agreed that belts were often the only possible treatment, as patients would not submit to operation. In answer to Dr. Mennell he would suggest that all patients, irrespective of their disease, who were compelled to spend a long time sitting up in bed should have a support for the lumbar spine. Difference of level of the posterior spines could certainly be noticed on occasions. Mr. Roth had advocated a band of strapping round the pelvis after reposition. This was his usual practice but he had omitted to mention it. Mr. Elmslie's sound observation on the flat sacrum in these cases he admitted, and he said he felt that it tended to support Nutter's view of the forward movement of ilium on the sacrum.

Section of Orthopædics.

President—Mr. H. A. T. FAIRBANK, D.S.O., M.S.

Two Cases of Scoliosis with Paraplegia.

By R. C. ELMSLIE, O.B.E., M.S.

Case 1.—D. B., male, aged 18. Normal until 7, then infantile paralysis of left lower limb. Scoliosis noticed one year later. November, 1922, aged 16, legs became weak, right first. Last walked February, 1923. No pain at any time and no affection of sphincters. P. C.—Very severe left dorsal convexity with its centre at D.5; keel-



Skiagram with lipiodol showing obstruction at level of fourth dorsal vertebra.

shaped projection of left ribs posteriorly. X-ray showed simple scoliosis with much deformity of individual vertebræ and rotation through right angle at maximum point of curve. Anæsthesia to cotton wool up to 2 in. below xiphisternum; hyperalgesia

to pin prick over lower half of the abdomen; analgesia from 1 in. above groin downwards; joint-sense absent at toe and ankle; abdominal muscles paralysed; complete paralysis of lower limbs with spasticity; knees extended; feet, plantar flexed; abdominal reflexes absent, upper and lower; knee-jerks + +; double ankle clonus; plantar response extensor; sphincters normal. By September 19 the anaesthesia had increased and was complete up to the fifth dorsal segment. September 24, 1923: Lipuol injection into cisterna magna, carried out by Sir James Purves-Stewart, and passed down as far as the fourth dorsal vertebra; injected by lumbar puncture it passed up as far as the sixth dorsal vertebra.

October 10, 1923, Laminectomy. Laminae and spines of the third, fourth, fifth and sixth dorsal vertebrae were removed. Posterior surface of these bodies could be clearly seen owing to the extreme rotation and to the fact that spinal cord and theca were tightly stretched along the concavity of the curve, leaving greater part of vertebral canal vacant. Theca incised; cord appeared normal. It was impossible to re-suture theca owing to the extreme longitudinal tension upon it. The muscles and skin were sutured without replacement of bone. There was some shock and temporary retention of urine after operation, but the latter abated in twenty-four hours.

During the next two months there was no improvement in the paralysis. December 5, 1923: A further operation was done with the intention of attempting to displace the cord out of the canal into the tissues. Owing to the amount of fibrous tissue encountered in the region left by the removal of the laminae, it was decided that the continuance of the operation was too risky, and the wound was closed.

January 5, 1924: Suspension by head and arms commenced and continued for an increased time each day until a maximum of six hours out of the twenty-four was reached.

February 16, 1924: Sensation to cotton wool down to D.10 on right, D.9 on left. Very slight voluntary power in flexors of toes and quadriceps. Gradual improvement of sensation, until by June 12, 1924, tactile sensation had become normal. Joint-sense still deficient in lower limbs. By this time there was feeble movement of flexion and extension of toes in right lower limb, and movement of toes and of quadriceps in left lower limb. Voluntary movement has gradually improved since. September 9, 1924: Subcutaneous lengthening of tendo Achillis, right and left, and application of plasters.

At the present time there are small voluntary movements of nearly all muscles in both lower limbs. There is still a good deal of spasticity, but much less than formerly. A spinal support has been fitted, and the patient is beginning to make efforts to learn to walk.

Case II.—A. R., male, aged 12. Said to have been normal at birth; weight, 9 lb. First noticed to be suffering from scoliosis when examined by school doctor at age of about 6 or 7 years. Seen at that time at St. Bartholomew's; diagnosis, congenital scoliosis; treated by exercises. About March, 1924, had influenza; from that time was never well and began to complain of tiredness and giddiness. Began to drag left foot during summer and used to catch the foot and fall. In August, 1924, tripped in a rabbit-hole and fell; patient thinks this the cause of the more rapid progress of the paralysis, which has been swiftly progressive during the autumn. Retention of urine for last two months. P. C.—Scoliosis—very severe curve to right, extending from C.7 to D.4; below this a more gradual curvature extending right down to lower dorsal region. Much rotation of vertebrae at apex of curve, which is situated about D.2 and 3. Complete motor paralysis of abdominal muscles and whole of lower limbs, with spasm. Hips and knees flexed; feet in equinus position with extended toes; abdominal reflexes absent; knee-jerks +; ankle clonus right and left; plantar response extensor; sensation natural. X-ray examination January 31, 1925. Lipuol injected into cisterna magna by Sir James Purves-

Stewart; this was seen in the X-rays to have passed down as far as the body of the first dorsal vertebra. Wedge-shaped vertebra on right below D.3. No treatment has so far been carried out.

In his original discussion on spinal caries, Pott called attention to the fact that when paraplegia arose in spinal curvature the curvature was always antero-posterior and not lateral. As far as I have been able to trace in the literature, no case has been recorded of paraplegia resulting from a scoliotic curve. In the first of the above cases there appears to be a proof, which is as perfect as can be obtained, that the paraplegia was due to the scoliosis. In the second case, in the absence of an exploratory operation, the proof is less perfect, but in my opinion there is a very strong presumption that again the paraplegia is definitely due to the scoliosis.

A Case of Unilateral Imperfect Formation of the Hip-joint: Subluxation, with Spontaneous Recovery.

By H. A. T. FAIRBANK, D.S.O., M.S. (President).

E. S., GIRL, aged 2; first seen November, 1922, when three weeks old, as mother thought, child cried when right hip was moved. Nothing abnormal was found. One month later there was some doubt as to stability of both hips. X-ray showed upper end of right femur displaced slightly outwards and upwards; head centre not



FIG. 1.—E. S., radiogram taken when seven weeks old (December, 1922), showing displacement upwards and outwards of right femur.

yet visible. Six months later there was no doubt by clinical examination that head of femur was gradually becoming dislocated. Another six months later this was not so definite, though an X-ray (December, 1923) showed poorly developed upper lip to right acetabulum, head of femur being displaced upwards and outwards; head centre larger than that on left.

28 Fairbank: *Imperfect Formation of Hip-joint ; Multiple Exostoses*

June, 1924: right leg discovered to be longer, rather than shorter, than left; some limitation of abduction; head of femur did not feel definitely up, but seemed to be enlarged outwards.

Present Condition.—No limp and no complaint of any kind; movements of the joint free; right leg about $\frac{1}{2}$ in. longer than left. X-rays show femoral head to be distinctly larger and neck thicker than that of other side, but there is no suggestion of subluxation of the joint; acetabulum still poorly developed as before.

The apparent displacement seen in the earlier X-rays might in part be due to limb being rotated out when photographs were taken, and partly due to some laxity



FIG. 2.—E. S. Radiogram taken when two years old (October, 1924), showing enlargement of head of right femur and imperfect formation of acetabulum.

of capsule. They certainly suggest early stages of congenital dislocation. Capsular laxity and bone deformity might be due to acute arthritis beginning soon after birth, but there was no sign of this when seen at age of three weeks, and no umbilical infection to account for it. Case regarded as one of congenital deformity which renders stability of the joint uncertain. It is proposed to watch the case and have radiograms taken from time to time.

Case of Multiple Exostoses, with Unusual Osteoma.

By H. A. T. FAIRBANK, D.S.O., M.S. (President).

J. B. G., MALE, aged 18. First seen September, 1924, for multiple, bony tumours. First tumour noted at age of twelve months.

On examination patient showed typical appearance of diaphyseal aclasis, though his stature was not markedly below the average. Enlargement of the diaphyseal ends with exostoses was present in all the usual situations including the scapulae and clavicles. Right ulna and both fibulae were abnormally short. The pelvis (especially the pubes) was affected, while metacarpals and phalanges in the hands and metatarsus and phalanges in the feet also showed changes here and there. Os calcis in

each foot altered in outline. Enlargement of both calves enormous; these bony swellings of the tibiæ and fibulæ, and one exostosis growing from the lower end of the right femur alone seemed to call for removal.

At operation the tumours in calves were found to consist of very open cancellous bone, showing here and there patches of cartilage, calcified cartilage and gelatinous material, quite unlike the usual appearances seen on sectioning a cancellous exostosis.

Specimen of the bone removed and a microscopic section of the specimen are shown.

Case of Osteogenesis Imperfecta.

By E. LAMING EVANS, C.B.E., F.R.C.S.

A. B., FEMALE, aged 2, was deformed at birth. Her father's cousin is a dwarf, but has not suffered from fractures. No other history of deformity in family.

Case presents following points of interest: Anterior fontanelle is widely open; sutures between two halves of frontal, and between frontal and parietals on both sides, are open for three-quarters of an inch; that between the two parietals is closed; fontanelle is thus triangular rather than diamond-shaped. Forehead prominent, and skull shortened antero-posteriorly. Marked increase in bitemporal diameter; in consequence the auricles are pointed downwards and a little backwards, and give appearance of being set low on cranium. Occipito-frontal circumference 19 in.; bitemporal diameter $5\frac{1}{2}$ in.; occipito-frontal diameter 6 in. Teeth yellow and translucent; in upper jaw four incisors and two premolars are present, whilst in lower jaw the four incisors only are erupted. Sclerotics show a slight blue discoloration.

Patient's mentality poor; she cannot talk yet; her disposition is placid. Chest presents a wide infrasternal angle; axillary circumference 17 in. Ribs present some nodules of callus; costosternal junction not beaded. Abdomen very prominent, liver and spleen enlarged, circumference $20\frac{1}{2}$ in. The recti divaricate. Spine shows a slight dorso-lumbar kyphosis.

Patient's length, from vertex to sole of foot, is 23 in. Extremities all misshapen. Left humerus bowed with convexity forward and outward, and middle third of shaft has been fractured. Left radius and ulna have been fractured, with a resulting convexity along the ulnar border of the forearm.

Similar lesions have occurred in right upper extremity with the resulting deformities. Both femora present forward and outward bowings. Most marked deformities are in tibia and fibula, where anterior convexity is pronounced. Cutaneous dimples are present over these convexities.

X-ray examination shows signs of several fractures, but there is no evidence of any rachitic changes. Structure of the bones is markedly abnormal, the appearance being that of extreme rarefaction.

Wassermann reaction negative.

Mr. LAWFORD KNAGGS, M.Ch., showed a number of microscopical sections and lantern slides illustrating the Pathology of Osteogenesis Imperfecta.

Osteitis Deformans at an Unusually Early Age.

By PAUL BERNARD ROTH, F.R.C.S.

PERCY S., aged 30, seen at hospital on January 8, 1925. He stated the shin-bone had been getting bigger and more curved for twelve years or longer, and had recently begun to ache occasionally.

On examination the right tibia is enormously enlarged, and curved with marked convexity forwards. The skin over it is dusky-looking, with prominent veins.

30 Perkins : *Old Fracture of Pelvis ; Arthritis of both Hip-joints*

Circumference of right leg is two inches greater than that of left. Appearance is typically that of osteitis deformans ; radiograms confirm this, as they show tremendous overgrowth of compact bone with obliteration of the medullary space.

Wassermann negative.

No other bones appear to be affected.

Old Fracture of the Pelvis.

By G. PERKINS, M.Ch.

BOY, aged 13 ; was run over at the age of 6. In bed three weeks and then commenced to walk, but as this was painful he reported at hospital and was treated "in irons for two years as a tuberculous hip."

Radiogram shows $1\frac{1}{2}$ in. separation between pubic bones in front, right pubic bone being displaced outwards and upwards. There seems to have been a fracture through the right sacro-iliac joint, and possibly also one through right pubic bone.

Case is shown to demonstrate the excellent function following severe pelvic injury. There is full range of movement at hip-joint ; the only disability is one inch of shortening of right limb due to upward displacement of pelvis on that side.

Arthritis of both Hip-joints following Epiphyseal Displacement.

By G. PERKINS, M.Ch.

MAN, aged 30 ; fell off pony at age of 14. This accident was followed by severe pain in both hips. Three weeks after injury was admitted to hospital ; adductor muscles on both sides were tenotomized and weight traction was applied to legs for six months. On discharge he used crutches for three months ; pain then slowly disappeared and he returned to work in a forge for six years.

For past two years he has had increasing pain and stiffness in both hip-joints. There is now severe bilateral arthritis, and only a few degrees of movement are possible at either hip-joint.

Radiogram suggests that injury was a bilateral displacement of the femoral epiphysis ; on left the head of femur has slipped down and back, and has united to neck ; on right the head of femur appears to have disappeared. The joint spaces are much diminished.

Case shown in order to obtain the opinion of Members on nature of primary injury.

Section of Orthopædics.

President—Mr. H. A. T. FAIRBANK, D.S.O., M.S.

DISCUSSION ON THE DIFFERENTIAL DIAGNOSIS OF NON-TUBERCULOUS COXITIS IN CHILDREN AND ADOLESCENTS.

Mr. ALAN H. TODD.

NOT many years ago, practically all painful affections of the hip-joint occurring in young patients were tacitly assumed to be tuberculous. It gradually became obvious from the course of the disease in some of these cases that they could not be tuberculous, and with closer clinical study, and especially radiological study, certain well-defined conditions have now been clearly eliminated from the group. I allude, in particular, to coxa vara in its various forms, to Perthes' disease, and to cystic conditions in the upper end of the femur.

There remains, however, a fairly large class in which differential diagnosis is still badly needed. When the London County Council brought into operation its scheme for sending tuberculous children to Carshalton and other large centres for treatment, the method of quarantining the patients in infirmaries was introduced in the hope of minimizing the introduction of infection into the country hospitals. This step had the effect of drawing one's attention to the difficulty of the early diagnosis of hip-diseases. It is a fairly common experience to see a child at the out-patient department complaining at what appears at first sight to be a fairly typical early tuberculous condition, to watch that child for a week or two, to X-ray him, to decide finally that it is probably a case of tuberculosis, and to send him to an infirmary "with a view to transfer;" then, after perhaps five or six weeks, to get a polite note from the infirmary superintendent saying that, as all symptoms have disappeared, the child has been discharged.

Of course, if these cases were not watched, it might well be argued that probably many of them were tuberculous after all; that they had merely been temporarily patched up by the period of rest in bed, and that after discharge they would light up again, and be lost sight of, with disastrous results. Years ago, in his excellent book, "Diseases of the Joints and Spine," Professor Howard Marsh emphasized the great importance of activating cases of suspected joint-tuberculosis, and not splinting them; he pointed out that if such patients were splinted, the symptoms would abate, and the case would be regarded as one of some trifling, transient affection; whereas, if the patients were made to go on running about for a fortnight, the diagnosis would be rendered obvious without risk. But when these rejected cases from the infirmaries are watched again at the out-patient departments from which they first came, they are found, for the most part, to be well and to remain well, which shows that they were never tuberculous at all.

According to my experience in various hospitals, about 10 per cent. of the cases that are sent up by outside doctors as possible examples of early tuberculosis are non-tuberculous, whilst in private practice the percentage is considerably higher.

It is to these undifferentiated non-tuberculous cases that I want to direct attention. I do not pretend that I am able to distinguish them myself; but I hope that by combined research we may get to know a great deal more about them.

Classification.

We may divide them into five main groups, viz.: (a) Traumatic cases; (b) Coxa vara; (c) Infective cases; (d) Coxitis of secondary onset; (e) Arthritis deformans juvenilis as distinct from Perthes' disease; (f) Miscellaneous cases.

(a) *Traumatic Cases:* (1) *Simple Transient Coxitis.*—It is a common event for a child to be brought to hospital, having had a fall a few days previously, and complaining of pain in the hip on movement; there is a limp, with avoidance of weight-bearing and limitation of movement in various directions. Some get well spontaneously, and no doubt the diagnosis of "contusion of the hip" or "synovitis" of the hip would be correct.

(2) *Acute Traumatic Chondritis*.—Others, however, continue to complain of a dull aching, in addition to the more acute pain on movement. Sometimes there is acute pain at one particular stage of one particular movement. Speaking from the analogy of cases with similar symptoms occurring in the knee and other large joints, where one has had an opportunity of verifying the diagnosis by exploration, I believe that these are cases in which a patch of articular cartilage has been bruised and acutely inflamed; one sees it softened, reddened and swollen. Such cases of acute traumatic chondritis get well after a few days' rest in bed, without splintage.

In other cases the symptoms clear up after a few days, with or without treatment, but recur after a few days of running about again; sometimes they recur more than once, and after a time slight muscular wasting begins to appear. One then becomes anxious as to the diagnosis, which may be very difficult. I shall have more to say on this subject a little later, but I may say now that I do not remember ever having seen a case of tuberculosis of the hip that became obvious within six weeks of a local injury, or arising as the result of a really severe blow upon the hip. When a case has been sent to hospital with a diagnosis of early tuberculosis following upon a blow which occurred, say, three weeks previously, it has invariably, I think, proved to be non-tuberculous in the long run. The injury that precedes tuberculosis is a mild injury, and there is always a fairly lengthy latent period between the trauma and the development of a recognizable disability.

(3) *Juxta-Epiphyseal Strain*.—Some of these cases of physical injury result in juxta-epiphyseal strain: rarely, the upper epiphysis is separated; that condition is easily demonstrated with the aid of X-rays, and we need not consider it in detail. If there is any suspicion that the juxta-epiphyseal region has been strained, the patient must be kept under observation and periodically X-rayed for some months afterwards, as a coxa vara may develop—slowly, as a rule, but occasionally rapidly.

Another class of juxta-epiphyseal strain is that in which there is no actual trauma, beyond the mere body-weight, but the bone is unduly weak owing to the presence of some nutritional disorder, rickets, or some endocrine disorder, whilst other cases are purely static, e.g., cases of excessive obesity in which there may be aching of the hips analogous to that which occurs in any other mechanically-overloaded part of the body.

(b) *Coxa Vara*.—This condition is well recognized, and can always be distinguished radiographically. I need not say much, therefore, upon this point. Early cases, however, are sometimes sent to hospital with a diagnosis of "tuberculous hip"—confusion arising owing to the limp, pain and shortening. Sometimes the pain occurs in mild, recurrent attacks; there was a discussion about cases of this type at the Congress of Surgery in 1912. It is convenient to remember that in quite an early stage of coxa vara abduction only is limited, whereas in tuberculosis there is limitation in more than one direction. But by the time that other limitations have appeared, in coxa vara X-ray examination makes the diagnosis obvious.

(c) *Infective Cases*.—Some of these directly follow febrile conditions, notably pneumococcal lesions, measles, scarlet fever, typhoid, influenza, and gonorrhœal vulvo-vaginitis. Others follow various surgical conditions, notably otitis media, tonsillitis, and operations upon the tonsils, and appendicular abscess.

We know so little of the bacteriology of some of these conditions that it is difficult to say whether the coxitis is specific or not. But at any rate it seems probable that some of them are microbial, and others simply toxic. The transient arthralgia that occurs after many infections, e.g., ordinary influenza, is probably of this nature.

There is a considerable difference between the various antecedent diseases as regards the virulence of the arthritis which they produce. Scarlet fever causes coxitis rarely, but as a rule, in my experience, it is a severe infection when it occurs; suppuration is not uncommon, and ankylosis, with or without suppuration, is fairly frequent.

Measles, on the other hand, is followed by coxitis fairly often, and generally the arthritis is of a mild order. I have always fixed these patients with a splint, usually a double plaster hip-spica, and in most cases the arthritis has completely subsided within two months; a little after-treatment speedily restores full function. In a few cases, however, the symptoms persist, and the case seems to merge insensibly into one of classical and unmistakable tuberculosis; I take it that these are examples, not of coxitis caused by the virus of measles, but of tuberculosis ensuing upon the debility that the measles produced.

There is, however, I believe, a true form of coxitis following measles that runs a severe course, but is not tuberculous.

D. B. had measles when aged 6; she developed double coxitis within six weeks of the onset, and was placed first of all in a double Thomas's splint. The pain continued, however, and was severe, so she was fixed, instead, in a double plaster spica. The pain then subsided gradually. X-ray examination at this stage revealed no bony abnormality. She went away to convalesce, but was brought back hurriedly, as abscesses had developed on both sides. Still the radiograms were negative. Fixation was continued. I now lost sight of her for a couple of years, and then she came back again with the hip-movement limited to about 30° of flexion on either side, and with an absence of all inward or outward rotation. There was a curious but very marked deformity in each hip-joint; the heads of the femora were wholly gone, and the upper aspects of the necks were roughened and worn down. Further, there was a marked upward displacement of each femur. The acute stage of the illness had passed, though movement was limited, it was painless, and the child is now able to walk a little.

Perhaps this case is one of those that Broca describes as "dry arthritis." He mentions, in an article in *La Presse médicale* for 1922,¹ that in some cases of coxitis, "not necessarily microbic," marked flattening of the femoral head may occur. Froelich, again, speaks of a non-tuberculous coxitis sicca, and says that in some cases there is a marked progressive luxation upwards, much as we see it in the case just mentioned. He also describes a caries sicca, non-tuberculous in origin, in which slow excavation of the acetabulum is a feature; I have not recognized a case of this condition myself. Broca mentions, in his article, that coxitis following measles is much more likely to result in tuberculosis than is coxitis from scarlet fever.

Typoidal and post-influenzal cases occur in all grades of severity, ranging from the mildest arthralgia up to fulminating states. If the cartilage is ulcerated through, ankylosis inevitably results. Spontaneous dislocation may occur in any of the more severe forms.

Gonorrhœal coxitis, secondary to vulvo-vaginitis, is usually very mild in type, but very persistent: eventually it resolves.

The pneumococcal cases are commonly divided into the so-called "primary" cases, and the secondary; but as all must be due to blood-borne infection, this distinction cannot be of great value. However, it serves to remind us of the marked difference in prognosis that exists between those cases in which the arthritis is the first recognizable lesion, and those in which it follows some other pneumococcal affection. It is stated that about 65 per cent. of the secondary cases are fatal, but of the primary cases only 25 per cent. In children, 60 per cent. of cases are of the "primary" type, and according to my hospital figures, 30 per cent. occur in children under fourteen years. As a rule, there is acute pain, but swelling is very slight, whilst high fever and acute mania are common. The teeth, throat, conjunctiva, and middle ear seem to be the most important foci of infectivity in the secondary cases.

(d) *Coxitis of Secondary Onset*.—In coxitis secondary to operations in septic areas the diagnosis is always easy, for the infection is invariably very acute. One or two cases have followed operation for appendicular abscess, but most have followed removal of the tonsils; in several cases death has occurred, and in many others pathological dislocation of the hip.

Coxitis secondary to acute or subacute epiphysitis occurs almost exclusively in quite young patients, chiefly children of about a year old. The onset is sudden, the course rapid; there is marked hyperpyrexia, and prostration, and in a very few days gross changes take place in the radiogram. This fact, and the presence of great leucocytosis, serve to make the distinction from tuberculosis; otherwise, differentiation is not easy, for at this early age tuberculous coxitis also is liable to be a very rapid and fatal disease. The upper epiphysis becomes rapidly destroyed, and more or less ankylosis is the rule. Some of these cases follow an exanthem, some are secondary to sepsis elsewhere, whilst others seem to be "primary." As a point in the differential diagnosis, I may add that coxitis in an infant of under one year, in the absence of other signs of tuberculosis, is more likely to be due to a non-tuberculous cause than to tuberculosis.

(e) *Arthritis Deformans Juvenilis*.—This is, I believe, a clinical, though possibly not a pathological, entity. In the past, this term has doubtless been applied to cases of Perthes' disease, coxa vara, rheumatoid arthritis, and the like; but I mean by it a condition distinct from all these, having definite and constant characteristics of its own. It is a condition which,

¹ "Arthrites non-tuberculeuses de la hanche," *Presse médicale*, 1922, xxx, p. 941.

34 Todd: *Differential Diagnosis of Non-tuberculous Coxitis*

as far as I know, affects males only, almost always youths of about seventeen years of age; it is insidious in onset, slow in progress, painful, and leading inevitably to disablement and ankylosis. There is gradual destruction, by caries, first of the articular cartilage of the head of the femur, then of the weight-supporting part of the neck, whilst new bone forms abundantly at the periphery of the joint. The X-ray appearance is as a rule characteristic, and not quite like that of any other condition. No other joint is involved, and when once the damage is done, the disease has expended its force, and there are no recrudescences.

The distinctions between arthritis deformans juvenilis and ordinary osteo-chondritis may be tabulated as follows:—

<i>Arthritis deformans juvenilis.</i>	<i>Osteo-chondritis.</i>
Rare.	Common.
Affects adolescents.	Affects children.
X-ray appearances are those of cartilage-destruction, with peripheral bone-formation.	X-ray changes are well known, and quite different.
Course slow, leading to ankylosis.	Course comparatively rapid, ending in restoration of function.

I need say nothing of true rheumatism, of coxitis in Still's disease, or of Perthes' disease, for these conditions are perfectly familiar. Still's disease presents the manifestations of rheumatoid polyarthritis, Perthes' disease a very typical cycle of X-ray changes.

(f) *Miscellaneous Cases.*—There remain a few odd forms of coxitis to be mentioned for the sake of completeness; the diagnosis is usually easy.

(1) *Hæmophilia* leads to recurrent attacks of pain, and limitation of movement, subsiding easily with a few weeks' rest in bed. The typical history points to the diagnosis.

(2) In *scurvy* there are the ordinary signs of the disease to help one; the diagnosis is confirmed by X-ray examination, and by the effect of treatment.

(3) *Intestinal pseudo-coxitis* is a curious condition, of which I have seen several examples. It is definitely recognized in French literature, though I do not think I have seen any description of it by English writers. It is something more than a mere intestinal pain, referred vaguely to the hip-region; it is a definite pain in the hip-joint, on movement, associated with a limp and limitation of movement in various directions, and occurring in a constipated child. The symptoms last about two or three weeks as a rule, and disappear under treatment with laxatives and a non-protein diet. The fact that reduction of protein does such marked good rather suggests that the disease may be some kind of a protein-sensitization.

(4) Finally, I may enumerate certain more remote conditions, which may need differential diagnosis, viz., lumbar caries, perinephritis, psoas myositis, syphilitic epiphysitis, hysteria, sacro-iliac affections, coxa valga, knee-affections and juxta-articular bursitis.

Diagnosis of Pseudo-tuberculous Coxitis.—Returning to this question, due regard will be paid, first of all to family history and to the history of the disease, and especially, as I have already said, to the rate of development of the symptoms in relation to the date of an injury, if any; the degree of muscular spasm and muscular wasting is also a guide in many cases.

A proper temperature chart will help. An ordinary morning and evening chart is of little use. It is necessary, first of all, to put the child to bed, and determine exactly its normal temperature at rest and the diurnal variations. Next, an exercise-test may be carried out; the child should be given, say, half an hour's exercise and then be put to bed. Exactly half an hour later the temperature should be taken again, and, possibly, after the lapse of a further half-hour; in this way, a slight rise resulting from the activity will not be overlooked. It is necessary to insist upon exactitude in recording these temperatures; the ordinary casual round of evening temperature-recording, in which a child's temperature may be taken at any odd time within two hours or so of going to bed, is worse than useless in this connexion.

Another very helpful point is the disproportionate rate of the pulse as compared with the temperature that is shown in all cases with active tuberculosis. This has been recognized for many years in phthisis and tuberculous meningitis, but it is seldom realized that the sign is a very constant one in surgical tuberculosis as well. After all, there is no reason why there should be any difference between the results of the circulation of tuberculous toxin derived from a surgical lesion and that produced by a medical lesion. If we study the charts of cases of tuberculous coxitis, we shall find that the pulse-rate is always high in proportion to the temperature. This does not occur in ordinary bacterial infections.

The injection of old tuberculin as a diagnostic measure is, I consider, dangerous.

The graduated von Pirquet's test is valuable if negative, as it definitely excludes tuberculosis; but a positive result is of no assistance.

Radiographic changes in the bone do not appear very early; clinical symptoms and signs always precede them. In my experience the first change is an increased transradiancy of the bones; this is followed fairly soon by a general haziness or blurring of the affected part, with some distension of the joint-cavity, and perhaps, subsequently, by a slight woolliness of the outline of the bone at one point. It is only later that a local erosion begins to appear, and this always presents an irregular, soft, fuzzy appearance that is quite characteristic. The interpretation of the radiogram of a suspected hip is open to several fallacies, and it is wise always to take the opposite hip, in exactly symmetrical position, for comparison. My practice is to have a full-size view of the whole pelvis, and two small radiograms, well stopped down, one of each femoral head. Even with this precaution much care is necessary; for example, some hips present an apparently transradiancy area, just to the inner side of the upper end of the calcar femorale; this is a well-known source of difficulty, but a mistake can be avoided by watching developments and by the demonstration of a similar condition on the unaffected side.

The most important test of all consists in the effect of moderate mobilization. If a tuberculous-hip patient is put to bed, the symptoms will subside, and the same will happen in a non-tuberculous case; unless observation is continued there is a danger that the diagnosis may be entirely missed. In doubtful cases, therefore, it is always wiser to make the patient run about for another week and then re-examine; I have never known this course followed by any serious exacerbation of the disease. If the case is tuberculous, it becomes sufficiently aggravated to be recognizable, at any rate after a fortnight, and then efficient treatment will speedily produce subsidence. But there is a difficulty in practice, for the state of some of the non-tuberculous patients does not clear up readily if they are kept running about. They get better for a time when rested, but relapse when allowed to get up, and perhaps relapse more than once. One plan, in this case, would be to treat them as tuberculous, with prolonged weight-extension; but this plan, though absolutely safe, wastes a certain amount of time and bed-accommodation, for a tuberculous case needs longer treatment for its complete cure than is required for a non-tuberculous case. I think that a quicker plan is simply to put the suspect to bed without any splint or weight-extension; a tuberculous case will never, as far as I know, get perfectly well with mere rest in bed; there may be temporary improvement, but sooner or later some sort of recrudescence of symptoms or signs will appear: possibly wasting will persist, or spasm, or slight pyrexia after activity. It is characteristic of tuberculosis, in my experience, that it demands complete immobilization for its cure; mere rest is no good. Wood splints are never efficacious in a case of active tuberculosis; but the application of a good plaster produces immediate improvement. It is just those few degrees of mobility that spoil everything in tuberculosis. Absolute fixation is the secret of successful treatment. This fact can be applied as a diagnostic principle; if we put a suspected hip-case to bed, but do not fix the patient, and he gets completely well, that case is not one of tuberculosis. This course is much more expeditious than that of treating all such cases as presumptively tuberculous patients.

In conclusion, let me appeal to you to institute a combined investigation of this difficult subject. Most of us have some vague ideas about it, but they need crystallization; we need to reduce them to definite form. If we were all to work at the question for a couple of years, and then take stock of our position afresh, we might find, I think, that considerable progress had been made.

Mr. R. C. ELSLIE said that various diseases such as coxa vara and coxa plana had emerged from the miscellaneous heap of hip affections. Both these were originally differentiated by clinical methods. Even tuberculous hips differed so enormously in their characteristics that the only way was to separate out groups gradually. He (Mr. Elmslie) referred to the known tendency of measles to be followed by tuberculosis and he also mentioned a case in which the fluid from a knee in what was apparently a case of rheumatic fever killed a guinea-pig with tubercle. He suggested that Mr. Todd should collect a series of certain definite types, to which the Section should attempt to make additions.

Dr. ROCYN JONES demonstrated two radiographs, and said that the first illustrated the difficulty of an absolute diagnosis even where the radiographs pointed definitely in one direction. It was the case of a boy, aged 8, admitted to hospital with a limp and complaining

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of pain in the right hip. A radiograph showed what one would regard as a typical coxa plana and yet the clinical signs and the fact that for some months his temperature ranged between 97° and 100°, with constant evening rises, strongly suggested tuberculosis. The patient was kept in bed with the limb abducted and weight extension applied, and nine months elapsed before free and painless movement became possible. It was somewhat difficult to say whether one should regard this case as primarily coxa plana with superimposed tuberculosis, or tuberculosis alone with an epiphyseal plate resembling that seen in coxa plana.

The other case was that of a girl, aged 6, who was admitted to a London infirmary with broncho-pneumonia. This was followed by bilateral coxitis with abscess formation, the pus being drained through small incisions. The child left the infirmary cured at the end of five months. He (Dr. Rocyn Jones) first saw her several months later. The right hip was dorsally dislocated and movements in all directions were very limited but quite painless. She compensated for the flexion by walking in equinus. The radiograph showed extensive destruction of the acetabulum, and, in the absence of the clinical history, this X-ray appearance would be strongly suggestive of tuberculosis. Both the patient's history and her rapid recovery from so extensive a lesion were against this. The disease should therefore be classified as an infective coxitis, probably pneumococcal in origin, since it appeared as a complication of broncho-pneumonia.

The diagnosis of early coxitis was often very difficult, but he had found one sign of great value, namely, the effect of rotation of the limb upon the patient. A child might tolerate flexion, extension, abduction and adduction quite well, but would resent rotation. This sign was often indicative of intra-articular mischief and was in evidence long before the radiograph showed any change. It was commonly present in tuberculosis but of course was not diagnostic of this disease.

Mr. H. A. T. FAIRBANK (President), referring to traumatic cases, said that it was sometimes forgotten that simple synovitis could affect the hip-joint. Many cases of limitation of movement were of this nature. In doubtful cases he was in favour of letting a child run about as a means of diagnosis, unless symptoms were too acute. With regard to Mr. Todd's description of the changes which gradually took place in tuberculous coxitis he presumed that he (Mr. Todd) was describing a primary synovial case. Primary bone disease had not been mentioned. There was a type of primary synovial disease which deserved notice, viz., that complicated by early dislocation of the joint. It was often remarkably painless. He had seen a Greek boy in the East who waved his legs about freely, in spite of both his hips being dislocated. There were two specimens of undoubted primary synovial disease of the hip-joint in the museum of the Children's Hospital, Great Ormond Street; in both specimens there were pathological dislocations without erosion of the bones; the patients in both cases died of tuberculous meningitis. In one case manipulation and reduction had been carried out. He had the radiograms of these two cases as well as of two others. The well developed lip of the acetabulum served to distinguish these pathological dislocations from congenital dislocations: the symptoms were not necessarily distinctive.

He was not certain what was meant by juxta-epiphyseal strain, as he believed that could not occur without displacement of the head of the femur. These cases were, therefore, those of early adolescent coxa vara, and their recognition was of the utmost importance. If unrecognized, greater displacement was certain to occur sooner or later.

As to endocrine disorders, he had noticed that a certain number of cases of adolescent coxa vara exhibited definite retardation of sexual development, a point to which Mr. Jenner Verrall had called his attention. The infective group was the most difficult. He had known bilateral ankylosis to occur, and yet the parents could give no history of the onset or early progress of the ankylosis. He agreed that epiphysitis was more likely to be met with in young children of about one year old. He had seen a punched-out hole in an otherwise normal femoral head in such a case. Pneumococcal arthritis provided the ideal cases for treatment by opening, washing out and closing the joint. The subacute and chronic non-suppurative cases he regarded as due to staphylococci of low virulence, but proof was not usually forthcoming. In a recent case, an adult, the hip had been opened by another surgeon and found dry: a culture yielded pure *Staphylococcus aureus*. The knee became affected and eventually pus was let out of this joint. Both joints showed marked erosion of bone. Still's disease was certainly worth remembering in bilateral arthritis of doubtful nature. The frequency of involvement of the cervical spine in this disease was worth remembering. Hemophilia affected the hip far less often than the knee or elbow. He had never seen scurvy of the hip-joint; it was the shaft of the femur that was affected and not the joint. Congenital subluxation

as a cause of arthritis of later onset, as already mentioned, he regarded as important and by no means rare. Patients might reach middle life before any symptoms developed. Radiograms of two unusual cases were shown:—

(a) A boy of 5, with limp for two months on and off. Nothing found but slight wasting of thigh and limitation of extension of left hip. X-ray showed broadened neck with clear area in region of epiphyseal line quite unlike tubercle. Wassermann negative. Treated as for tubercle for twelve months, well for six months, then relapse. Then limitation of all movements. X-ray showed some narrowing of joint-space and fluffiness of joint. What was this affection? Sir H. Gauvain had shown him a similar case, the head of one tibia being the site of the disease.

(b) A girl of 15: hips always stiff. No definite onset. Limitation of abduction of both hips was complained of, and this, and some limitation of extension, was all that was found on examination. Right more affected than left. X-ray showed slight mushrooming of both femoral heads and erosion of the lower part of heads and corresponding part of acetabula. Some increase of limitation in twelve months. No other joints affected.

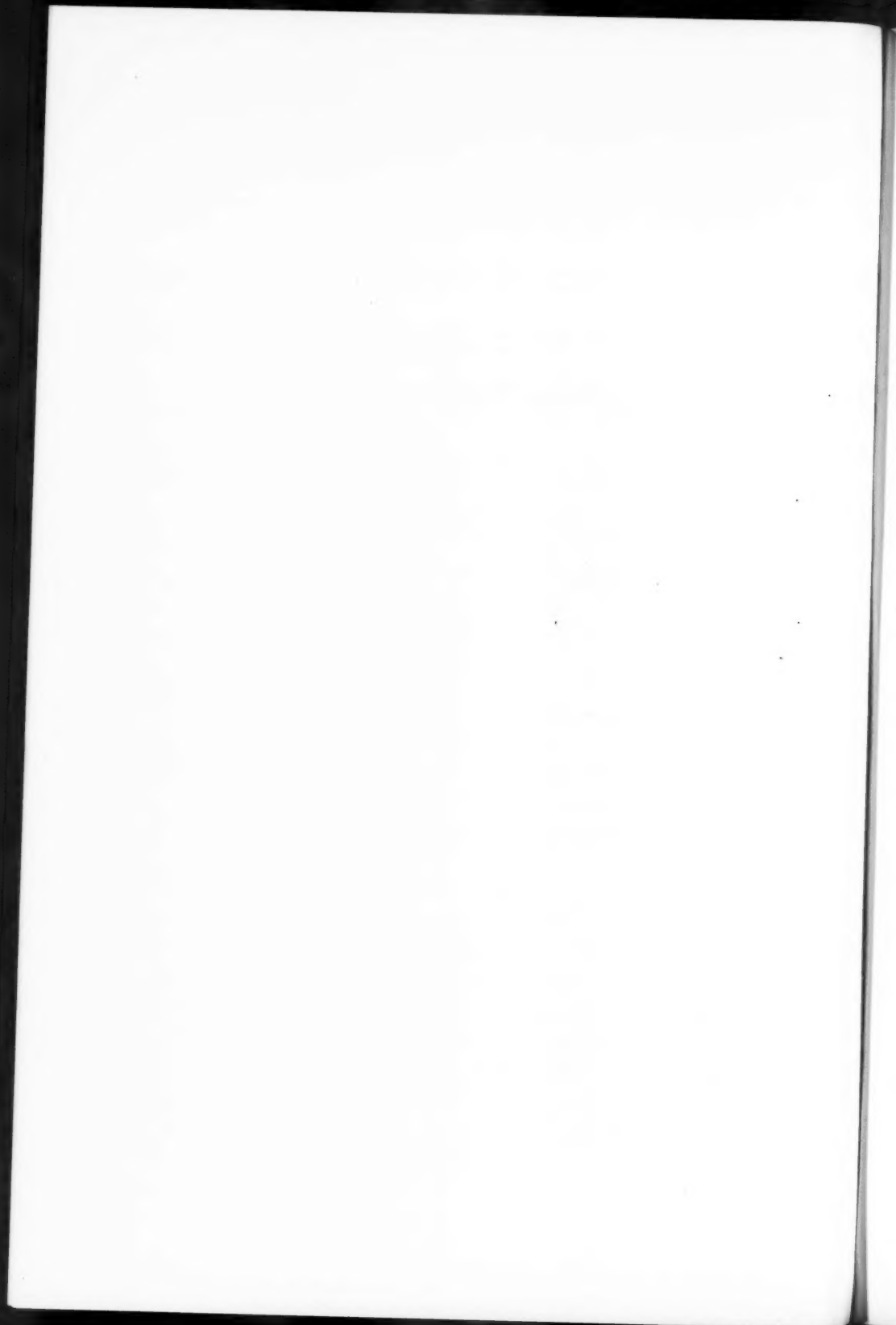
Mr. WHITCHURCH HOWELL described three cases of post-scarlatinal arthritis on which he had operated.

Dr. KNYVETT GORDON: I will make a brief comment from the point of view of the pathologist. In the differential diagnosis between tuberculous and other infections of joints, the histological examination of the blood is often of service, though it does not, of course, override a clinical opinion. In closed tuberculosis, the blood-picture—the mechanical “blood-count” being supplemented by careful scrutiny of the stained film—almost always shows the leucopenia with relative lymphocytosis characteristic of a bacillary lesion, while in the “infective group” which is usually due to coccal sepsis, leucocytosis with relative predominance of granular cells is the rule.

In regard to infective arthritis secondary to scarlet fever, this complication used to be quite common in the days when suppuration in, or necrosis of, the mastoid antrum and cells (which is such a usual feature of scarlatinal otitis) did not receive surgical treatment. Whilst in charge of a large fever hospital, I had to open and drain many cases of pyæmic infection of the hip-joint before the importance of the mastoid cells as a primary focus was recognized. When scarlatinal otitis received the same treatment inside the hospital as would have been accorded to it in an otological clinic, pyæmic arthritis practically disappeared, and at the present day, I imagine, it must be very rare.

In the bacteriological diagnosis of fluid aspirated from joints, reliance should never be placed on a failure to find tubercle bacilli by staining or cultural methods. These methods should always be supplemented by inoculation of experimental animals. Even in streptococcal or staphylococcal cases, inoculation not infrequently yields a positive result when cultural methods have failed to detect the causative organism.

Mr. TODD (in reply) said that rotation tests showed presence or absence of arthritis and not necessarily of tubercle. He said he approved the washing out of pneumococcal joints.



Section of Orthopædics.

President—Mr. H. A. T. FAIRBANK, D.S.O., M.S.

Spinal Curvature in Recklinghausen's Neurofibromatosis.

By W. T. GORDON PUGH, M.D.

THE patient, a girl, aged 12, is an orphan who came from Ireland about two years ago: on this account no history is obtainable but the child says she was born with the drooping eyelid. She has marked thickening and ptosis of the left eyelid, with a scar the result of an operation for neurofibroma of the left orbital region at the Royal London Ophthalmic Hospital. There are two subcutaneous tumours (one $\frac{1}{2}$ in., the other $\frac{3}{4}$ in. long) in the intercostal spaces, and seven minute ones on the chest, upper arms and lower neck. There are about eighteen coffee-coloured patches on the trunk varying from $\frac{1}{2}$ in. to 2 in. in length, with smaller ones in groins.

The spinal deformity consists of a rounded angular kyphosis in the upper dorsal region with its centre about the fourth dorsal vertebra, associated with some reversed-S scoliosis, the rotation of the vertebræ producing some prominence of the right ribs behind and lifting the right shoulder slightly. The clinical appearance is not incompatible with its being a case of tuberculosis of the spine, but the X-ray shows an unusual amount of rotation and the von Pirquet reaction is negative. There is no deformity of the other bones.

The case thus shows the characteristics of Recklinghausen's disease, viz., multiple neurofibromata situated on nerve trunks or on their terminal cutaneous twigs, associated with areas of coffee-coloured pigmentation widely distributed over the body.

This disease is sometimes hereditary, and is sometimes accompanied by definite clinical evidence of changes in the skeleton. Brooks and Lehman,¹ of St. Louis, studied and described seven cases in which bone changes were found. Two of these were in mother and son; the father of another patient had the disease in an advanced stage. In the first case described there were scoliosis and periosteal cysts in the lower end of each femur and on the anterior superior spine of the right ilium. In the second there were tumours attached to both tibiæ and one fibula; one tumour was excised and found to be a neurofibroma, associated with new-formed bone trabeculae. In the third case there probably was a subperiosteal cyst. The fourth had scoliosis and spina bifida. The fifth had scoliosis. The sixth showed lengthening of the left femur with left coxa valga, scoliosis and cortical cysts of the ischium and ilium. The seventh had scoliosis, coxa valga, irregularity of contour of acetabulum, and great trochanter, suggesting the presence of subperiosteal cysts, and periosteal irregularities of the shafts of the femur, tibia, and fibula. The X-ray appearance, suggesting the presence of a subperiosteal cyst, was regarded as being due to the development of a neurofibroma of a nerve in the periosteum, which had caused a certain amount of bone destruction and regeneration, and had led to the tumour being covered with a thin shell of bone.

England and Weiss² have also noted the presence of scoliosis in fifteen cases of Recklinghausen's disease. Gould³ has found that there is frequently a softening of the skeleton; this softening is microscopically and macroscopically the same as osteomalacia.

¹ *Surg., Gyn. and Obst.*, 1924, xxxviii, p. 587.

² *Arch. Derm. and Syph.*, 1921, iii, p. 144.

³ *Quart. Journ. Med.*, 1918, xi, p. 221.

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Dr. C. P. SYMONDS referred to the statement that bony changes occurred in 7 per cent. of these cases. Nodules occurred on the cerebral nerves—especially the eighth nerve. He showed pictures of a case described by Pierre Marie, thirty years ago.

Mr. H. A. T. FAIRBANK (President) called attention to the fact that there were two conditions associated with the name of von Recklinghausen, viz., neurofibromatosis, and multiple fibrocystic disease, and these two could, as already stated, be associated in the same subject. Were they one and the same disease?

Enlargement of the Ends of the Bones of the Lower Limbs.

By W. T. GORDON PUGH, M.D.

THIS boy, now aged 11, was healthy until the age of 5: he walked at 18 months and in due course attended school. The history is that in 1918 he had a severe attack of influenza with rheumatism, and that about two years later the bones of his legs began to bend; he had sharp pains in his legs, and for over three years he did not leave his room. From babyhood he has been under the care of a



foster-mother, who says that since his illness she has given him white and brown bread, sponge-cakes, bovril, cocoa, vegetables and gravy, Brand's essence of chicken and beef, eggs beaten up in bovril and tea, and petroleum emulsion. He has shown a great objection to vegetables and fruit.

The deformity is limited to the lower limbs, in which there is marked enlarge-

ment of both ends of the tibiæ, fibulæ and femora, with extreme wasting of the shafts. The movements at the hips, knees and ankles are restricted; at the hips abduction and rotation practically cannot be obtained; flexion is complete, but extension is not; the knees also cannot be fully extended. The right tibia shows varus, the left tibia valgus deformity; in the latter there is a well-marked curve, concave postero-externally; the left foot is in a position of calcaneus, and movement at the ankle-joint is limited.

Pelvis is not obviously deformed. Spine straight, the chest fairly well formed, with only slight thickening at the costo-chondral junctions and no Harrison's sulcus is present. Bones of arms and wrists slender. Teeth irregular and decayed. Patient's height is 42 in. and weight $32\frac{1}{2}$ lb., the height and weight of the average boy of 5 and 3 years respectively. The calcium content of the blood and urine has been estimated by Dr. Patterson, of Charing Cross Hospital, and found to be normal.

Skiagrams show a concentration of lime salts at the ends of the long bones of the lower limbs, producing triangular shadows. There is also marked osteoporosis of the pelvic bones, the vertebrae, the small bones of hands and feet, the scapulæ and the upper ends of the humeri. The lower ends of the ulnæ and radii, however, do not show any of the signs characteristic of rickets, and these are the least affected of all the bones.

The tendency in recent years has been to regard rickets and osteomalacia as essentially the same, the only modifying influence being age; histologically, it is said, the transition from typical rickets to late rickets, and from late rickets to osteomalacia takes place imperceptibly. The X-ray appearances in the case under consideration (see fig.), however, do not suggest that the disease belongs to this group, and I shall be glad to receive help in the diagnosis.

Mr. ROWLEY BRISTOW suggested that the case might be one of scurvy, in spite of the age. He drew particular attention to the boy's definite dislike of all vegetables, while he recalled a case of scorbutic infantilism which he had seen with Dr. Cassidy; this case had been shown by Dr. Cassidy at a meeting of the Clinical Section.¹ In this case concentration of lime salts at the ends of the diaphyses was a feature of the radiograms shown.

A Case of Congenital Dislocation of Both Hips.

By B. WHITCHURCH HOWELL, F.R.C.S.

F. Y., A GIRL, was first seen by me in 1922, for congenital dislocation of both hips, with some coxa vara. She was then $3\frac{1}{2}$ years of age.

The dislocations were reduced and the right femoral head easily retained in the acetabulum. The left femoral head has, however, always tended to subluxate.

Present condition.—March 3, 1925: Aged $6\frac{1}{4}$ years. Slight waddle.

Left Lower Limb.—About $\frac{1}{2}$ in. shortening. X-ray shows reduction of right femur, with varoid neck. Subluxation of left femur, with varoid neck and anteverted head.

Opinions are asked as to further treatment.

Discussion.—Mr. H. A. T. FAIRBANK (President) said he was against operation on the joint, but suggested it might be worth while reducing the subluxation by simple abduction and, later, operating to form an upper lip to acetabulum by bone-graft. In any case the chances of arthritic changes occurring in the joint were great.

Mr. W. H. TRETHOWAN said he thought anteversion of the neck of the femur an important factor in relapse, and considered that the hip should be internally rotated, and that later an osteotomy should be done below the neck in order to rotate out the shaft and lower end of the femur.

Mr. T. H. OPENSHAW said he agreed with Mr. Trethowan as to the importance of anteversion, and advised separation of the cotyloid ligament rather than making a new acetabular lip.

¹ *Proceedings*, 1922-23, xvi (Clin. Sect.), pp. 16-22.

Arthrodesis for Relief of Forearm Paralysis.

By B. WHITCHURCH HOWELL, F.R.C.S.

A. B., A BOY, aged 10, had complete paralysis of the left upper limb at the age of 2½.

Past Treatment.—Arthrodesis of the shoulder-joint, and reefing of the tendon of the biceps by a colleague, in 1922.

Present Condition.—March 3, 1925. Complete paralysis below the shoulder, except for very slight movement in the fifth digit. Shoulder ankylosed. Elbow-joint at 135°; passive range, 135°—60°.

Opinions are asked as to whether the forearm should be amputated. If so, at what site?

Discussion.—Mr. T. H. OPENSHAW said he was opposed to amputation and proposed arthrodesis of all distal joints.

Mr. W. R. BRISTOW was also opposed to amputation.

Rarefying Osteitis in the Feet of Unknown Origin.

By ROBERT S. LAWSON, F.R.C.S.

THIS is the radiogram of the feet of a pensioner, aged 36. He states that when he joined the Army, in 1916, his feet were somewhat flat, and that badly-fitting boots soon produced callosities on the inner sides of both great toes. These callosities "went septic" and discharged for some months, eventually healing up. Some five weeks ago his right foot showed a slight "flare up," but this has subsided easily under treatment by means of fomentations.

Condition of Feet.—Left foot appears practically normal, except that the great toe is markedly shortened as a result of absorption of the first phalanx. There is a scar on the under surface of the great toe, soundly healed.

Right foot still somewhat œdematous, and the great toe is slightly inflamed, swollen and tender. The skin looks atrophic. The callosities which are present on the under surface of the great toe have recently healed. The toe is shorter than normal, as if from absorption of the first phalanx.

X-ray Appearances.—Left Great Toe: First phalanx almost entirely missing, two shell-like fragments, partly enveloping the head of the metatarsal, being the sole representatives of it. Base of terminal phalanx irregular and eroded.

Right Great Toe: The distal half of first phalanx almost completely eroded away. Base of terminal phalanx irregular and eroded.

Examination of patient's central nervous system does not reveal any signs of tabes dorsalis, syringomyelia or other disease. Wassermann negative. Complement-fixation test for tubercle inconclusive. Suffered from influenza in 1918. Has never had "trench feet." Denies having had venereal disease and has never been in the East.

[May 5, 1925.]

General Enlargement of Joints.

By HUGH THURSFIELD, M.D.

JOHN M., aged 5 years and 4 months.

The deformities of the joints were first noticed by his mother when he was 6 months old, after an attack of bronchitis. The feet turned outwards.

He was a backward child; did not speak until he was 3 years old. Began to walk at 3 years and 7 months, but could never get along without support. He was able to sit up unaided.

Personal and Family History.—He was breast-fed for two months, and then

iven condensed milk, Allenbury's food, and later ordinary food, including milk, fruit and vegetables. He is the third child of five, the eldest being over 15 and the youngest a little over a year; the other children are unaffected. The mother is very delicate.

Osseous System.—Enormous enlargement of all joints; bones involved as well as cartilages. Head rather flattened, circumference twenty inches, meatus to meatus thirteen inches. Bridge of nose wide and rather sunken.

The following striking peculiarities are present:—(1) Kyphosis and scoliosis of the dorsal spine, (2) enormous osseous and cartilaginous enlargement of joints, (3) disproportionately great size of the halluces, and (4) inability to flex fingers, especially at the metacarpo-phalangeal joints.

The upper segments of the limbs are not specially shortened. Finger-tips reach down to mid-thigh, the nails are unusually short, fingers thick and stumpy, but without any resemblance to the "main-en-trident" of achondroplasia. Wassermann reaction negative. Urine normal. Blood calcium, 12 mgm. per 100 c.c.

Mr. H. A. T. FAIRBANK (President) thanked Dr. Thursfield for bringing this interesting case, and showed radiograms of a case he had seen with Dr. Donald Paterson, which may have been similar in nature but in an earlier stage. He called attention to the curious affections of the digits associated with certain general diseases, e.g., the shortened big toe in idiopathic myositis ossificans.

Chronic Great Palmar Bursitis (Compound Palmar Ganglion).

By PAUL BERNARD ROTH, F.R.C.S.

G. R., GIRL, aged 15. Mother states that since the age of 3 years there has been a swelling in the palm of the right hand much the same as it is now: sometimes bigger, sometimes smaller. It is quite painless, and patient is able to write, play the piano and paint, but she cannot grip anything.

Examination reveals marked distension of the great palmar bursa, both below and above the annular ligament, with fluctuation through. X-ray examination shows the bones to be normal.

Family History.—One maternal aunt died of phthisis, aged 35.

What is the cause of this condition? I believe it to have been originally tuberculous; but the active process has been dead for years, and has left behind it distension, thickening and melon-seed bodies. I propose to open the sac and empty out the contents.

Members of the Section agreed that the condition was probably tuberculous and advised careful dissection out of the diseased synovial membrane.

Recurring Dislocation of Ulna.

By PAUL BERNARD ROTH, F.R.C.S.

J. D., GIRL, aged 15, suffering from weakness of right wrist. She states that five years ago she fell down stairs and broke her forearm. She was admitted to King's College Hospital under Mr. Fairbank, and the radius was operated upon.

Examination reveals that the lower end of the ulna is quite free from the radius, and can be moved backwards or forwards from it. When she tries to grip anything, a click is felt, due to partial dislocation, and the effort ceases.

X-ray examination shows—besides the line of fracture in the radius, 2 in. from its lower end, with sound bony union and slight angulation—that the ulna is quite free from the radius, is deficient in length, and that its styloid process is detached.

I propose to fix the lower end of the ulna to the radius with a graft.

Mr. JENNER VERRALL suggested that before destroying pronation and supination in this way it was worth trying to stabilize the ulna by transplanting the extensor carpi ulnaris tendon into a deep groove cut on the inner side of the ulnar head. He had had success by the use of this method.

Specimen of Unusual External Semilunar Cartilage.

Shown by H. A. T. FAIRBANK, D.S.O., M.S. (President).

PATIENT, a boy, aged 14, complained that his knee gave way. Nine months previously his knee suddenly gave way and he fell during a tug of war. He continued to pull. No definite locking. Since that time his knee gave way whenever he ran. Pain is at back of joint, which is always slightly swollen. Knee snaps when at about an angle of 100° during flexion: something jumps at outer side of ligamentum patellæ. At operation the external semilunar cartilage was found to be very broad, so that the external condyle of the femur could not articulate directly with the tibia. The inner border of the cartilage, normally free, was reduced to a short length, and this quite close to the spine of the tibia. In front, the outer margin of the cartilage was loose and attached only to the infrapatellar fat for about a quarter of its length; behind it was firmly attached to the tibia for a quarter, i.e., as far back as the region of the external lateral ligament. On this being liberated the cartilage came freely forward, showing that the posterior part of the free border was unattached until the posterior end was reached. The cartilage was divided as far back as possible and its anterior three-quarters removed. The cartilage is almost as broad as it is long.

Mr. W. R. BRISTOW suggested that the anterior quarter had been torn from its attachment and that the snapping was thus explained. He also thought that the looseness in the posterior part was merely an increase of the normal freedom of the cartilage in the region of the popliteus tendon.

Section of Otology.

President—Dr. J. KERR LOVE.

On Some Neglected Corners of the Otological Field:

PRESIDENT'S ADDRESS.

By J. KERR LOVE, M.D.

I PROPOSE to indicate rather than to describe two or three corners of the otological field which are imperfectly occupied or not occupied at all. It is over forty years since I started practice and a good deal over thirty since I began the study and practice of aural surgery. In 1891 when I took charge of the ear beds in the Glasgow Royal Infirmary—there were no ear wards—the beds were on the medical side of the house. There was no house surgeon. I had a small share of a house physician. I was the entire staff. No radical mastoid operation has been done in the ear department, if indeed in the infirmary. Long before I left the hospital there was a fully equipped modern department. During these thirty-three years the progress of aural surgery has been magnificent and it has been uninterrupted. But outside of surgical treatment, there has not been great advance in otology. We can hardly do more than we could do fifty years ago in the cure of deafness. We are only beginning to try to prevent deafness, and the consequence is that the management of chronic deafness is largely in the hands of the empiricist and the quack.

There are three corners of the otological field which I would like to see more fully occupied:—

(1) We must urge on local authorities and if necessary on the Government the need for the better staffing of infectious disease hospitals, Poor Law hospitals and institutions for the deaf with otologists.

(2) We must supply the public with some guide as to the use of aids to hearing.

(3) We must devote our time to obtaining some guidance with regard to the prevention of hereditary and congenital deafness.

There is no body in this country so well fitted to speak of these subjects as this Section of the Royal Society of Medicine. As an example of the lack of staffing in hospitals for infectious diseases, I take my own city. There are several thousand beds in the hospitals in and around Glasgow and no otologist has been appointed to any of them. One result is that many cases of infectious disease are dismissed with discharge from the ear and nose and fresh cases of the disease occur. Another result is that many cases which might be cured by partial operation in hospital come up later for more serious operation. The infectious diseases hospital, under the charge of the otologist, is the proper place for the practice of preventive measures. Were this practice carried out many of the major operations of aural surgery would never be needed. Most of the cases of intractable middle-ear suppuration occurring in

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school children—cases which resist prolonged treatment by cleansing, by ionization and by both of these after tonsil adenectomy—are due to scarlet fever and measles which should have been treated to a finish before they left the infectious diseases hospital.

As an example of the lack of staffing in institutions for the deaf, I give that of a large English institution in which there are on the staff, a physician, an ophthalmologist, a dermatologist, an odontologist—but no otologist although there is not a hearing child in the school. There are cases of middle-ear suppuration and plenty of cases in which tonsil adenectomy should be done. But that is not the chief use to which the clinical material in these institutions for the deaf must be put. These institutions are the real stage on which the connexion between congenital deafness and hereditary syphilis must be worked out. A dozen years ago I established the fact that congenital deafness was due to hereditary syphilis in some cases in our British institutions for the deaf and gave in proof detailed family trees. From the Paris institution we learn that this association may be far more common than has hitherto been suspected. This corner of the otological field should be occupied as soon and as fully as possible. To make this problem as concrete as possible, let me say that the great majority of deaf-born children in our institutions and schools are single cases of deafness without any clear history of family deafness, without any stigmata of syphilis, either in the deaf child or in the parents. I call these cases of sporadic congenital deafness in contrast to family or true hereditary deafness. On one side are cases of acquired deafness, some of these due to syphilis and a few of them congenital or with deafness occurring in very early life. On the other side are cases of family or hereditary deafness unconnected, so far as we know, with syphilis or any other poison.

Now, recent figures from the Paris institution would throw nearly all the cases of sporadic congenital deafness into the syphilitic group and make syphilis account for at least half the cases of congenital deafness. In spite of the unsavouriness of the connexion, I almost wish this would turn out to be true, for then we should have something definite to combat, if not in the children, at least in the parents.

My own observations made some fourteen years ago consisted of over 150 Wassermann tests, about half of them in cases of sporadic congenital deafness. My conclusions were that whilst congenital deafness is sometimes due to hereditary syphilis, this result was uncommon. Instead of putting this ratio down as 50 per cent., as does Dr. de Parrel, of the Paris institution, I should put it down as about 5 per cent., and at most 10 per cent. of all cases of congenital deafness. Now, I am not much interested for the moment in asserting that I am right and the French observers wrong. If the figures from the Paris institution are right, we could not take them as true for Great Britain. We must settle the question for ourselves, and what I want to emphasize here is that the real stage for the settlement of the question is the deaf school of this country, and that this work is not being done. We have here a neglected corner of the otological field.

I should perhaps explain here that Dr. de Parrel, who supplies the figures from the Paris and other French institutions, relies not on the Wassermann reaction but on what he describes as the far more delicate Desmoulière test. Regarding this latter test, Professor Carl Browning, who made the blood examinations for my series of cases, remarks, "An antigen which yields so many indiscriminate positives ought never to have been used."

Leaving congenital deafness due to hereditary syphilis, we come to that large class of patients for whose deafness we can do nothing and who fall back on some form of "aid to hearing."

Most of us must have felt commiseration for the poor patient who has invested a good part of her savings in an expensive electrical aid and who shortly puts this on the shelf as useless. I hope that some day a physicist will make use of the clinical

fact that many patients with middle-ear disease hear better in a noise, and give us an instrument attached to the side of the head which will create near the ear the disturbance which makes "hearing better in a noise" possible. In the meantime an indication from this Section might be given as to the kind of case in which the electrical aid is likely to be of use.

A well known writer approached me recently with the object of getting guidance as to the prevention of hereditary deafness. His plan was to sterilize the deaf. He did not seem to know that a much more important step would be to sterilize the hearing relatives of the deaf. Both, of course, are quite impracticable. But this Section of the Royal Society of Medicine might put in the hands of those who manage the deaf a set of recommendations by which the occurrence of hereditary deafness would be gradually, but certainly, lessened. Again the institution for the deaf is the real stage for this inquiry.

There is nothing dramatic, nothing spectacular about the work indicated under these three headings. One is an application of surgical principles which is long overdue: another is a piece of service due to a long-suffering and isolated section of our patients sitting behind the gates of deafness; the third is part of the great movement of preventive medicine which, in the long run, will be the chief work of the practitioner. All are humanitarian in their outlook, but to all the scientific method may be applied.

Case of Nævoid Angioma of the Tympanum (? Endothelioma). Microscopical Section.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

A MALE patient, aged 29, had suffered for a little over a year from attacks of giddiness. A specialist found a swelling in the depth of the meatus involving and occluding the postero-inferior two-thirds of the tympanic membrane, and lowering of the caloric reactions of the labyrinth. Paracentesis was followed by so much hæmorrhage that he suspected the presence of a bulging of the jugular bulb.

I saw him three months ago and found the condition as reported. The swelling was smooth and red, pulsating when felt with a probe and resilient after compression, bleeding when manipulated.

Adrenalin drops were used for a few days. A portion was removed for microscopic examination. The hæmorrhage was considerable but could be controlled with enough ease to exclude the diagnosis of projection of the jugular bulb.

The microscopical examination showed it to be a nævoid angioma on which possibly an endothelioma had arisen.

Operative removal was carried out by means of sharp spoons and free use of the galvano-cautery, a ready approach being afforded by mastoid detachment of the auricle and membranous meatus, and enlargement of the bony meatus by means of the gouge, and of the external meatus by Körner's plastic. There seemed to be no deep extension into the bone, the growth apparently arising from the tympanic membrane and mucous membrane of the adjacent floor of the cavities.

The tissues removed at the mastoid operation were reported to consist of the remains of an angioma "without any evidence of epithelial or sarcomatous new-growth."

When seen recently after an absence of two months the patient said he felt perfectly well but there was a small elastic swelling in the postero-inferior part of the tympanum. This was punctured with the galvano-cautery point and again with the zinc electrolytic needle. The question of radium treatment is under consideration.

A case of the same kind is described by Fischer, and criticized by Wittmaack; a case has also been exhibited by Dr. Peters. In the latter, although the *naevus* was confined to the tympanic cavity, there was interference with the labyrinth. My operation in this case was performed last July.

Specimen showing Bony Ankylosis of Malleus and Incus.

By E. WATSON-WILLIAMS, M.C.

THIS specimen shows bony ankylosis of malleus and incus, with filling up of the groove lodging the capsular ligament of the joint. The whole of the long process of the incus, and the tip of the handle of the malleus, have been destroyed; but the bones are otherwise not obviously diseased. The specimen was removed at operation from a young man who had been deaf for many years, with *otorrhœa*.

DISCUSSION.

Mr. SYDNEY SCOTT said that the beautiful way in which this specimen had been demonstrated by the exhibitor was a method new to him.

Mr. A. R. TWEEDIE said that the beautiful specimen shown served to emphasize the importance of indicating the exact locality of disease, rather than employing the loose expression "*chronic suppurative otitis media*"—a point to which he had already directed attention.

Sir JAMES DUNDAS-GRANT said that in cases of ankylosis of malleus and incus without perforation a diagnostic sign would be a labyrinthine "*fistula symptom*" on suction. In cases in which he knew there was action by suction upon the fenestra ovalis, the disturbance of equilibrium was enormous. One obtained a nystagmus in the opposite direction to that usual in fistula of the external canal. It did not merely act on one canal, but on them all. In one case the membrane of the fenestra ovalis was visible, and suction caused such a disturbance that the patient almost fell off the chair. When this was elicited in a person with a normal membrane, one must expect to find ankylosis of the malleus and the incus.

Dr. J. KERR LOVE (President) said that in the intratympanic stage the mastoid operation was usually hurried too much, and enough notice was not taken of what was removed from the middle ear. It would be well to observe carefully the conditions in the tympanum.

Statistics of Results of Zinc Ionization in Chronic Otorrhœa in over 600 Cases.

By A. R. FRIEL, M.D.

IN 1900 Professor Leduc, of Nantes, began to publish his discoveries in the method of treatment which we call ionization, with the principles governing its application.

These statistics show the results obtained by applying his principles in the treatment of local sepsis in chronic *otorrhœa*.

The patients were school children at the London County Council Ionization Clinics, held weekly. While attending they received no treatment in the intervals.

Table I gives an analysis of the causes of chronicity met with in these cases; the number of cases in which a particular condition was regarded as the main element in the case; and the results obtained. For convenience the table is drawn up largely on the anatomical basis to which we are accustomed. In the tympanic cases it is possible to state, besides the position, the conditions present, but in the attico-mastoid cases it is only possible to state the position.

TABLE I.—OTOLOGICAL CLASSIFICATION AND SUMMARY OF RESULTS OF SUPPURATING EARS TREATED BY IONIZATION

Cause of chronicity	Total	Cured	Lost sight of	Still under treatment	Sent for operation or other treatment
<i>I.—Tympanic conditions:</i>					
(a) Tympanic sepsis	251	234	17	—	—
(b) Tympanic sepsis + granulations ...	82	61	16	5	—
(c) Tympanic sepsis + polypus ...	29	18	5	3	3
(d) Tympanic sepsis + caries ...	4	2	2	—	—
(e) Tympanic sepsis + cholesteatoma ..	3	1	2	—	—
(f) Tympanic sepsis + other conditions	3	2	1	—	—
<i>II.—Tympanic conditions + involvement of the Eustachian tube, nose, pharynx, or mouth</i>	37	18	9	4	6
<i>III.—Tympanic conditions + attic or mastoid</i>					
No previous operation	—	24	33	6	72
Operation already	—	11	7	5	15
<i>IV.—Tympanic conditions + external otitis ...</i>	14	9	2	1	2
<i>+ stricture of meatus</i>	1	1	—	—	—
<i>V.—External otitis</i>	12	9	2	1	—
<i>VI.—Tympanic conditions + cause undetermined</i>	7	4	2	1	—
Total	616	394	98	26	98

The incidence of the special causes of chronicity shown in the table approximates to, but is not quite identical with, the actual incidence, for a fair number of children come to the clinic in whom the otorrhœa is very slight, and, as a rule, these are not ionized. Boracic powder is insufflated, and they are directed to return in a week. Usually the otorrhœa has then ceased. If so, they are discharged, and they are not included in these statistics. A few others obviously require operation, or live too far away to attend again, or they or their parents object to electrical treatment. These, too, are not included.

It will be seen that the majority of the cases fall into two classes—those in which the cause of chronicity is to be found in the tympanum, and those in which it is found also in the attic or mastoid. Contrary to what might be expected there was not a large number in whom it was considered that the maintenance of the otorrhœa was due to throat and nose conditions.

Table II gives an analysis, in the tympanic sepsis cases, of the number of ionization treatments and the number of visits paid to the clinic till the discharge ceased. This table shows the kernel of the ionization case in its scientific and economic aspects.

TABLE II.—ANALYSIS OF TREATMENT OF TYMPANIC SEPSIS CASES.

<i>A. Cured.</i>		
Number of cases	Number of ionization treatments	Number of visits till discharge ceased
143	1	1
41	1	2
7	1	3
4	1	4
1	1	6
1	1	8
9	2	2
10	2	3
8	2	4
4	2	5
1	2	6
2	3	3
1	3	4
1	4	6
1	4	7

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Number of Cases.	B. Lost sight of.
8	1 visit, 1 ionization
3	2 visits, 1 "
3	3 " 1 "
1	3 " 2 ionizations
1	4 " 2 "
1	4 " 3 "
—	
17	

Zinc ionization was used by Professor Leduc as the remedy for local sepsis. The treatment of tympanic sepsis by this method resembles a laboratory experiment in which the conditions are so arranged as to give a decisive answer to a single question. The table shows that one may expect in such cases a rapid cessation of the discharge.

The only treatment given to these tympanic sepsis patients was zinc ionization without or with insufflation of boracic powder.

To turn to the attico-mastoid cases. In a few a favourable result has been obtained. The diagnosis of mastoid or attic disease has been made by observing pus on aspiration from the aditus region, or discharge at Shrapnell's membrane.

Table III summarizes in general outline the principles by which the writer is guided in the treatment of chronic otorrhœa.

TABLE III.—INDICATIONS FROM THE

CAUSE OF CHRONICITY	AS TO	TREATMENT
I.—Accessible sepsis, e.g., tympanic sepsis		Zinc ionization with or without boracic powder insufflation
II.—Accessible sepsis with second factor in ear, e.g., polypus		Remove second factor, then ionize ear
III.—Accessible sepsis with inflammation in a neighbouring organ, e.g., rhinitis		Treat neighbouring organ, and ionize ear
IV.—Inaccessible sepsis, e.g., cavity in mastoid opening directly into tympanum		Use special instrument to gain access (a) attic cannula; (b) gelatin covered wire
Attic disease		or, make area of sepsis accessible
Chronic mastoiditis		e.g., destroy outer attic wall, ossiculectomy, partial or complete mastoid operation, and then ionize

Ionization is not a remedy for chronic otorrhœa, but it is a remedy for sepsis, and as sepsis is frequently the sole cause of chronicity, it happens that zinc ionization is a remedy for many cases of chronic otorrhœa. Cases are unaffected by ionization when the process cannot "get at the spot."

DISCUSSION.

Dr. A. G. WELLS said he would like to bring before the Section some points of interest in connexion with ionization, in his capacity as aurist to the London County Council. In addition to the useless treatment which obtained at the sixty treatment centres at the hands of nurses, he had incorporated in his scheme of treatment that by ionization, and hospital treatment for the purpose of operating on mastoids. That hospital was a Metropolitan Asylums Board institution in London, which commenced its work in 1917. Over a large number of cases, his results closely approximated those Dr. Friel had now put forward.

The number of operations done for chronic mastoid disease in London was comparatively small; most were on acute cases. It would be agreed that they were unsatisfactory, as in the majority of cases the patients left hospital before they were healed. This was due to the demand for beds, and to the fact that the after-treatment was in the hands of house surgeons. Sooner or later, cases operated upon for chronic disease found their way to him and a proportion were healed without further operation. In a large number of cases something further had to be

done. One of the trying features of these cases was after-treatment, and it was with the object of shortening this period that he had adopted ionization treatment for some years as a routine measure.

In 1920 the operations done at that hospital numbered 118, and in about 80 it was successful, giving a figure of 67·7 per cent. In 1921, 158 patients were operated upon, and gave 87·3 per cent. of successes, and last year the number was 137, of which 129, or 94 per cent. were successful. It was difficult to get an idea of the percentage of patients operated upon at general hospitals who were cured, but careful investigation over a definite period led him to say that 15 per cent. to 20 per cent. at the outside were cured as a result of the operation done at the hospital. Latterly, ionization was begun closer to the date of operation. Though he did not attribute the whole of the good after-result to the ionization, it was an important factor. Another factor was the increased experience of those looking after these cases. Last year, the average length of time per case was sixteen weeks from operation to date of healing. There were sixty-four cases for which he arranged an intensive course of ionization; i.e., it was commenced within a day or two of the operation and administered on several occasions. In those, the average length of time per case from operation to healing was 10·9 weeks, as compared with the sixteen weeks for the less intensive treatment. As a result of this treatment he had noticed reduction of the inflammation, clearing up of the wound, and lessening of the pain. At one period the 94 per cent. of cures dropped to the seventies, and that was traceable to the work being indifferently done and to the fact that there was a new staff of house surgeons inexperienced in the work. He asked if others had undertaken this procedure, as it would be of interest to compare experiences and results.

Mr. J. S. FRASER said it was well known that when a new remedy or a new method was introduced, there was a tendency for a very brilliant impression to be entertained respecting it; then, as years went on, the colours faded and something like a true picture was recognized. The specialty had experienced this in regard to the treatment of otosclerosis by blistering the drumhead or by injections of fibrolysin followed by massage.

As a result of his own experience of ionization, he was not prepared to homologate the opinions expressed by the last two speakers. He certainly did not agree with Dr. Wells as to the poor results following the radical mastoid operation. He found that by the immediate skin-grafting of the cavity by Mr. Marriage's method one could obtain at least 70 per cent. of cures. Dr. Wells had also mentioned the prolonged and painful after-treatment of these cases. The first dressing was done on the fifth day after the operation, usually without any pain, and the wound was re-packed with iodoform worsted; the second dressing was done one week after operation and there was no more packing after that. As a rule patients left hospital within fourteen days of the operation, many of them in ten days, and in Edinburgh the patients were sent to the convalescent home, where they remained three weeks. The after-treatment could be carried out by anybody with brains and a pair of hands. In any case a nurse could do all that was necessary, with peroxide drops and syringing. The superficial layers of the graft came away; at the end of five weeks the cavity was lined by smooth epithelium, and a week or two afterwards it became white and glistening. It was true that some cases did not do as well as this, but his own experience of trying zinc ionization in cases which did not dry up after operation had not been good. In these the Eustachian tube remained open and there was a mucoid discharge. The patient, however, was free from any danger of a labyrinthine or intracranial complication. Ionization was not always free from pain to patients. Children did not like it, especially with a current of more than two milliamperes. The conservative (wet) method of treating chronic middle-ear suppuration, carried out at his clinic, gave 50 per cent. of cures or apparent cures. Of the remaining 50 per cent. ionization would cure less than half, and the remainder, which belonged to the attic and antral group, were not cured. Even cases which were supposed to have been cured by one or two ionizations were apt to relapse. If cases were seen again in a month or two, it was often noted that the discharge had returned. Therefore, though all possible help was needed in regard to these cases of chronic purulent otitis media, it was not wise to be too enthusiastic about the results.

Mr. E. WATSON-WILLIAMS asked whether this method of treatment was applicable to the adult as well as to children. He had found that even 2 milliamperes of current disturbed the adult considerably, even when a second anode was placed on the opposite mastoid. He used the method in a Pensions clinic for about a year, only for the resistant cases, and the number of successes did not warrant a continuance of the treatment. In one clinic, he had

had thirty-seven radical operations; thirty-four were completely successful. There was an enormous gap between that proportion and the 15 per cent. to 20 per cent. mentioned by Dr. Wells.

Dr. WELLS asked leave to explain his meaning. He said it was difficult to ascertain the exact proportion of cases which cleared up. He had to take, for a certain period, a large number of cases which had been operated upon in hospital, and to find out whether they cleared up as a result of the first operation. In *that* series he found that 15 per cent. to 20 per cent. was the proportion which had cleared up. That might not be the true figure for all cases, but in the majority of instances the patients who were operated upon needed something further in order to get them well. The procedure was as applicable to adults as to children, and he did not consider the treatment an unpleasant one. He had a special clinic where over 2,000 ionizations a year were being done, and in the great majority no unpleasant symptoms were caused. In answer to Mr. Fraser he (the speaker) did not suggest that the radical mastoid operation was not successful; his idea was to find out whether, by ionization, the length of the after-treatment could be diminished, and his conclusion was that ionization, applied intelligently, did accomplish that.

Dr. J. KERR LOVE (President) said it was necessary to keep in mind that this was a particular application of a general principle. If the cavity was wide enough to be kept clean and it were well drained, it would heal. There were many ways of doing that, of which zinc ionization was one. In Glasgow it was used for the old cases in which cure had not resulted.

Dr. FRIEL (in reply) said that in cases of chronic otorrhœa it was important to state what factor one was treating. Strictly speaking zinc ionization was not a remedy for chronic otorrhœa but it was a remedy for sepsis, and as sepsis in an accessible situation, the tympanum, was frequently the cause of chronicity it happened that zinc ionization was a remedy for many cases of chronic otorrhœa. But cases were unaffected by ionization when the sepsis was in a position which was not accessible. In such cases one might try, but seldom with success as far as his (the speaker's) experience went, to gain access by the use of special instruments such as an attic cannula; or one might make the area accessible by operation and ionize subsequently. This was the method Dr. Wells had adopted with the result he stated. With regard to the idea that it was an unpleasant form of treatment a good deal depended upon the kind of rheostat which was used. If one chose the water rheostat designed by Prof. Leduc, the graduation of the current could be so gentle that unpleasantness was minimized. That could not be said when one cell was introduced after another by a handle passing over studs. Children occasionally fell asleep during the treatment. Ionization was equally applicable in the case of adults. Zinc ionization did not guarantee that there would be no recurrence, but in his experience recurrence of discharge in tympanic sepsis cases was uncommon. In regard to cases in which polypi or granulations were present he made it a rule to see the child again in a month before discharging it from the clinic.

Tubercular Disease of Mastoid.

By FRANCIS MUECKE, C.B.E., F.R.C.S.

PATIENT, a male, aged 16. October 1, 1924. *History*: Two years discharge; one month swelling behind ear.

Large circumscribed swelling behind ear. Free meatus discharge coming mostly from sinus near the rim. Sinus went backwards to mastoid. Posterior

swelling was typical suppurating tubercular gland; microscope showed usual changes. Whole cap of mastoid separated as one sequestrum, greenish in colour, exposing dura and sinus freely. Further large sequestra of outer table of occipital bone removed.

Treatment: Direct sunlight. Excellent progress.

Malignant Disease of Antrum (Mastoid).

By FRANCIS MUECKE, C.B.E., F.R.C.S.

PATIENT, a female, aged 51. *History:* One month impairment of hearing; two weeks of facial paralysis.

Hard swelling completely surrounding meatus with bleeding granulation in centre. Section showed malignant disease.

February 24.—*Treatment:* Free removal with knife and chisel; origin found to be in antrum, and process had broken through into meatus, extending upwards and downwards. Free use of diathermy over bone; light use of diathermy in middle ear.

Result.—Good recovery. One attack of cellulitis, necrosed bone slowly separating operation section from meatus, showed malignant epithelial disease of parotid type.

Complete Sequestrum of Petrous Portion of Temporal Bone.

By FRANCIS MUECKE, C.B.E., F.R.C.S.

PATIENT, a male, aged 34. Mid-ear suppuration, following shell-burst in France in 1916. Continuous suppuration since. Facial palsy fourteen days. Great difficulty at operation in making sufficient room to remove sequestrum. Carotid easily seen. No sign of any tubercular disease. Complete recovery and complete mastoid healing. Sequestrum shown.

DISCUSSION.

Mr. SYDNEY SCOTT said that the sequestered labyrinth was a perfect specimen. He remembered one similar specimen in St. Mary's Hospital Museum. They had also had one specimen from a case at St. Bartholomew's. In the course of a radical mastoid operation, a sinus was found in the region of the external semicircular canal. Mr. West passed in a probe and felt bone rocking. It proved to be a sequestrum of the whole labyrinth. It was easier to remove such a sequestrum if the facial nerve was destroyed, but in the case at St. Bartholomew's the nerve was intact and the labyrinth was removed in two sections.

Mr. J. S. FRASER (discussing the sequestrum case) said that many otological text-books stated that necrosis and sequestrum formation were common in chronic middle-ear suppuration of the ordinary pyogenic type. That had not been his experience. If a sequestrum was found at operation, subsequent investigation usually revealed the fact that the case was tuberculous. Mr. Muecke, however, told him that it was apparently not so here. He agreed that in bad cases of acute scarlatinal otitis media and mastoiditis there might be sequestrum formation. These cases also were rare. He was at a loss to understand what process had produced the large sequestrum in the present case.

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Mr. G. J. JENKINS said that a mass destruction like that in the sequestrum case was very difficult to explain, and he wondered where the lesion which cut off the blood supply was situate. Had there been a localized meningitis in the neighbourhood of the internal auditory meatus, or septic labyrinthitis?

Mr. MUECKE (in reply to discussion on the last case) said that he did not know whether the explosion the man was in had anything to do with the condition; it was feasible to think it had, especially as the patient said he did not suffer from any trouble of the kind until after the explosion. Possibly a fracture was produced at the time. No trace of disturbance of taste or of any other nerve function could be found.

Section of Otology.

President—Dr. J. KERR LOVE.

Skiagrams from a Patient with Malignant Disease of the Ear.

Shown by SYDNEY SCOTT, M.S.

G. G., A MAN, aged 27, first seen at St. Bartholomew's Hospital, November, 1924, complaining of pain and swelling of the ear.

History of Present Condition.—Between three and four years ago an epithelioma had been removed from the scalp, and glands excised from the left side of the neck. Histological sections from scalp and glands showed well-marked cell-nest formation. (Report kindly sent by Mr. C. O. Davies, of the Royal Infirmary, Liverpool.)

Notes on Present Condition.—On the vertex of the skull the scalp shows a broad, flat, well-healed scar over the left side of the sagittal suture. The left pinna protrudes and the meatus is blocked by an ulcerating mass. Irregular mass below the ear, adherent to the skin, and immovable, apart from deep cervical structures.

X-rays show absorption of the apex of the mastoid.

Comment.—The case illustrates apparently secondary malignant growth of the cervical lymphatics which has invaded the auditory meatus and mastoid process. Without the history, one would have regarded the ear as the primary seat of the disease.

DISCUSSION.

Mr. W. H. JEWELL referred to the case of a patient whose breast had been removed a year previously for a fungating carcinoma. She had pain in the right ear. There was a mass of growth behind the ear, involving the mastoid. She died two months later. The ear and breast disease were on the same side. There was no recurrence locally in the breast or glands.

Dr. J. KERR LOVE (President) said he had not been impressed with the success of radium treatment; it was not successful even after operation.

Sir JAMES DUNDAS-GRANT drew attention to sections he exhibited showing the action of radium on a growth described as an endothelioma engrafted on angioma of the middle ear. Radium had since been applied and the appearances were now those of a chronic inflammatory, degenerative character, with no sign of tumour cells.

Sir WILLIAM MILLIGAN said that one would not expect any great change as a result of application of radium in a dense squamous-celled growth; it had not the same effect as in endotheliomatous conditions, or in those with an amount of fibrous tissue. The present growth was an epithelioma of the ear secondary to an original focus in the scalp, a type of case not often seen. If amenable to surgical treatment, an attempt should first be made to remove the growth by surgical measures, i.e., by the removal of the external ear and by dissection of the lymphatic plexus in both anterior and posterior triangles of the neck. Should recurrence take place, the best treatment was diathermy, which blocked the lymphatic tributaries more potently than radium. If radium was chosen, the best way to give it was not by means of external applicators, but by a dense barrage of small tubes inserted all around the area which the growth had occupied. He did not remember a case of the kind in which life lasted longer than a few years; metastasis occurred because the methods of operation in the first instance were probably not radical enough.

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Mr. H. V. FORSTER said he had had one case of primary epithelioma of the external meatus. After a preliminary operation recurrence rapidly took place, and he did a radical mastoid operation. After this operation there was another rapid recurrence and he then applied radium, but without good result.

Dr. LOGAN TURNER said it should be remembered that cases of the kind which Mr. Jewell mentioned were not necessarily recurrences or metastases of the original growth. The woman in question might have had a tendency to cancer formation, and in the second area the tumour might have been an entirely new formation.

Mr. SYDNEY SCOTT (in reply) said he considered it was more probable that the growth in the ear was secondary to that in the scalp, for the occurrence of two primary malignant growths in one patient was very rare. He had searched the literature with Sir Henry Butlin twenty years ago, for instances of malignant disease implanted by contact, such as malignant disease on one vocal cord being implanted on the opposing cord, or two separate cancerous growths on the lip or the vulva; but they found records of very few indeed, out of many thousands of cases. He had seen very few cases of secondary malignant disease in the ear. He recalled one other. The patient was suffering from dysuria. When admitted he had good hearing, but gradually the drum disappeared. There was no growth in the external meatus. The patient died some months later, and the autopsy revealed malignant disease involving the petrous bone and middle ear, secondary to a primary malignant growth in the prostate. Sir Frederick Andrewes kept the specimen for the museum. He agreed with Sir William Milligan's views and recommendation of diathermy whether the bone became necrosed by the treatment or not.

Dr. ALBERT GRAY said he had seen one case of cancer of the meatus secondary to disease elsewhere. After treatment with radium the cancer disappeared. The primary disease was scirrhous of the breast.

Dr. J. KERR LOVE (President) said that probably the case under discussion was another example of independent growth in the same patient. The probability of these conditions being independent should always be borne in mind.

Large Mastoid Fistula Closed by a Tube-flap.

By DAN MCKENZIE, M.D.

BOY, aged 9. Fistula followed a Schwartze operation with unduly prolonged packing of the post-aural wound. After many unsuccessful efforts to close it by the ordinary methods, we at last succeeded by using a tube-flap (Gillies) cut from the neck. The result is good, but it would have been better had we cut the flap from the forearm.

DISCUSSION.

Mr. J. F. O'MALLEY asked whether Dr. McKenzie had tried under-cutting the old wound incision, and then doing a plastic operation through the meatus, closing the post-aural wound, and dressing through the meatus.

Sir JAMES DUNDAS-GRANT asked whether the fistula continued to discharge, or was it simply a dry perforation, and was the operation proposed for æsthetic purposes? If there was a discharge, Mr. O'Malley's proposal seemed excellent, though he (the speaker) disliked a fistula being left in the posterior wall of the meatus if the post-aural opening could be closed without it.

Dr. DAN MCKENZIE replied that there was no discharge when he operated. There was a good deal of contraction and collapse of the meatal wall. He did several elaborate operations and they all healed well, but slowly and persistently the contractile tissue re-asserted itself, and two months after matters were no better. He strongly recommended the operation in similar cases.

Dr. J. KERR LOVE (President) said he had had several cases of the kind. He had succeeded, without much difficulty, in closing these fistulae by manipulating the flaps on the back of the auricle and the side of the head.

Case of Deafness, with Loss of High Tones, (?) Congenital.

By T. H. JUST, F.R.C.S.

E. B., MALE, aged 21. Works in engineering tool shop.

Deafness and tinnitus for years, ? increasing lately. Cannot hear whistling or birds singing.

Scarlet fever at age of 6. No history of aural discharge.

Examination: Membranes somewhat dull and indrawn. Cannot hear high tones; d.v. 1024, and above, not perceived by air or bone conduction. Hears d.v. 32. Cold caloric test: vestibular part of eighth nerve functions normally for all canals. The condition is symmetrical.

DISCUSSION.

Mr. JUST added that the man said the noise in his engineering shop was not loud, and that previously to his working in that shop he had been unable to hear birds singing and people whistling.

Sir JAMES DUNDAS-GRANT thought there was probably an infection of the lowest turn of the cochlea. The absence of a history of an aural discharge and the normal functioning of the vestibular part would be rather against the congenital idea.

Mr. SYDNEY SCOTT said he had watched a patient with this form of deafness; she was now aged 19. From the first she could not hear the monochord, or high whistles, and the low tone limit was raised to 36 d.v. There was no suppuration, and she had never been exposed to loud noises. Tonsils and adenoids were removed, and the low-tone limit extended down to 16 d.v. But the high tones remained inaudible, yet she heard conversation easily. It appeared to be a case of contracted auditory field, the anatomical explanation of which he would like to ascertain.

Mr. W. M. MOLLISON said he had seen two cases with loss of upper-tone perception. In one the patient had never heard the whistling of birds, nor had her brothers; it was familial deafness. The second case was that of a girl, 17 years of age. She was not deaf to ordinary conversation or a whisper, but she could not hear a monochord note at all. The father's deafness was exactly similar, though of higher degree.

Dr. J. KERR LOVE (President) recalled the case of a friend of the late Professor Tindall, who was deaf to insect music in the Alps in summer. He came across another case, when testing for fine differences of pitch. One man, in a choir, was able to differentiate 1/80th to 1/64th of a semitone, but was deaf to pitches of 3,000 and 4,000 vibrations and upwards. Was there in this case an association between the attack of scarlet fever and the onset of the deafness, as there was a scar in the membrane? This man admitted that he had become more deaf since he had worked in the engineer's shop for five years. There might be a congenital condition in association with an acquired element.

Sir WILLIAM MILLIGAN said that as there was no history of discharge when the patient had scarlet fever, one had to fall back on the congenital idea. There might have been a limited toxic labyrinthitis. He agreed as to the possibility of deafness becoming worse as a result of working in an engineering shop.

Mr. F. J. CLEMINSON suggested that other members of the family should be tested.

Mr. COURTENAY MASON said it had been shown experimentally that people who had become deaf as a result of working in a noise of a particular pitch had a deafness which affected certain parts of the labyrinth, sensitive to that pitch. He thought that this boy's deafness was due to otitis media suppurativa in childhood.

Mr. A. J. M. WRIGHT asked whether there was any real evidence that blocking of the meatus, by cotton wool or other means, was an efficient safeguard to people who were exposed to continual noises of high pitch.

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Mr. J. F. O'MALLEY said he thought men in engineering shops derived harm from the vibrations as well as from the actual noises; and if they could be insulated from contact with the vibrating surfaces it would be beneficial. He thought that the patient had suffered from a toxic labyrinthitis at the time of scarlet fever. It was known that specific fevers would cause it, and it was on all fours with toxic manifestations in connexion with streptococcal septic foci. Such conditions as irido-cyclitis, pancreatitis, chronic arthritis, and mucous colitis, were often traceable to infection of the blood-stream by some septic focus, and it was not unreasonable to suppose the labyrinth could be affected in that way.

Dr. J. KERR LOVE (President) said the late Dr. Thomas Barr investigated boiler-maker's deafness as he found it on the Clyde, and said it was always present after eight years of such work. But he (the speaker) found it came on earlier, after five years. Men had admitted their power of hearing was reduced before eight years' work. The fact was, ordinary people had so much reserve hearing capacity that a slight reduction passed unnoticed. Fifty per cent. loss of hearing was compatible with ordinary requirements. It was difficult to exclude boiler-makers' deafness in the present case.

Mr. JUST (in reply) said that he considered this a case of congenital deafness, which had been rendered worse by the occupation. If a localized labyrinthitis was answerable, following on infection, it was strange that the condition was so symmetrical. He would, as suggested, try to test others in the family.

Chronic Suppurative Otitis Media ; Left Temporo-sphenoidal Abscess ; Naming Aphasia.

By T. H. JUST, F.R.C.S.

A. B., MALE, aged 36.

History.—Seven years ago, left otitis media. Discharge ceased after some time. December 23, 1923 : Tonsillitis, followed by pain and discharge from left ear. January 19, 1924 : Left facial paralysis ; no rigors.

Examination.—Left tympanum filled with granulations ; much pus in left ear ; reflexes normal.

Treatment.—January 23 : Radical mastoid operation : extensive infection of bone ; dura mater of middle fossa healthy ; cholesteatoma. January 25 : Naming aphasia (could recognize objects but could not recall the name) ; nystagmus to right and left ; left optic disc hazy ; no headache ; pulse-rate never below 68 ; left temporo-sphenoidal lobe explored ; no abscess found. February 1 : Weakness of right grip ; slight hiccough. February 3 : Paralysis of right arm and right leg ; incontinence of *fæces* and urine ; becoming more drowsy ; hiccough incessant ; could not be roused.

Operation.—More bone removed over left temporo-sphenoidal lobe ; brain explored and a small abscess the size of a walnut found anteriorly in the lobe ; tube-drained.

Condition steadily improved.

February 11 : Right grip good : paralysis of right leg has disappeared. His mental condition remained altered for seven weeks, in that while he could answer questions relating to the present sensibly, he had complete loss of memory, which suddenly returned.

DISCUSSION.

Dr. J. KERR LOVE (President) asked whether Mr. Just believed that the intracranial condition arose from the recent attack, or from the old attack. It was a chronic type of case, on which the intracranial changes were grafted.

Dr. LOGAN TURNER said it was interesting that there should have been a hemiplegic condition in association with so small an abscess. If there was contralateral paralysis there

was probably either a large abscess, or a meningeal deposition of pus sufficient to produce cortical pressure. The presence of aphasia was also interesting with such a small abscess.

Mr. SYDNEY SCOTT said that Mr. Just was to be congratulated on the result in these cases; he remarked that it was rare for a patient with hemiplegia and incontinence to recover so completely as one of these patients had recovered. Patients who survived a brain abscess, especially cerebral abscess, were always likely to be irresponsible and unsatisfactory afterwards; but he had come to feel more hopeful, and he read a letter from a patient, written fifteen months after a temporo-sphenoidal abscess had been opened, which indicated a high order of intelligence. The patient had been promoted in his work to be manager of an electrical engineering factory since his recovery. Mr. Just's patient also seemed perfectly normal, mentally.

Sir WILLIAM MILLIGAN said that in this case there might have also been some localized meningitis. If it had been an uncomplicated brain abscess the pulse would have been slower than the record showed. He asked what was the experience as to the mental condition of these patients after a year or two. It had been said that the result of opening large temporo-sphenoidal abscesses was that the patient's disposition altered for the worse.

A further question which this case raised was as to the best way of draining a brain abscess. This case was tube-drained and he asked what kind of tube was used, and how often the abscess was washed out; also, how frequently the tube was changed. Had Mr. Just experience in draining these abscesses by means of strands of gauze? He himself had used a double metal tube, one tube within another, and he syringed through the narrower tube, and allowed the material to pass out through the broader one. Even then drainage sometimes failed, as the tube became blocked up with sloughs.

Sir JAMES DUNDAS-GRANT asked whether there was any paresis of the face, as this would enable one to know whether the abscess was in the anterior or in the posterior part of the temporo-sphenoidal lobe. Also was there any sign of affection of the third nerve?

He referred to the case of a patient who had a large temporo-sphenoidal abscess opened by him, and who ever since could be described as somewhat "silly," though she could do her work as a milliner. In that case he put in an india-rubber drainage tube, and shortened it quickly as the brain substance closed up; if there had been a capsule the case would not have taken such an easy course.

Mr. WATKYN-THOMAS said he had kept in touch with some of the patients on whom he operated during the war for wounds of the brain, and the letters from them showed they were mentally in a good condition. Most of the lesions in those cases were cortical or sub-cortical, but in pathological cases the lesions were usually deeper. The abscess in this case was fairly deep, and there might have been direct pressure on the motor tract.

Mr. COURTENAY MASON said that in cases in which the patients recovered their mentality the condition might be regarded as due to extensive encephalitis round the abscess without destruction of cerebral tissue. That might account for the rapid recovery in this and similar cases. He himself had had a similar case, in which it was shown that during the aphasia and mental dullness the patient's ideas were confined to the work he was usually employed at.

Dr. J. KERR LOVE (President) said he had been impressed by the absence of mental symptoms at the time of the operation in the case of large cerebral abscesses, rather than by the presence of mental symptoms during the period of recovery.

Mr. T. H. JUST (in reply) said that when he examined the ear he thought that it had been moist all the time. In regard to symptoms arising from such a small abscess, it was an acute abscess, and he considered that the causation of the paralysis and its quick disappearance was oedema of the anterior part of the temporal lobe, pressing on the motor centres. Drainage caused the subsidence of the oedema. If it had been a low type of meningitis, he would have thought it would have taken longer to return to the normal. There were full visual fields in both eyes. In two out of six similar cases the patients had become irritable afterwards, one so much so that he lost his work through incivility, though healthy and able. The other, a woman, was now very querulous and complained about the smallest trifles. In this patient there was no paralysis on the opposite side of the face. The first sign of paralysis noted was diminished grip on the contralateral side. There was no third nerve paralysis.

Chronic Suppurative Otitis Media; Right Temporo-sphenoidal Abscess; Lateral Sinus Thrombosis; Fistula of External Semicircular Canal.

By T. H. JUST, F.R.C.S.

F. C., MALE, aged 32.

History.—Chronic discharge from both ears for five years; more profuse right. Giddiness eight weeks; more so in dark. Headache. "Chills" for six days.

Examination.—February 11, 1924: Ill; mentally active; discharge from both ears; no facial weakness; right mastoid tenderness; nystagmus, rotary, to left; optic discs normal; grips good; no inco-ordination; gait, staggers to right; no fistula sign; severe headache; rigor.

Treatment.—Radical mastoid operation; cholesteatoma: large excavation of bone; perisinus pus; sinus wall infected and unhealthy, opened up, clot found, sinus obliterated; dura mater of middle fossa apparently healthy; large fistula in the external semicircular canal; cerebro-spinal fluid, opalescent fluid, 20 cells per cm., sterile.

February 13: Violent headache; drowsy; lumbar puncture; cerebro-spinal fluid turbid, 408 cells per cm., largely polymorphonuclears; sterile; temporo-sphenoidal lobe explored, large abscess found, washed and sucked out; rubber tube drain; uninterrupted recovery.

March 5: Plastic operation for closing wound behind ear; in less than twenty-four hours violent headache; lumbar puncture; turbid fluid under pressure; wound opened up; track into brain explored; small quantity of pus let out; wound left wide open.

Recovery uneventful.

DISCUSSION.

Mr. H. V. FORSTER said he would much appreciate hearing experiences as to the drainage of cerebral abscesses. In the temporo-sphenoidal abscesses he noted that the drainage had been easy, but the results had been unfortunate. Two of the cases which came to autopsy appeared to have spreading encephalitis. In the second case under discussion at this meeting lumbar puncture had been performed, and that brought up a point on which Bárány had written in the *Acta Oto-Laryngologica*, in which he dealt with lumbar puncture, ventricle puncture, and callosal puncture in cases of brain abscess, to prevent spreading encephalitis. He (the speaker) gleaned from that article that Bárány advocated treating brain abscesses, after they had been explored, by repeated lumbar puncture, and even by performing ventricular and callosal puncture. It was taught that it was dangerous to do lumbar puncture before a brain abscess was evacuated, but this article advocated such a procedure after the skull had been opened. By callosal puncture he meant puncture of the corpus callosum.

Dr. LOGAN TURNER said he had often thought there were two subjects which the Section might specially discuss with advantage: the first was as to the best way of draining brain abscesses. Various methods had been employed, and all the Members must have abundant material; sometimes different methods were used in the same case. The second subject dealt with the after-history of patients cured of cerebral abscess. He had seen cases of temporo-sphenoidal abscess with convulsive attacks at various periods after the operation. This, and the mental sequelæ, would form a useful topic of discussion.

Mr. SYDNEY SCOTT said he wondered whether other Members had found that left-sided brain abscess was less often fatal, than was an abscess on the right side. If so, he would suggest it was possible to diagnose it at an earlier stage. In the case under discussion there was no neurological localizing sign, except that the suppuration and path of infection had been found to lead into the temporo-sphenoidal lobe from the middle ear, otherwise the abscess might

not have been discovered until too late. As to the method of drainage: though there had been recoveries in cases of brain abscess without use of drainage material after the abscesses had been opened, he felt it was the right procedure to introduce material for drainage. He would not say he had not heard of cases of recovery with gauze drains, rigid or semi-rigid tubes of different calibre, but he certainly felt more confidence in small flexible rubber tubes. He sometimes used a group of as many as six small tubes.

Sir JAMES DUNDAS-GRANT remarked that the appearance of the cerebro-spinal fluid did not always enable one to diagnose between brain abscess and meningitis. The prognosis depended greatly on the sterility or otherwise of the fluid.

Dr. J. KERR LOVE (President) agreed with Dr. Logan Turner's view as to the desirability of having a special discussion on the points he named.

Mr. T. H. JUST (in reply) said he had had no experience of treating brain abscess by lumbar puncture afterwards, and he could not say whether it lessened the chance of encephalitis. His practice in treating these cases was like that of Cushing in the case of head wounds in the war. If there was an abscess he used a No. 10 catheter and syringe, and washed it out and sucked it out with saline until all pus and debris had come away. He had been very lucky with these cases, because in the last eighteen months he had had six cases of temporo-sphenoidal abscess. Five patients survived, and one died of ventriculitis in the third week. Therefore the washing and sucking out of the material could not be a dangerous practice. It was a low grade of infection in a brain abscess. The obstacle to proper drainage was the leaving of some necrotic material in the cavity. He used a medium-sized rubber tube drain, and he kept the tube patent by mopping out with wool on a probe, or by gentle washing out, and leaving the tube in for ten days, until a tract was formed, and then using a smaller tube.

In the present case, within forty-eight hours after the closing up of the wound the patient nearly died; he had violent headache, there was turbid fluid in his theca, and there was a little pus in the tract. In the preceding case no plastic operation was done.

Case of Localized Otitis Media leading to Mastoiditis.

By F. J. CLEMINSON, M.Ch.

Mr. B., AGED 62, a civil servant, was seen first by Mr. Somerville Hastings, and later, in his absence, by myself in August, 1924. He complained of deafness and discomfort in the right ear, and on examination the malleus was seen to be deeply injected, and the postero-superior portion of the membrane was reddened. There was no bulging observed at this or at any other time. The patient was found to have a slight evening temperature, and while under observation right-sided headache and mastoid tenderness developed. On August 26, Schwartze's operation was done. The mastoid was cellular in character and the majority of the cells contained pus. The wound was treated with eusol and remained clean. The aditus persistently failed to close, however, owing to discharge through it from the middle ear. On October 18 a paracentesis of the membrane was done, and fluid syringed into the external meatus passed out into the wound through the aditus. The opening in the membrane remained patent for four days only. Except for the opening into the aditus the posterior wound has now healed.

Treatment: Every day colloidal silver in saline is syringed through from the aditus into the middle ear, and it finds an easy exit by the Eustachian tube. After this a few drops of 10 per cent. argyrol are injected into the middle ear. The discharge from the aditus is rapidly diminishing, and the hearing, which is poor and varies very much, is on the whole improving.

If the present progress continues there should be no further discharge in ten days. During the time he has been under observation the state of the lower half of the drum has been normal.

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DISCUSSION.

Mr. H. V. FORSTER said that he noted improvement in this case to be associated with the use of colloidal silver. He (the speaker) said he had used colloidal silver in cases which healed slowly and discharge persisted after the operation for acute mastoiditis. Colloidal silver had been definitely useful in such cases.

Mr. CLEMINSON (in reply) said that he did not originally do paracentesis as there had been no bulging of the membrane. He had not attached much importance to the colloidal silver in itself; the important point was that he was able to get a flow of fluid through the aditus, the ear and the Eustachian tube.

Postscript (February 28, 1925) :—Within three weeks of the meeting the aditus had closed. The injection of the tympanic membrane has now vanished and the hearing is normal.

Section of Otolaryngology.

President—Dr. J. KERR LOVE.

Kinematograph Film illustrating Anastomosis of Facial Nerve in Cases of Facial Palsy.

Shown by LIONEL COLLEDGE, F.R.C.S., and L. D. BAILEY, M.C., L.R.C.P., M.R.C.S.

Case I.—Girl, aged 17. Operation for suppuration in the right labyrinth and meningitis. The cochlea was full of thick green pus, and the *Staphylococcus aureus* was isolated from the cerebro-spinal fluid. Recovery after six lumbar punctures, but complete right facial palsy remained. The reaction of degeneration was still complete at the end of six months, with no evidence of recovery. The facial nerve was anastomosed end to end with the whole of the central end of the hypoglossal. The peripheral end of the hypoglossal was implanted laterally into the spinal accessory. Wasting of the tongue is scarcely perceptible, but there is slight wasting of the sterno-mastoid. Faradic response is present in all the facial muscles. Film shows state of face three years after nerve anastomosis.

Case II.—Man, aged 27, had a radical mastoid operation on the right ear in India two years previously. This resulted in a complete facial palsy. When seen the mastoid cavity was still suppurating and the face completely paralysed. A number of unopened cells were obliterated and the cavity grafted. When healing was complete the facial nerve was anastomosed to the whole of the central end of the hypoglossal. The descendens noni was divided and the central end was anastomosed to the peripheral end of the hypoglossal; the peripheral end of the descendens noni was implanted into the side of this junction. Faradic response present in all facial muscles. Film shows the state of the face after fifteen months.

Case III.—A woman, aged 56, had a radical mastoid operation in a suburban hospital. The cavity was still suppurating and the right side of the face completely paralysed, when seen. The mastoid operation was repeated and the cavity grafted. Four months later the face showed no sign of commencing recovery. The facial nerve was anastomosed to the central end of descendens noni. The peripheral end of the descendens noni was implanted into a slit in the side of the hypoglossal. Faradic recovery of all facial muscles. Film shows state of face after twelve months.

Discussion.—Sir CHARLES BALLANCE said that the admirable description of the films by Dr. Bailey and the exposition of the subject by Mr. Colledge left him (Sir Charles) little to say. The descendens noni was used to prevent atrophy of the tongue; in some cases the whole hypoglossal was anastomosed end to end to the facial nerve, but this did not prevent the tongue from showing some atrophy. Expressed otherwise, the descendens noni had been shown to be not sufficient for the maintenance of the nutrition of the tongue. He suspected the same to be true in cases in which the descendens noni had been used to bring about recovery from the facial palsy. It was a small nerve, which came from the first and second cervical. He did not think it would be found ultimately that the descendens noni was the nerve to use for the recovery of facial palsy; but all operations for the treatment of facial palsy were still in the stage of evolution, and, by every case of anastomosis done, one was brought nearer to the final conclusion as to what it was best to do. The glosso-pharyngeal seemed the best nerve in monkeys to use for the recovery of facial palsy; it was suggested to him in 1895 by Professor Sir Edward Schafer. At that time he thought that gentleman's view was wrong; that the nerve was a very small and deeply situated nerve, and not the best to use. But he (Sir Charles) was wrong. The first operation of facial-hypoglossal anastomosis was done by Professor Körte, and three months later he (the speaker) himself did one. Mr. Colledge and

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he used the lingual nerve and got apparently complete recovery in an Abyssinian trotting monkey.

Dr. W. S. SYME said that three years ago the late Mr. Kennedy had performed for him the operation of facial-hypoglossal anastomosis on a boy. After two years there was satisfactory return of power. Shortly before Mr. Kennedy died he had seen the boy, and had considered the outlook very hopeful.

Mr. SYDNEY SCOTT said that it seemed that the experimental stage had been passed in regard to these operations and Members would look forward to their considered opinions as to the best method of dealing with facial paralysis. It might be that one operation was more suitable for certain individuals than for others: for a coal-heaver one operation might be chosen, and another for a lady who had leisure to practise facial movements and exercises before a mirror for a long time afterwards, to achieve such excellent results as were seen in the patient Sir Charles Ballance had brought to the meeting. The result of operation was incomplete unless re-education of facial control followed. It would be of interest to know what was the duration of the facial paralysis in each of the cases operated upon, and at what period there was justification for feeling hopeless as to spontaneous recovery taking place. He had seen a patient in whom there was no sign of recovery until three years after the onset of facial palsy. She eventually recovered under Dr. Kinnier Wilson's care without any operative intervention being directed to the nerves.

His late colleague, Mr. West, adopted another procedure in a case of a man with facial paralysis resulting from an operation he (Mr. Scott) had performed in August, 1915, for malignant disease of the middle ear. He (Mr. Scott) had removed the facial nerve deliberately, with most of the petrous bone. Mr. West removed the sterno-mastoid and the glands in the neck in May, 1916, and in August, 1921, tried to improve the patient's facial appearance by bringing down strands of the temporal muscle and attaching them to the angle of the mouth. The result was that the patient could move the angle of the mouth on the side previously paralysed. This patient was later shown at the Section of Laryngology by Mr. Harmer.¹ In this original report the deliberate removal of a part of the facial nerve with the labyrinth, mastoid and tympanic plate was not mentioned.

Mr. G. J. JENKINS asked which part of the spinal accessory nerve Mr. Colledge took; just before going into the muscle it divided into two main trunks, upper and lower. It was possible, he thought, to get a bundle of fibres going to the trapezius which would be useful, leaving the sterno-mastoid alone. With regard to the descendens hypoglossi, one knew of the great variation in its size in different subjects; sometimes it was absent, while at other times a large element in the ascending portion was absent. It was necessary to consider what should be done when the nerve was too small to be used for facial anastomosis. Was there any reason why both ends of the descendens noni should not be used? The two together might form a bundle which would be sufficient to restore the facial nerve.

Dr. ALBERT A. GRAY asked, with regard to the lingual and glosso-pharyngeal anastomosis, whether there was any sensory result from anastomosing a motor nerve with a sensory or secretory one. Was there, for example, any effect on the secretory glands of the mouth?

Mr. COLLEDGE (in reply) said the descendens noni varied a good deal in different persons. In the woman depicted on the screen this nerve was fairly large, nearly as large as the trunk of the facial nerve. He repeated the operation in another case and there was a greater difference in size, the descendens being decidedly smaller than the facial. With regard to Mr. Jenkins' suggestion that both ends should be used, Sir Charles Ballance did that in one of his experiments on monkeys, but he (Mr. Colledge) thought that if it were attempted in the case of the human being it would be found difficult to bring both ends up to the facial, because the human neck was much longer than that of the monkey.

In reply to Mr. Jenkins' question concerning the spinal accessory, he simply nicked it and implanted the hypoglossal into the side. There was a little wasting of the sterno-mastoid. He would not again employ the spinal accessory.

In answer to Dr. Gray's question as to sensory changes after anastomosis with the lingual, the operation concerned had not been done on the human subject, and it was difficult to make out anything very definite about it. Any success which had followed these operations was very largely due to the unremitting care of Dr. Bailey, and his attention was a large factor in the recovery of the patients.

¹ *Proceedings*, 1922, xv (Sect. Laryng.), p. 33.

Septicæmia and Acute Infection of the Middle-ear Tract.

By G. J. JENKINS, O.B.E., F.R.C.S.

I HOPE that the title of this communication will not suggest discussion on what is known by the term "septicæmia." Pathologically, the term implies an infection of the blood. Clinically, it implies a general infection, varying in degrees of severity from a mild disease with apparently inoffensive symptoms, to a disastrous condition with a fatal termination. The only absolute proof of the existence of true septicæmia is the finding of organisms in the blood. Positive evidence of their presence, even with the most careful methods of culture, is so often lacking in patients who, clinically, are suffering from septicæmia, that I use the term "septicæmic state" until true septicæmia has been proved to exist. I have only met two groups of organisms in true septicæmia, viz., streptococci and pneumococci. I am inclined to think that the former cause a more serious infection, though I have seen recovery in infections from either group. Before this Section we have had for many years past interesting papers and descriptions of cases of intracranial complication of middle-ear infection, but I think it is comparatively rare to find any mention of septicæmia at these meetings. It seems to me that this shows a neglect of an important part of our work, for the following reasons:—

(1) In my experience septicæmia, or the septicæmic state, is almost as common in acute middle-ear infection as in any single intracranial complication.

(2) There is a real danger of failure to recognize the middle-ear infection in the early stages of septicæmia, when something might be done to save the patient.

(3) Septicæmia is probably more common with infections of the ear, nose, throat (especially the ear), than with those of any other part of the body, including the pelvis.

The senior surgeons of our Section have often remarked on the rarity of intracranial complications of middle-ear disease at the present day, in contrast to their frequency in the early days of the speciality. These intracranial complications are becoming less and less common, but I venture to say that septicæmia in association with aural sepsis is less likely to disappear, as this is a condition that may arise in the very early stages of the infection.

I do not propose to place before you a list of cases, but rather to give you a synopsis of my personal experiences. I shall not enter into detailed descriptions of the clinical features of septicæmia, as these are well known to you. Septicæmia, or the septicæmic state, is much more likely to be met with in children than in adults, and I think more commonly in patients who have not previously had a gross lesion of the ear.

The description of the following case will serve as an example of the milder type of "septicæmic state":—

Female, P. E., aged 12. Admitted to King's College Hospital, August 18, 1924. Six days before admission had pain in right ear, the following day slight pain in left ear. Some discharge from right ear that had ceased the day before admission.

On admission: Temperature 103° F., pulse 92. No pain in left ear, and pain in right ear had practically disappeared. There was doubtful tenderness over the right mastoid (patient had been frequently examined), no tenderness over left mastoid, no periosteal thickening over mastoid on either side. Right ear: Small perforation of postero-inferior part of tympanic membrane; perforation patent and margin pale; slight injection of upper part of margin of membrane; faint injection of posterior walls of meatus visible, but no swelling; moist external auditory meatus (probably slight discharge). Left ear: Slight injection of superior margin of membrane; meatus dry, no meatal swelling. Beyond some degree of middle-ear deafness nothing more found in either ear. Patient's doctor and her relations had noticed something unusual in her general condition in the last few days, and the high temperature and listlessness of the patient suggested a more or less serious illness. So convinced was I that the ear condition was not sufficient to account for the general condition, that I hesitated to operate and expressed the opinion that something else might be causative.

Nothing else could be found, and the history of the trouble in the right ear suggested that the cause was there. It then occurred to me that this might be a case of septicæmia secondary to or associated with infection of the middle-ear tract. I had a cytological blood-examination made, and when the pathologist reported only 6,000 leucocytes, I decided to operate on both mastoids.

Operation.—Both sides very cellular, cells extending external to lateral sinuses. Thick "laked" pus seen in all the cells on both sides. Mucous membrane lining the cells thin and pale, almost normal in appearance.

Clinical Progress.—For some two days there was an improvement in the temperature, but this always rose in the evening to 100° or 101° F.

General condition not satisfactory. After a few days the temperature again reached 103°. No signs of labyrinthine or intracranial inflammation; nothing found on examination of the wounds under an anæsthetic; the dura and lateral sinuses appeared normal on both sides. Patient had complained of vague pain in the right kidney region, but nothing definite beyond some tenderness was there discovered.

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Urine normal. White cell count about 8,000, phagocytic index 0.5. Immuno-blood transfusion given, 500 c.c. being injected; next evening the leucocyte count was 16,000, and the following day 25,000. Pain in region of right kidney disappeared. Temperature reached normal in two days and after that remained normal. Blood-culture had not been done.

This case is more or less typical of many which, clinically, appear to be a mild type of septicæmia.

When septicæmia appears the symptoms and signs of inflammation at the primary site of infection are profoundly modified. It is safe to presume that in the above case, before the immunity was lowered and the septicæmic state came on, there must have been considerable pain in both ears, as well as some tenderness and some degree of periosteal thickening over both mastoids; there must also have been marked congestion of both tympanic membranes, and probably also swelling and redness of both meatal walls.

The symptoms and signs of active local inflammation had completely disappeared with the onset of the septicæmic state.

When the primary lesion is in the middle-ear tract then the clinical appearances will depend on the stage reached by the inflammation and on the anatomical formation of the mastoid. When the incidence of the septicæmia occurs in the early stage of infection of the middle-ear tract, there may be no obvious local signs of inflammation, though in a patient whom I saw recently there was a pale, slightly yellow tympanic membrane, which showed some bulging, and although there was no injection of the drum, yet the middle ear contained pus.

If the time of incidence is in a later stage of inflammation, as in the case of the child P. E., the appearances may be those of chronic suppurative middle-ear disease.

I have been fortunate or unfortunate in seeing cases in which the periosteal thickening and meatal swelling could be recognized, but the discharge and injection had disappeared. So completely does the septicæmic state mask the local signs in some instances, that even a skilled otologist may have difficulty in recognizing a serious local lesion, and those less experienced are likely to fail completely. Sudden cessation of pain and discharge, together with obvious alteration in the patient's general condition, should direct the attention of the observer to the ears for careful examination.

In the case described, the appearance of the interior of the mastoid was characteristic. The "laking" of the pus in the big cells, the thin, pale mucous membrane lining the cells, almost normal in appearance, the absence of congestion, are characteristic features of mastoiditis followed by a septicæmic state. On some occasions such an appearance at operation has suggested to me a diminished resistance, and this has been confirmed by a low leucocyte count. It is easily understood that this characteristic appearance may be the first sign to suggest the presence of a septicæmic state, as the general symptoms may have been attributed at first to the mastoiditis.

The Clinical Progress after Operation.—The general symptoms predominate. There is no evidence of reaction in the wound, which has the appearance of an operation done post mortem. A smear from such a wound may show a few lymphocytes and polymorphonuclear cells and a large number of organisms. I remember one case in which such a smear had almost the appearance of having been taken from a pure culture.

When a certain degree of immunity has been established, a certain degree of reaction in the wound may immediately be detected.

It is interesting to note that in some cases of septicæmia, especially those of pneumococcal infection, granulation tissue may form. The stage at which immunity is re-established is not without its dangers, as at this period there may be regions of localized infection which, though often they are not of serious importance, may sometimes go on to abscess formation.

Treatment.—It seems to me all important that the middle-ear condition should be thoroughly dealt with. In the majority of instances this amounts to a Schwartze operation and paracentesis. Then comes the question of the treatment of the general infection.

Until recently it has been my custom to give these patients large doses of normal horse serum as soon as possible, and to continue this daily for as long as may be necessary, but lately, as in the case above described, I have been trying immuno-transfusion. I am inclined to be favourably impressed with this procedure, but there are others more qualified to give an opinion on this matter than I am. Of the value of vaccine therapy I can say little. My own experience has been unfortunate, but possibly some of you will be able to encourage us to try this line a little further.

I have said nothing about the serious cases of septicæmia. The clinical aspect of these is

similar to that obtained in cases of milder degree, both as regards the local signs and symptoms, and as to the general symptoms. Their progress is, however, much more rapid, and treatment seems to have little or no effect, at any rate when once the disease is well established. It is possible that immuno-transfusion, or some such procedure, may yield better results than have been obtained by other methods.

I suggest the following points for discussion:—

(1) *What are the means of distinguishing a severe toxæmia from septicæmia?* In my previous remarks I have attached a good deal of importance to the cytological examination of the blood, the leucocyte and differential counts. A low white count, and a high proportion of polymorphonuclear cells have appeared to me to be important factors in diagnosing a low immunity and a septicæmic state. I have, however, seen cases with comparatively high counts (e.g., 20,000) when pneumococci were found to be present in the blood.

(2) *What is the value of the phagocytic index in the diagnosis and treatment of septicæmia?* I consider the value of bacteriological examination of the blood, when the result is negative, in cases where septicæmia is suspected, to be very low.

(3) *Has any one proved the value of bacteriological examination of other fluids, such as the urine?* So far I have hesitated to draw off cerebro-spinal fluid for such purpose, being afraid of injuring the cerebro-spinal system, and so localizing the infection.

(4) *Has anyone had experience of methods that have been suggested for localizing the infection in regions of small danger?*

It may seem that I have attached rather too much importance to the recognition of milder cases of the septicæmic state, which may be in some cases only instances of a rather low immunity. I think, however, that it is by training ourselves to the identification of these slighter degrees that we become better prepared to recognize the more serious forms in an early stage. Moreover, some of these milder affections, if left untreated, are apt to become more serious and end fatally, though it is admitted that the great majority of them recover without any special treatment.

One of the objects of this communication is once more to draw the attention of the general physicians to the fact that when a patient is in this septicæmic state, the ears, nose and throat should be carefully examined, that they should bear in mind the effect of the general infection on the appearance of the primary lesion, and that full value should be given to a history of recent trouble in these parts.

Discussion.—Dr. W. T. GARDINER said that cases of this kind occurred more frequently than one might believe; the patients were usually children. He had had five such cases. With the beginning of the otitis the septicæmic picture commenced. There was so little local sign that it was difficult to think the ear accounted for the condition. Operation was disappointing, as it did not arrest the septicæmia. In his first case there was a blood-stained discharge for eight days. It was possible to perforate the mastoid region too early. In eight days the discharge was purulent, and as the high temperature still persisted he opened the mastoid, but it did not bring the temperature down. At the end of fourteen days he thought the patient had sinus thrombosis. He therefore opened the sinus and the jugular vein—the findings were negative. Still the high temperature persisted. He then administered 50 c.c. of antistreptococcic serum intramuscularly on three occasions and followed up this by giving autogenous vaccine. Eventually the temperature came down and the patient recovered, and so did all five of his cases. If he had another case he would try immuno-transfusion. Had Mr. Jenkins done a differential blood-count in his first case? A leucocytosis of 6,000 with a high polymorphonuclear count would show a definite infection, yet a poor resistance on the part of the patient. This fact about ear cases was first pointed out by Dr. J. M. Darling of Edinburgh.

Dr. NEIL MACLAY said that the type of case described had perplexed him a good deal. He had had two cases of the kind, both in young people, and his difficulty was to know what he was dealing with. He had spent much time in looking for some intracranial complication.

The patient in the first case, a boy, had two slight shivering attacks, and though there was no ear discharge he had inflammatory signs in his tympanic membrane, and there was mastoid tenderness. Operation showed that the mastoid area contained pus. Paracentesis made little difference to the temperature, which was 104°, and the boy looked ill, though he had been unwell only four days. His lateral sinus was explored, with a negative result. The cerebro-spinal fluid was also negative. After waiting three more days a specimen of blood gave a luxuriant growth of streptococci. He did not know how the question of septicæmia could be decided unless the blood was examined. The case ended fatally, though anti-streptococcic serum was injected subcutaneously into the veins every day.

These cases were very apt to be missed and often ended fatally; he was surprised that Dr. Gardiner's experience had been so good.

Mr. A. R. TWEEDIE said the lesson to be derived from Mr. Jenkins' paper was that in these cases at the time of operation we should adopt as a routine measure the cultural and cytological examination of the blood more often than was apparently done at present. He (Mr. Tweedie) had reason to regret not having done so, but he intended not to omit this precautionary investigation in future.

Dr. W. S. SYME said that he had had cases of acute middle-ear suppuration in which a septic temperature had persisted a long time, and he had not been able to convince himself that he would be doing right in opening the mastoid. His experience was that the patients in these cases, after having caused a great deal of anxiety, got better. There were cases in which he had wrongly attributed the symptoms to some other condition. Examination of the blood, and especially a differential count, were very important.

Dr. J. KERR LOVE (President) said he discouraged the attitude of being afraid to open the mastoid for the purpose of exploration. There was very little risk attaching to a Schwartze operation, and in cases such as those that had been related free drainage was important.

He said that on Christmas Day he had had to operate on a lady whose mastoid he had opened a fortnight earlier, but found no pus pent up in the antrum, though there was some in the middle cells. There was good drainage through the aditus and through the perforation. After being in the home a fortnight, she came back on Christmas Day with a high temperature and a history of a single rigor, and on the strength of that he opened up the lateral sinus, but there was no clot there. Still, he ablated the sinus, and in forty-eight hours she was free from fever. He believed there had been commencing septicæmia, and if he had not acted promptly he might have had to deal with thrombosis of the lateral sinus. The case showed the importance of opening such in an early stage.

Mr. JENKINS (in reply) agreed that a differential count was important, but that a simple white-cell count was valuable. He said he had set out to emphasize the extraordinary change which occurred in the local condition when a general infection supervened. A patient might have much periosteal thickening and swelling of the posterior wall, with a profuse discharge from the ear, and when septicæmia was set up those signs almost disappeared, so that there might be a difficulty in recognizing that this had been the primary site of infection. If the infection was recognized in the early stage of acute middle-ear infection, there might be only slight local signs. One patient gave him much anxiety. He had been ill only a few days; there had been considerable pain in both ears. The child had been awake all night. The tympanic membrane was pale, and there was pus behind it, yet there was no congestion, and there was then no pain. The blood gave 8,000 leucocytes. The early stages of this septicæmia must be recognized if these cases were to be successfully dealt with. A doctor called to a case with high temperature, in the septicæmic state, and not on the look-out for the masking of signs and symptoms of the local trouble, was likely to miss the nature of the case.

Specimen and Slides of a Case of Malignant Disease (Endothelioma) of External Auditory Meatus and Middle Ear with Complete Involvement of Peritoneum.

Shown by F. J. CLEMINSON, M.Ch.

MISS M., aged 60, sent by Dr. Lauchlan. First seen December 9, 1924, complaining of discharge from right ear, swelling in the right side of the neck, and great pain, especially at night. The external auditory meatus shows an irregularly granular surface which bleeds easily on being touched and is covered with pus, reaching on the anterior wall as far outwards as the base of the tragus. Below and in front of the ear a mass of glands can be felt, which have apparently undergone some softening.

No signs of lesion in the cranial nerves, except a slight weakening of the muscles closing the right eye. Bone conduction on right diminished by ten seconds. Bone conduction on left normal. To turning tests, both external horizontal canals react normally. Patient seems very ill and weak.

The patient eventually consented to go into the Middlesex Hospital for observation and treatment, and was admitted just before Christmas. A small piece of the

growing edge of the tumour was removed (section shown), and was reported by the pathologist to be malignant. On January 7, 200 mgm. of radium were placed in the external auditory meatus. The patient died the same night.

Specimen and slides are shown.

NOTES ON POST-MORTEM BY DR. S. L. BAKER, MIDDLESEX HOSPITAL.

The external auditory opening passes into a cavity about 1 in. in depth from surface and $1\frac{1}{2}$ in. antero-posteriorly, occupying the position of the external auditory meatus and middle ear, and encroaching on the squama of the temporal bone, an area of which is eroded so as to leave nothing but dura covering the lateral part of the right temporal lobe of brain. A small area of dura in this situation showed a pale thickening about $\frac{1}{2}$ in. in diameter, suggestive of invasion by growth. The lower boundary of the main cavity communicated with irregular spaces lying deep in the sterno-mastoid, extending two-thirds of the way to the clavicle. These spaces were surrounded by fibrotic tissue showing bands of muscle in walls, and contain broken-down debris and yellowish fluid, apparently formed from disintegration of cervical glands. Several pale, firm cervical glands are in relation to the wall of the cavity.

Remainder of the body, including nasal sinuses and chest, are entirely negative, except for the peritoneum. The surface of the peritoneum is universally studded with minute nodules of growth, in part forming confluent patches, but there is no evidence of any primary abdominal growth.

(Signed) S. L. BAKER.

It was thought that the disease was clinically carcinoma, but the slides show it to be endothelioma. It seems to have been a case of primary malignant disease of the middle ear or external auditory meatus, with secondary deposits in the abdomen. Though the pathologists at the Middlesex Hospital spent two hours looking for metastases in the chest, they could find nothing.

Special Swab-holders for use in Tonsillectomy.

Shown by A. R. TWEEDIE, F.R.C.S.

By the use of the pair of instruments now exhibited, it has been found that the bleeding from the sites of the tonsils can be easily controlled during the operation—thus affording a bloodless field for removal of the second tonsil, and also enabling one to obtain an uninterrupted view of the pharynx (after both tonsils have been enucleated) prior to removal of the adenoids. These swab-holders have been made for me by Messrs. Down Bros.

Dr. J. KERR LOVE (President) mentioned a tonsil-compressor he had been using; this he promised to show at the next meeting of the Section. He said that Mr. Tweedie's instrument required more assistance when being used.

A Modification of Bartel's Spectacles.

Shown by A. R. TWEEDIE, F.R.C.S.

THESE spectacles represent a modification of the device originated by Dr. Bartel of Dortmund. They consist essentially of + 20 lenses (with the whole convexity on the outer surface so as to permit close approximation to the eyes) mounted in elaborated motor-goggles.

Dr. Bartel's idea is that, with the use of these, the finest traces of nystagmus can be more easily recognized by the observer—whilst the element of volitional

control of nystagmus by the patient is also eliminated, as the high + lens prevents any "fixation."

It is suggested these spectacles, as modified, are of distinct value. They have been made by Messrs. Hamblin, of London.

Case of Keloid of Mastoid.

By E. D. D. DAVIS, F.R.C.S.

PATIENT, a boy, aged 13.

History.—Sent by surgeon for treatment of suppuration of ear. The keloid has been excised three times, and skin grafted. Fibrolysin, six months. Treated by X-ray and radium; he has visited most of the London hospitals. Suggestions as to treatment are asked for. The mastoid cavity suppurates occasionally, and it is proposed to reopen this area and excise the keloid.

Mr. T. J. FAULDER said that a good deal of treatment had been tried in this case. The patient had been to several London hospitals, but without good effect resulting. Some years ago, after a radical mastoid operation had been performed on a man aged 20, a keloid developed in the scar in the form of a row of nodules, two being more prominent than the others. He treated it by diathermy puncture, and it disappeared, and there was no recurrence after the healing. He would try to ascertain whether the cure has been permanent.

Section of Otology.

President—Dr. J. KERR LOVE.

Intercranial Infection by Erosion from Subperiosteal Abscess.

By T. RITCHIE RODGER, M.D., F.R.C.S.E. (Hull).

THIS paper, which will appear in full in an early number of the *Journal of Laryngology and Otolology*, deals with four cases, three of brain abscess (two otitic and one frontal sinus in origin) and one of extradural suppuration. The route of infection in each case was not, as is usual, through the inner table of the skull separating the infected mastoid cell from the meninges, but by formation of a subperiosteal abscess, with tracking for some distance beyond the limits of the infected cell, and re-entry through the whole thickness of the skull.

In each case no evidence of gross erosion of the skull over the infected mastoid cell was demonstrable, and this may be the determining factor. Where a subperiosteal abscess forms by gross erosion the pressure of the pus under the periosteum is never high, as there is a free exchange of pressure between it and the infected cell, and the latter is generally constantly relieved by discharge into the ear or nose. When a subperiosteal abscess occurs by spread through the venous or lymphatic channels of the periosteum without gross erosion of the bone, the pressure is high, and we have present the condition conducive to erosion at another site, viz., pus under pressure acting on bone already denuded of its periosteal blood-supply.

Case I.—Temporo-sphenoidal abscess communicating with an erosion high up on the squama, the mastoid cells showing only the remains of an inflammatory condition, and the tegmen antri and adjacent squama being intact.

Case II.—Extradural suppuration through a large erosion of the occipital bone due to tracking backward of a subperiosteal mastoid abscess.

Case III.—Temporo-sphenoidal abscess with a stalk under the portion of squama covered by temporal muscle, well in front of the auditory meatus, a subperiosteal abscess tracking forwards in this region having been evacuated at an earlier operation.

Case IV.—Frontal lobe abscess evacuated through an erosion in the orbital roof 1 cm. external to the outermost limit of the infected frontal sinus.

Discussion.—Dr. J. KERR LOVE (President) said one might think that with involvement of the mastoid process, it was not necessary to look far outside for effects of infection, but Dr. Rodger had now reported a series of cases in which infection had taken place over a much wider field.

Mr. G. J. JENKINS asked whether the author had considered the other possible channel of infection than that he had described. A track of infection through the bone might apparently disappear. Cases were met with of extradural abscess of the posterior fossa, secondary to acute middle-ear infection, in which the mastoid at operation was apparently normal.

Sir JAMES DUNDAS-GRANT said there were two classes of cases resembling those described. There was: (1) The extradural abscess in which erosion had taken place through the bone, which was to be detected by the fact that there was œdema and bogginess

at a much higher level than in simple erosion from the mastoid antrum. But in such cases there had generally been an evident discharge from the ear, and there was a sort of "shirt-stud" bulging, externally and internally. These cases had been described by Hartmann many years ago, but the mode of development of the ulceration of the bone was much more evident than in Dr. Rodger's cases. (2) A type of subperiosteal abscess of the temporal region was described by Gruber in great detail, and the description was to be found in Law and Jewell's translation of Gruber's book. That description had apparently been forgotten, as some years ago Luc, at Boston, brought forward cases of the kind which were thought by him to be new. There was apparently no connexion with the mastoid cells. Gruber's rule was that if the pointing was above the level of the meatus, it was probably subperiosteal, and, whatever opening was made externally, a counter-opening had to be made in the roof of the cartilaginous meatus. He (Sir James) had seen several cases in which the disease subsided after the counter-opening had been made. His own experiences of frontal sinuses with erosion of bone at a higher level had been somewhat distressing. In one of the cases a diagnosis of angio-neurotic oedema was wrongly made, and there was perforation through the bone at a higher level than the extension of the frontal sinus, but the result was not as good in the case Dr. Rodger reported. Dr. Rodger had brought forward some considerations of great interest and importance.

Dr. RITCHIE RODGER (in reply) said the patients in these cases had cellular mastoids; he could only deal with the naked-eye appearances of the intervening bone, although nibbling was carried out until healthy bone was reached. There seemed to be no osteitis, and it was justifiable to think that the route was the one he had described. In the frontal sinus case osteitis of the diploë could not have been the route, as there were no cellular spaces in the thin orbital roof.

Otogenic Pterygo-maxillary Abscess.

By DAN MCKENZIE, M.D.

ATTENTION has already been drawn to this condition;¹ since the last record two additional cases have come under my notice.

In the first the pterygoid abscess was merely a detail in a severe widespread infection, which terminated fatally.

The second case was that of a woman, aged 29, to whom I was summoned on July 6, 1923, for a mastoid operation. The acute symptoms had appeared shortly after the (left) ear had been syringed for the removal of a supposed ceruminous plug, a fortnight previously.

When I saw her there was great pain in and especially in front of the ear, where there was much swelling. Difficulty was experienced in mastication. There was no pain, tenderness, or oedema, in the mastoid region. The walls of the external auditory meatus were so much swollen that suspicion of a furuncle was aroused. The tympanic region could not be seen, but some débris and desquamated epidermis were removed from the deep meatus. The temperature was running at about 100° F.

The patient was anesthetized and it was found that pressure in front of the ear caused a copious flow of pus from the meatus. A probe passed into the meatus led into a large abscess cavity, through a fistula in the bony floor close to the tympanum, and it could be passed as far as the submucosa of the pharynx.

The auricle was reflected forward by the usual post-aural incision, and a large cast of the meatus was removed; it disclosed a tympanic cavity full of granulations. The radical mastoid operation proved the original disease to have been cholesteatoma. The pterygoid abscess was drained successfully by enlarging the fistula in the meatal floor.

Pterygo-maxillary abscess is not common. I have on two occasions successfully drained the abscess through a meatal opening close to the tympanum, but when large it is perhaps most suitably drained by an opening in the pharynx, as this is the lowest point.

¹ *Journ. of Laryng.*, 1913, xxix, p. 309; 1914, xxx, p. 12.

Sequestra of Labyrinth.

By F. W. WATKYN-THOMAS, F.R.C.S.

PATIENT, a girl, aged 18. Old-standing chronic suppurative right otitis media, following scarlet fever. Swelling over right mastoid and temporal. Complete right facial palsy. Complete right deafness. Radical mastoid operation. Cholesteatoma. Sinus deep to the lateral sinus leading to sequestra, which were removed by the Neumann route. Uneventful recovery. Cavity filled by scar. Some facial recovery.

History vague. No recollection of vertigo.

Facial weakness noticed for "some months."

Dr. J. KERR LOVE (President) said he had no doubt the sequestra were from the cochlea and semicircular canal.

Dr. J. KERR LOVE (President) showed his double instrument for tonsillar operations, as a supplement to the instrument shown at the Section's last meeting by Mr. Tweedie.

[May 2, 1925]

The Mastoid Emissary Vein and its Surgical Importance.

By ARTHUR CHEATLE, F.R.C.S.

ANATOMY.

THE mastoid vein is treated with scant courtesy by anatomists and with still less by aural surgeons. Even Politzer does not mention it, except in connexion with "Griesinger's sign" in septic thrombosis of the lateral sinus and extension along the vein. Yet it is of interest as having a distinguished ancestry in being the remains of the primitive jugular vein, and sometimes it is of great surgical importance. In the greater number of specimens the vein is present; is single, and small; originates on the outer edge of the lateral sinus groove just below the bend; has a short course in the substance of the bone in an upward and backward direction; opens on the surface just behind the upper posterior edge of the base of the mastoid process on a level with the roof of the bony auditory meatus, to enter into the occipital or posterior auricular vein, then to form part of the external jugular vein. Being a vestige, it is subject to variation in every particular, and I show you instances which I have met with in my collection, and in the skulls at the Royal College of Surgeons' Museum. I must say at once that the investigation has been made in the canal of the dry bone only, a procedure to which there are some drawbacks, one being that a branch of the occipital artery passes through the same canal, and it may be that when the canal is duplicated, one of these is for that vessel.

Presence.—It is not uncommon for all traces of the vein to be absent bilaterally or unilaterally; I suppose that in the course of time the vein will disappear altogether.

Size.—The canal may be the size of a mere thread, or of a wax match, or from $\frac{1}{8}$ to $\frac{3}{8}$ of an inch in diameter. In two skulls it was nearly $\frac{1}{2}$ inch, and as these cases are of special importance, I shall describe them separately. The veins with a canal of $\frac{1}{2}$ of an inch or more in diameter must certainly make a great difference in the size of the external jugular vein, and it is possible that the canal would show in an X-ray photograph in the living subject. Those of $\frac{1}{4}$ of an inch or more are certainly seen in the empty cleaned dry skull, as can be noted among the photographs shown. The groove for the lateral sinus below the origin of a large mastoid vein appears smaller.

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Number.—It may be single at the origin and double at the exit, or double at origin and exit, or double at origin with three points of exit. In connexion with this the canal for the branch of the occipital artery must be remembered.

Origin.—This may be above the bend of the lateral sinus groove or much lower down, or in the groove itself. The vein may run in a groove before entering the bone. It may be double. There may be a distinct sulcus.

Course.—This is always in the substance of the bone, and although cells may border it above, outside and below, they are never internal to it. In other words, the canal is never surrounded by cells. It may be very short, $\frac{1}{8}$ of an inch, especially if it originates low down, or long, up to $\frac{3}{4}$ of an inch. The direction is often upwards and backwards, but it may be upwards, backwards, downwards and backwards, or downwards. Not infrequently it is curved. It may be translucent to the surface, especially when originating low down.

Exit.—This may be far back, reaching the masto-occipital suture, or even beyond it. It may be low down in the digastric fossa, or above and behind it, or behind it. If double, one may be behind, or below, the other. Sometimes there is a regular sulcus, into which diploic veins, or a superficial vessel, can be seen to enter.

Asymmetry.—This is not at all uncommon. The following instances have been noted:—

- 1 { Double at origin and exit on right side.
Single at origin and exit on left side.
- 2 { Single at origin and exit, and in a groove at origin on right side.
Single at origin and double at exit, and no groove on left side.
- 3 { Small, and running in a groove at origin on right side.
Large and no groove at origin on left side.
- 4 { Small and single on right side.
Large, single at origin, double at exit on left side.
- 5 { Large, with large digastric cell lying against it below on right side.
Mere thread on left side.
- 6 Very large, replacing the lower part of the lateral sinus on the left side only.

Association with the Petro-squamosal Sinus.—There is no constant association, but it is seen here in two specimens. The petro-squamosal sinus groove occurs most frequently without abnormality of the lateral sinus, or of the mastoid vein.

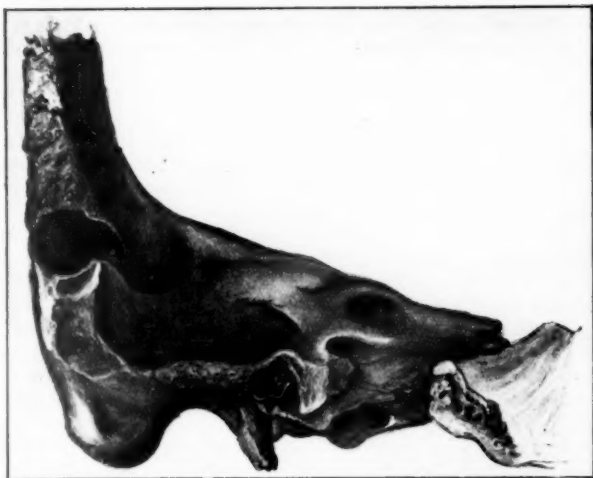
TWO SKULLS IN WHICH THE VEIN TAKES THE PLACE OF THE LOWER PART OF THE LATERAL SINUS:—*On both sides in one, and on the left side only in the other.*

I will now describe the two specimens alluded to before, in which the vein canal measured nearly half an inch in diameter.

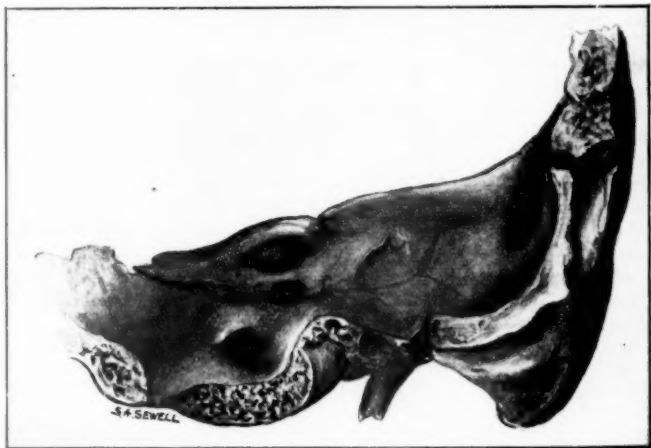
This condition was found in two instances among 1,500 skulls examined in the collection of the Royal College of Surgeons' Museum.

(1) No. 612.1, Royal College of Surgeons' Museum, No. 13 among the specimens shown. It is a French skull in which the back part is broken away. On both sides the lower part of the lateral sinus groove is absent. On the left side the lateral sinus groove, which is larger than that on the right, comes to an end just below the outer end of the posterior border of the temporal bone, and turns backwards to a large mastoid vein canal nearly half an inch in diameter. The same condition is present on the right side, but the lateral sinus groove and vein canal are much smaller, the latter measuring a quarter of an inch in diameter. A deep groove runs downwards superficially for an inch from the exit. This external groove is seen in ungulates. The jugular foramina are small and the fossæ are absent on both sides. There are no other abnormalities.

(2) No. 154.1, Royal College of Surgeons' Museum, No. 13A among the specimens shown. This is an English skull sectioned vertically in half. On the



Left bone.



Right bone.

NO. 612.1.—COLLECTION OF SKULLS. ROYAL COLLEGE OF SURGEONS' MUSEUM.

Drawings of the posterior aspect of the temporal bones from a French skull in which the back part is broken away.

On both sides the lower part of the lateral sinus groove is absent.

On the left side the groove is large and measures nearly $\frac{1}{2}$ in. in diameter; the right side $\frac{1}{4}$ in.

On the right side there is a deep groove running superficially downwards from the exit for an inch, a condition seen also in the skulls of some ungulates.

The jugular foramina are small on both sides, and the jugular fossæ absent.

left side a large lateral sinus groove runs down the posterior surface of the temporal bone, then comes to an abrupt stop half an inch from the jugular foramen, turns outwards, and after forming a deep "elbow" runs backwards to emerge at the masto-occipital suture by an opening nearly half an inch in diameter. A well-marked petro-squamosal sinus groove is also present, but without an external opening. The superior and inferior petrosal sinuses are present. The jugular foramen is small. The jugular fossa is absent. There are no other abnormalities. On the right side the lateral sinus groove is smaller, and is continued to the jugular foramen. A rather large vein canal is present, measuring an eighth of an inch in diameter, originating rather low down and emerging by two openings—one behind the other. The lateral sinus groove is smaller below the vein origin. The jugular bulb is present. There is slight grooving for a petro-squamosal sinus. The only other description of such a condition I have found is one by P. P. Laidlaw, in the *Journal of Anatomy and Physiology*, 1902-3, xxxvii, p. 364. He describes an Egyptian temporal bone in the Cambridge University Museum, in which there is partial absence of the lateral sinus groove associated with the presence of a large mastoid foramen. There is also absence of the internal acoustic meatus and of the stylo-mastoid foramen. The jugular fossa is absent. The superior petrosal sinus groove is present, the inferior absent.

Piersol, in his "Human Anatomy" (1918), makes the following two statements: "The horizontal portion of the left lateral sinus has been observed to be lacking or reduced to an exceedingly fine channel, and one or both of the sinuses have been observed to pass through a greatly enlarged mastoid foramen, to open into the posterior auricular vein, the sigmoid sinus being represented only by a very small channel" (p. 869). In writing of the petro-squamosal sinus, he says that it "represents the original terminal portion of the lateral sinus, the sigmoid portion of that sinus being a secondary formation" (p. 869). It seems that, in some animals at all events, the mastoid vein has a share in emptying the lateral sinus.

In searching for a reason for this abnormality, the following dicta were borne in mind. The first by Wiedersheim in "The Structure of Man," that "the venous system shows unmistakable traces of a very primitive condition inherited from lower vertebrates"; the second, by Cunningham, in his text-book on "Anatomy," that "abnormalities are generally due to the retention of conditions which normally are only transitory in ontogenetic development, or to the acquirement of conditions which, though not as a rule present at any time in man, occur normally in some animals." From my investigations, which are not yet complete, there is no doubt that this abnormality is an atavistic one. One finds a similar condition in the lemur, in dogs, and in ungulates, as you will see in the skulls shown. In all these a large petro-squamosal sinus is also present, the lower part of the lateral sinus is absent, and there is a well-marked mastoid vein canal, especially in the dog and horse. I hope to report further on this matter.

SURGICAL IMPORTANCE.

Injury.—The vein may be opened at its exit in making incisions, or when stripping up the periosteum in various operative procedures, or in its course, in removing bone for extensive cellular infection, or for exploration and treatment of lateral sinus thrombosis, cerebellar abscess or tumour, or in the operation of decompression and in fracture of the skull. As a rule the bleeding is trivial and easily controlled by plugging with gauze or Horsley's wax, but in the larger ones in which we are practically opening the lateral sinus, and actually doing so in a case like the two specimens separately described, it may be troublesome, and I would suggest using a muscle plug. The temporal muscle is always at hand in aural operations.

Sepsis.—The vein may be infected from the lateral sinus and this would appear to be inevitable in the larger ones. In such cases an œdematous swelling or abscess

may form at the exit, constituting "Griesinger's sign" of lateral sinus thrombosis; and as Politzer states, in his only reference to the vein, this sign may be behind the mastoid process, or in the posterior triangle. The specimens shown demonstrate why this varying position of the sign should occur. The vein may become septically thrombosed after an accidental, or incidental, opening, with secondary implication of the lateral sinus. I have heard of such a case occurring after an operation for extensive mastoiditis, necessitating the opening of and shutting off of the lateral sinus. It may be separately and directly septically thrombosed from infected mastoid cells; such a case was described by Dr. Joseph Friedman and Dr. Samuel D. Greenfield, in *The Laryngoscope*, 1923, xxxiii, p. 347. During an operation for acute mastoid infection, the vein was exposed and found to be of great size, $\frac{3}{8}$ in. in diameter, and to be thrombosed without implication of the lateral sinus. It was left unopened, but subsequently the sepsis spread to the lateral sinus, which had to be dealt with. One can imagine sepsis spreading from the vein to the diploic veins, which open into it, causing osteo-myelitis and local inflammatory signs in unexpected parts of the skull. One can also realize that if the sepsis has extended down the occipital and external jugular veins, or if the lateral sinus empties entirely through the mastoid vein, it would be necessary to ligature and excise the external jugular in the same way as the internal jugular is dealt with in lateral sinus thrombosis.

Abnormality of the Groove for the Superior Petrosal Sinus.

An Indian skull sectioned vertically in half, No. 653.2, Royal College of Surgeons' Museum, No. 12A in the specimens shown. Although not germane to the original subject, the abnormality was discovered during the investigation. On the right side a large mastoid vein canal is present, measuring $\frac{1}{4}$ in. in diameter; on the left side the canal is small. On the right side only, a vein canal leaves the upper and anterior aspect of the inner tympanic wall, penetrates the superior surface of the temporal bone at the level of the ampullary end of the superior semicircular canal, then divides into two branches, one running backwards and inwards on the superior surface to reach the posterior edge, when it turns outwards to run on its usual course to the lateral sinus groove, the other branch running inwards to the cavernous sinus.

I wish to offer my sincere thanks to the Council of the Royal College of Surgeons for the use of its material, to Sir Arthur Keith for good help and advice, and to the staff of the Museum.

Discussion.—Dr. DAN MCKENZIE said that unless one practised the dissection of temporal bones for oneself he did not think it was possible to obtain the essential intimate knowledge of the anatomy of this region. The temporal bone was one of the most variable of all human structures.

On one occasion, he (the speaker), when a junior, had had to operate in the country for mastoid suppuration. He made the usual incision and found a cellular mastoid process; he made a horizontal incision further back, and in doing so, he cut into an enormous vessel which yielded a great spurt of blood. He placed his finger on it to stop it. He could not tie it, he had no antiseptic wax with him, and he thought it might be possible to put in an ordinary strip of gauze. So large was the foramen that the gauze went in comfortably, and the bleeding stopped. He wondered whether in such a case the orifice could be stopped safely with soap. He had found that the inside of a cake of ordinary toilet soap was sterile. Subsequently, as he had on a former occasion reported to the Section, this case developed a septic clot in contact with the packing, but the lateral sinus and emissary vein were opened up and the patient recovered.

Dr. LOGAN TURNER said it was a great pleasure to see Mr. Cheatele again exhibiting some of his anatomical work. Specialists in rhinology and otology seemed to have struck a region which was full of abnormalities. Dr. McKenzie's remarks about the variations in the mastoid might be equally applied to the sinuses.

Another point of interest was the possibility of cavernous sinus thrombosis arising through the mastoid emissary vein. He (the speaker) recalled the interesting case of a man with

a carbuncle of the neck, who developed cavernous sinus thrombosis and meningitis. At the post-mortem examination the process of infection was found to have extended from the occipital vein, through the mastoid emissary vein to the lateral sinus, and thence through the superior petrosal to the cavernous sinus. Hence there arose a connexion between the superficial occipital venous system and the cavernous sinus in certain cases which might make primary infection in the neck a possible source of cavernous sinus thrombosis.

Mr. CHEATLE (in reply) said he had forgotten to mention one point, which had reference to what Dr. Logan Turner said about secondary infection of the vein. It was recorded that veins behind the auricle empty into the emissary vein, hence there was always the risk of infection spreading from the surface.

Case of Bilateral Mumps Deafness.

By NICOL RANKIN, M.B.

(Introduced by Dr. DAN MCKENZIE).

K. H., AGED 10, female. About the middle of January, 1925 she had a pyrexial attack during which it is said she had some transient vertigo. This was followed by "ringing" and "thumping" in both ears, and on January 29 she suddenly became deaf in the left ear. Next day she was found to be deaf in the right ear also, and at the same time she was found to be suffering from mumps. No vertigo was experienced at this stage.

Hearing.—Tuning fork (middle C) is not heard at all by air-conduction on either side, and the bone-conduction is minus 30 (approx.).

Vestibular tests (rotation and caloric) produced a normal ocular response, but without any vertigo, although after prolonged rotation the child vomited. No vertigo was noticed during the mumps attack.

Suggestions for treatment are invited. Is pilocarpine of value?

Discussion.—Sir JAMES DUNDAS-GRANT said he wondered whether Members had better success than he with pilocarpine in this class of case.

Dr. KERR LOVE (President) said that this whole group of cases, and the cases of congenital deafness, had one feature in common, namely, that the cochlear part of the eighth nerve was involved, and, as a rule, the vestibular nerve was not. That raised the question of the pathology, as to whether these cases were toxic; whether there was a neuritis, or a meningitis, or, lastly, whether they must be classed as "hysterical."

Sir WILLIAM MILLIGAN said that the prognosis of these cases was bad. The deafness was not always bilateral. He agreed that pilocarpine was practically useless; the only time he had seen it do good was when injected within a few days of the onset of the deafness. It was an interesting speculation as to what caused the extreme deafness in these cases. In some there was probably a localized basal meningitis, the deafness occurring much on the same principle as the profound deafness in cerebro-spinal meningitis. He (the speaker) had not found any treatment of use in these cases.

Mr. RANKIN (in reply) said that the best course to take in this case would be to instruct the child in lip-reading. He had not known of any case in which benefit was derived from pilocarpine.

Case of Sudden Bilateral Nerve Deafness of Unknown Origin in a Child.

By DAN MCKENZIE, M.D.

THE girl is 10 years of age. Two years ago she awoke one morning almost completely deaf in both ears. There was no vertigo and the incident was not associated with any other illness. She had measles when 4 years old, and mumps a year after the deafness appeared. There has never been any ear discharge. Tonsils and

adenoids have been submitted to two operations for removal. The Wassermann is reported to be negative.

Hearing.—She can hear a loud shout in the left ear; not in the right.

	R.	L.
T.F. 250	— ∞	Meatus — 50
	— 10	Mastoid — 15.

Vestibular Tests.—Rotation to both sides induces normal nystagmus and vertigo.

We have since had a Wassermann test performed both of the blood and the cerebro-spinal fluid, and it is negative.

Discussion.—Dr. KERR LOVE (President) said he had had three similar cases. In one, a child aged 5, suddenly, when requested to say her prayers, had made no response. There was no giddiness. Two days later the deafness was complete; and he (the speaker) believed it had remained so. The child had been educated accordingly. The Wassermann reaction had been negative. The second case had been submitted to two or three medical men before he saw it. The deafness came on suddenly in one ear, and some months later, after an adenoid operation, deafness was equally sudden in the other ear. In this case also the Wassermann reaction had been negative. This patient had not recovered after several years. In the third case, of similar onset and nature, he had had no opportunity of having the Wassermann reaction tested.

The pathology of these conditions was still somewhat nebulous, and he inclined to the view arrived at largely by a process of exclusion, that they were due to a neuritis. But he was still puzzled as to why the neuritis should affect the cochlear branch of the nerve, and not always the vestibular branch. Such cases were not *very* rare.

Mr. ARTHUR CHEATLE said that he supposed that Dr. McKenzie had considered the possibility of there being a functional cause of the conditions; that was his (the speaker's) own view, and he would like to know whether the child had been in contact with deaf people. With regard to treatment, he would separate the child from its present surroundings, and apply faradism. A long time ago he had made a mistake in the case of a boy who was deaf for many years and who subsequently regained his hearing; he (Mr. Cheatle) thought the same would happen in the present case.

Sir JAMES DUNDAS-GRANT said he agreed that this was most probably a functional case, otherwise it pre-supposed a simultaneous lesion of the two cochleæ, or a small hæmorrhage which picked out the two nuclei in the medulla. The spontaneous lip-reading proved the reality of the deafness here, in high degree. In testing cases at the Aural Boards it was useful to ascertain whether lip-reading had been acquired, as if so, the fact was evidence of the patient's *bona fides*. He elicited from the mother that on the previous day a chair had been pulled away and that the child fell and struck the back of her head. That might have frightened her. There was a very active knee-jerk.

Dr. Arthur Hurst, in November, 1919,¹ showed the case of a man who had been deaf since early infancy, when he appeared to have been dropped by his nurse. He (Sir James) had seen the child then, and he was apparently deaf, but rotation tests had induced the normal nystagmus, and his (the speaker's) note was that the prognosis was hopeful. When he grew up to manhood he had acquired, by education and suggestion, a fair amount of hearing power. It would be interesting to learn further developments in the present case.

Sir WILLIAM MILLIGAN said he could not agree that such a case was probably neuritic in origin, as the onset was so sudden. He thought the diagnosis must rest between some vascular change and hysterical deafness. He suggested that a skigram should be taken, as the fall might have caused some damage to the bone. He would recommend treatment by auto-suggestion.

Dr. P. WATSON-WILLIAMS alluded to the mother's statement that since the anæsthetic was given five days ago, the child could not yet retain food, nor even fluids. This was a long interval for post-anæsthetic vomiting, and gave support to the suggestion that the deafness might prove to be essentially "hysteria."

Dr. KERR LOVE (President) said that terms like "functional" and "hysterical" should not be used in association with such cases as the present. He had seen many of these in the

¹ *Proceedings*, 1919, xiii (Sect. Otol.), p. 8.

36 McKenzie: *Bilateral Nerve Deafness*; Ryland: *Mastoid Operation*

schools for the deaf, and the patients never recovered. It was necessary to educate them until they became adults, and to teach them lip-reading. The diagnosis of hysteria should only be adopted after every search for an organic cause had proved negative.

Dr. DAN MCKENZIE (in reply) said that the idea of the deafness being functional had occurred to him, as he had been impressed by the facility with which the child seemed to lip-read her mother's speech. If deafness had been complete, however, that would have favoured the functional view still more. The child would be separated from her mother and watched. In favour of the case being functional was the point that, had there been an inflammatory lesion in the labyrinth, there would have been vestibular symptoms, with some giddiness, whereas this child's vestibular reactions were normal. He (the speaker) would report on the case later. Functional deafness was itself a serious matter, as it might persist for years.

Natural Cure similar in Result to that following a Radical Mastoid Operation. Cavity Completely Dry and Epithelialized.

By ARCHER RYLAND, F.R.C.S.

WE all see cases of chronic middle-ear suppuration in which cholesteatoma is present, and in which the advancement of the disease, or the gradual necrosis of bone, is in the direction of a natural radical mastoid operation. In these cases, all one has to do at the time of operation is to take a sharp spoon or curette and scrape away soft necrosed bone; the radical mastoid operation is then complete. The difference in the case here shown is, that nature has entirely finished the operation, and the whole cavity is dry and epithelialized. There is no history of any operation having been done.

Sir JAMES DUNDAS-GRANT said that this case supported the view which he (the speaker) had advanced many years ago as to the advisability of retaining the lining of the cholesteatoma cavity when that lining was homogeneous, shining and fairly attached, because it served the purpose of a skin-graft. When the disease was seen at that stage it was well, in the radical operation, to scoop out the debris very carefully, and leave the shining membrane, which could be kept dry for years by means of spirit drops and occasional clearance.

Section of Otology.

President—Dr. J. KERR LOVE.

DISCUSSION ON ARTIFICIAL AIDS TO HEARING.

Dr. J. KERR LOVE (President)

said he had written to Dr. Jones, of Los Angeles, whom he saw two years ago and who was much interested in this subject, asking for a wiring chart of the amplifier set which Dr. Jones brought forward chiefly as a test, but also to a large extent as the most advanced form of electrical aid to hearing.¹ The diagram had arrived by air mail, only five minutes before he (the speaker) had left Glasgow to come to this meeting. He would be glad to supply Members with a copy in due course.

Mr. J. F. O'MALLEY: Aids to hearing are divided into two classes, mechanical and electrical.

Mechanical Aids are in the form of tubes, trumpets, auricles, resonators, &c., these being made in various shapes and sizes in order to give the desired help to deaf persons. They are constructed from various materials such as ebonite, non-inflammable "imitation-tortoiseshell," and metal, usually brass. Each of these materials has a different effect on the quality of the sound received, ebonite being the least resonant.

The size of the opening which receives the sound waves, the length of the conveying medium to the ear and the material of which the instrument is made are important features in obtaining the desired results. If these can be obtained from a mechanical aid it is to be preferred to an electrical aid, as the tone is more natural, the initial cost is less, there is no cost of maintenance and the instruments give no trouble.

Electrical Aids always consist of three parts, a battery, a receiver and a transmitter, these being connected by a cord or cords in various ways and made up in various forms. The principle on which an electrical instrument acts is as follows: The sound waves strike the transmitter diaphragm, where they are converted into electrical waves, by the varying pressure of the diaphragm on a number of hollow carbon balls or granules. These electrical waves are transmitted by the wires to the receiver, where they are re-converted into sound waves by means of the current produced by the transmitter varying the magnetism in the pole pieces of a permanent magnet. This is done by winding coils of wire on pole pieces suitably connected. The varying magnetism moves the soft iron diaphragm, thus producing sound waves. The pitch of any sound received by the transmitter may be altered by the relationship between the diameter of the transmitter and the receiver diaphragms, this being an important factor in accommodating various types of deafness. The volume of sound produced is varied to suit the case under test by altering the distance between the diaphragm and pole pieces in the receiver, thus making the voice as clear and natural as possible; if this distance is known it may be recorded, and thus the instrument, if damaged, can be readjusted without a personal visit.

The different wave forms given by these instruments are very noticeable. The oscillograph shows that the instruments chiefly used for the very deaf are those which give a wave form most *unlike* the telephone of commerce or human speech, which probably explains why many patients easily tire of an electrical aid. The oscillograph is a very delicate

¹ See *Laryngoscope*, 1924, xxxiv, pp. 673-86.

instrument used to take photographs of the wave form in an electric circuit and is most useful in studying instruments for the deaf.

Thermionic Valve Instruments.—These instruments are useful in extreme cases of middle-ear deafness. It is generally found that the chief objections to these instruments are: the weight, the initial cost, the cost of upkeep and the bulkiness of the instruments. My experience has been that if persons can carry on ordinary conversation with an instrument which can be worn on the clothes, they will decide on the smaller instrument on account of its portability and cost, although the results obtained from a thermionic valve instrument may be decidedly better.

Full details of the person's deafness should be supplied, and preferably by an aural surgeon, before a test. When these details are obtained it is possible to eliminate a number of instruments, thus making the test shorter. This is an important factor, as in some cases the ear tires quickly when tested, and thus gives unreliable results. In some cases, if the person becomes tired it is better to defer the test.

It is necessary in fitting aids that the weakest instrument should be tried first and care taken to increase the strength gradually.

A trial of all instruments should be made at home, as it is impossible for a person to decide in a consulting room, which may not have the same general acoustic properties as the ordinary living room, the kind of aid that is most suitable.

Mr. W. M. MOLLISON: I use these aids very little personally; but send patients who want them to an electrical expert. I shall therefore only say a few words about my experience of patients using aids to hearing.

Patients with high degrees of middle-ear deafness, and those with otosclerosis are almost always helped by some form of electrical instrument. Patients suffering from internal ear deafness, with short bone conduction, are occasionally, but only seldom, helped by such instruments. I have found one useful test to guide me, roughly, in advising patients, and that is, of the highest pitch, the monochord, heard through bone. Many patients cannot hear the monochord at 30 to 40 cm. through air, but hear well at 16 to 18 cm. through the bone. For these the little cornets worn under the hair are very useful.

Mr. HERBERT TILLEY said he had been struck by the fact that many patients with large perforations had not been tested by their aural surgeon as to the possibility of relieving the deafness by means of some form of "artificial drum." In many of these cases a very marked improvement in hearing could be brought about by this type of "aid."

He (Mr. Tilley) was in the habit of employing a small piece of wool soaked in liquid paraffin. The patients were taught how to insert the wool with suitable forceps, and they often quickly learnt to place it in a position which gave the maximum improvement of hearing. He did not advise the use of the gutta-percha disc, because it became foul and septic. Neither did he like the long wick. He assumed the patient to be tolerant, and to have a fairly large perforation. He had even enlarged a perforation so that a more effective application of the "drum" could be ensured.

Mr. A. R. TWEEDIE said that Bárány¹ had alluded to this method some fifteen years ago in connexion with his theory of the aerial conduction of sound to the sensory termination of the cochlear nerve.

Bárány considered that this function was independent of the tympanic membrane, but that the essential point was the passage of the aerial vibrations through the membrane of the oval window via the helicotrema to the round window—an excursion of the latter corresponding to an incursion of the former, and the regular route of these vibrations being determined and maintained by certain physical features.

The beneficial effect of the application of liquid paraffin to the middle ear, presupposed a tolerant middle ear. The essential point in this was occlusion of the fossula rotunda with a droplet of liquid paraffin (or some other inert fluid, such as mercury) by tilting the head slightly backwards after its instillation. In this position, the paraffin took the place of the droplet of mucus, which presumably occupied this position normally and tended to protect the round window from the direct action of the aerial vibrations, and thus ensure their constant course as above mentioned.

¹ *Monatsschr. f. Ohren.*, Jahrg. xlv, No. 5, April 25, 1910, and abstract of the same, *Journ. Laryng., Rhinol. and Otol.*, September, 1910, xxv, p. 491.

Dr. KERR LOVE (President) said that the condition of knowledge on chronic non-suppurative diseases of the middle ear was one of the reproaches of otology. During the last thirty years great advances had been made in the surgery of the ear, but little or none could be registered in the treatment of this middle-ear condition.

Electrical aids were very expensive; eighteen guineas was the amount paid for an instrument the use of which in a few weeks was abandoned. And further, these instruments were very unreliable; one could not predict that they would certainly be of value in a particular case. Moreover, the patient was ready to accept the fact of increased loudness having been produced as a proof that continuous use would make him distinguish sounds better. But there were two reasons why this was not the case. The first was, that the human voice was a compound sound, and no instrument, so far, intensified the voice equally over its whole range. It was in this direction that he (the speaker) thought success would ultimately be reached; it had already been partially attained by the Western Electrical Company of New York, whose instrument was on the principle of the audio-amplifier. The men who had done the best work in this sphere were Dr. Isaac Jones, of Los Angeles, and Professor Knudsen, a physicist. They had used two valves for the audio-amplifier, and they were apparently able to amplify high and low tones at will. In the middle-ear type of deafness the difficulty was to increase the value of the low tones without increasing that of the high, and vice versa. Seventy children in the Glasgow school for the partially deaf were subjected to tests with an electrical aid and the teachers were disappointed with the result; they spent three weeks in carrying out the tests.

[Diagrams were shown illustrating the wiring scheme devised by Professor Knudsen. These had been approved by Dr. Jones, of Los Angeles.]

Professor Knudsen had said:—

"Our experience with hard-of-hearing patients indicates that selective amplification does not fulfil our initial high anticipations. It is possible that through a process of adaptation, the individual has become accustomed to associating various images of his real world in terms of the distorted sounds he has perceived. Therefore, when selective amplification is provided to restore his hearing to the normal or approximately normal condition, these sounds really seem distorted to the individual."

And this brought him to the second reason for failure, a reason differing from that given by Professor Knudsen, viz., that chronic deafness was often of mixed type, the internal ear getting involved after the middle ear, so that amplification of the part of the scale selected did not always give the expected result.

He (the President) agreed with Mr. O'Malley that there was no more efficient instrument, to date, than the hearing tube. Otologists should try to protect their patients from having instruments given to them without the chance of trying them to see if they suited their case. With regard to rubber discs, he (the speaker) had sometimes found them useful, but now and then they were liable to set up a discharge in a dry ear. In all these discussions the importance of lip-reading must never be forgotten.

Mr. SYDNEY SCOTT said that a number of years ago a Committee had been appointed to investigate the various projected aids to hearing, and of that body he believed Dr. Kelson and the late Mr. Hunter Tod were members, while the late Frederick Womack acted as physicist. They investigated the instruments put forward by Hawksley, including the acousticon. At that time they could not, of course, examine the Marconi instrument, though some members had done so since.

That Committee had found that, as a rule, certain cases of middle-ear deafness of the form classed "otosclerosis" were benefited by electrical aids; but patients with senile deafness, who could hear low tones but could not clearly analyse voice sounds, and had lost high tone perception, heard better with a plain speaking tube. Mr. Cheate had pointed out that it was the person who heard best in a noise who was benefited by the acousticon. He (the speaker) had never met with a case of a child whose hearing had been improved by the use of the acousticon; it had never seemed worth while to buy an instrument for a child suffering from middle-ear deafness.

With regard to Mr. Tilley's suggestion relating to the insertion of paraffin-soaked wool into a perforation in a middle-ear case, he (Mr. Scott) had been struck by the frequency with which suppuration was thus set up. There was some danger of active suppuration being caused by

this foreign body when the ear had become dry. Wool, as an aid to hearing, could be used when there was no perforation at all, namely, where there was a relaxed drum, especially if the right quantity of wool was used. Patients came once a fortnight to have the drum inflated, and, after inserting such a plug, they had not needed advice or help for three or four months, while their friends had noticed remarkable improvement in their hearing. In some cases pressure had to be applied to the stapelial region, in others to the region of the fenestra rotunda, and in one case a false drum had to be inserted in front of the malleus.

Sir JAMES DUNDAS-GRANT said that, if possible, one should in the first instance try mechanical apparatus, though from the point of view of elegance the electrical devices were to be preferred, and it was to be hoped there would be still greater improvements in them. Many patients derived much satisfaction from wearing small electrical apparatus, but many reverted to the use of the mechanical forms. He had found the "banjo-shaped" instrument the most acceptable; it was flat, and its tube could be shortened, while it could easily be carried in the pocket.

Tubes for conversation varied enormously. Some were made of flexible material and of almost uniform bore; but they should be gently tapering, and the more distinct and uniform the tapering the better the result. He (the speaker) endorsed Mr. Mollison's remarks about artificial auricles and quoted the case of a teacher who, more than twenty years ago, had had these auricles supplied, and ever since then had been able to maintain herself by her profession; whereas without them she could not have done so. He also mentioned the case of an American lady who with their help had kept her conspicuous place in society.

He (Sir James) referred to a Scandinavian report of 368 patients on whom various kinds of apparatus for improving the hearing were tested. Of those with middle-ear deafness a small number derived no benefit, but 5 per cent. obtained most help from hearing tubes, 44 per cent. received most assistance from trumpets, and 45 per cent. from electrical apparatus. Of otosclerosis cases, 4 per cent. had most help from hearing tubes, 46 per cent. liked trumpets, 40 per cent. preferred electrical apparatus, 10 per cent. were unbenefited. Of those with internal-ear deafness, 15 per cent. received most help from hearing tubes, 52 per cent. from trumpets, and 19 per cent. from electrical apparatus. For old-age deafness, trumpets and tubes were found most effective. Therefore the Scandinavian figures went to confirm our experience in England.

He (the speaker) thought the cotton-wool plug had deserved its name of "artificial drum." He believed its use originated with the late James Yearsley. It was an invaluable aid, and he (Sir James) had been taught to use it by Laidlaw Purves, who strongly recommended it for cases of perforation and of relaxed membrane. He (Sir James) found it very satisfactory when moistened with parolein and a little menthol. When it was applied in suitable cases he had not seen suppuration re-started by it, but he told patients to apply, in the intervals between wearing it, a little dilute alcohol, as that seemed to be a safeguard against inflammation. A patient was sent to him from the North with the strictest injunctions not to use a drum, as one which had been applied had been lost in the ear, and there had been great difficulty in removing it. He (Sir James) did use a drum in that case, but he secured it with a thread, as indicated in his description of the "captive artificial drum."¹ The improvement in hearing had been so great that the patient had had to move from the front to the far end of the church he attended.

Professor BURGER said that in the schools for the semi-deaf in Amsterdam there was in use a simple megaphone with small modifications, and this gave satisfactory results.

Mr. CLEMINSON said that some deaf people found a difficulty in separating the words in a conversation; such patients were particularly those suffering from senile deafness. He was

¹ *Lancet*, 1922, ii, p. 1062.

shown by one instrument maker the Meyrovicz apparatus, which tended to bring about this separation of words without greatly magnifying their intensity. This apparatus consisted of a very small box, containing a diaphragm, attached by a small portion of its circumference, the remainder being free. The instrument could be held in the hand, and was said to have the effect of clarifying the sound by aiding the user to separate the words.

He had used an artificial drum for one patient only, and it had been a great success. The patient had worn it daily for five years, and so far as he knew—he saw her six months ago—she had had no suppuration at all. The addition of a grain of menthol, or two grains of carbolic acid, to the ounce of liquid paraffin, as the medium in which the cotton-wool was soaked, had seemingly prevented it.

Mr. J. S. FRASER said his experience had been that long black tubes for cases of nerve deafness were best, though it was doubtful whether they gave much better hearing than was obtained by placing the curved hand behind the ear. Celluloid auricles were good in cases of otosclerosis and of cicatrized tympanum.

He agreed with the President as to the enormous prices charged by makers of these instruments, and in the North these makers had ceased to allow patients to take away an instrument on a fortnight's trial before purchase. He spoke of this to one maker, and the reply was that "cases of deafness were very infectious," and one patient passed it on to another; so he (Mr. Fraser) ordered him out of the room.

The best instrument of the telephone type for cases of otosclerosis was one which had a special bone-conduction attachment, but it weighed 16 lb.

Mr. H. J. BANKS-DAVIS said that a novel form of artificial drum had come under his notice a few years ago. The patient was a University Professor and Lecturer in Law, suffering from tinnitus and Ménière's symptoms. The tympanic membrane had been destroyed and the foramen ovale was visible. When asked as to how he was able to carry on his coaching duties, he said that he always wore an artificial drum, and as cotton wool always produced a discharge he now used a conical plug of bacon-rind. On this being supplied, he quickly cut out a tapering piece about half an inch long, and adjusted it to his meatus and said, "I now hear quite well."

Mr. R. GRAHAM BROWN (Brisbane) said that he advised patients to try an instrument for a week or a fortnight before making a purchase. To young people his advice was that they should avoid aids to hearing as long as possible, because, especially in the case of electrical instruments, after using them for a time patients found they could not do without them.

Mr. LAWSON WHALE said that he had several times found that people who were deaf were made worse by electrical aids; these patients were always suffering from tinnitus. The additional sounds perceived by the use of the machine aggravated the original tinnitus.

Mr. SCOTT STEVENSON said he did not think members went far enough back in regard to electrical aids. Bell discovered that the vibrating diaphragm was a good means of conveying the human voice half a century ago, but he (the speaker) thought there must be a better means of conveying sound than in the microphone now employed. That was a very primitive instrument, considering how physics had advanced in the last fifty years. Instead of sending patients to the people who made these things and relying on their doing the experimental work, otologists should make an attempt to re-examine the microphone as a means of conveying sound and see whether it could not be improved. He told patients that they were not likely to get more good from anything else than a hearing trumpet or a speaking tube.

Casts of Labyrinths.

Shown by ARTHUR H. CHEATLE, F.R.C.S., and V. E. NEGUS, F.R.C.S.

THE casts represent the semicircular canals and cochlea in correct orientation. A frame was fixed on the skull, with two lateral pieces so placed that the lower edges corresponded to the Frankfort plane, i.e., the upper margin of the bony auditory meatus and the lowest point of the orbit. From uprights was carried a cross-bar, and from this a vertical support carried a small tube, the ends of which fit into each internal auditory meatus. After the opening up of the middle-ear spaces, including the mastoid antrum and cells, followed by the cementing of the cross-tube in position, the frame was detached from the tube and the latter left *in situ*. Metal was then run in through the foramen ovale and rotundum of one side in such a way that it ran across the small tube and into the semicircular canals and cochlea of both sides.

Mr. Shaw, of the Royal Dental Hospital, did the actual casting. He used an alloy which

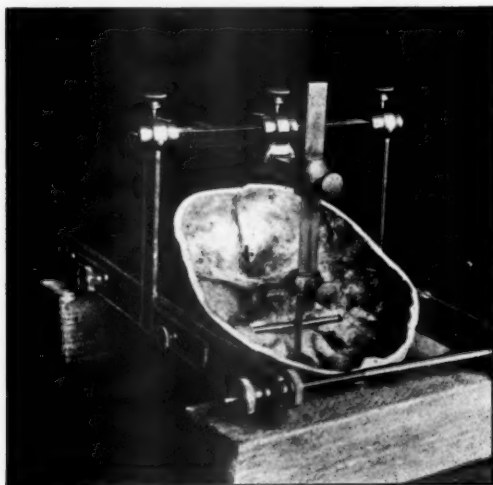


FIG. 1.—Frame used in casting models. The hollow cross-tube is seen in position in each internal auditory meatus.

consists of tin two parts, lead one part, and cadmium one part. The molten metal was run in by gravity and was assisted in its passage into the semicircular canals by exhaustion of air by means of a pump. A second frame has been constructed with pointers to correspond with the Frankfort plane on either side, and when these have been placed in position, the model of the labyrinths is held on the top of the frame on the patient's head. The model is then in the exact orientation it occupies in the skull. The utility of the method is in the first place to show the exact position of the internal ear for operative purposes; secondly, for demonstration purposes; and thirdly, in testing the patient's vestibular reactions. The frames used in casting and fitting on the patient's head have been made by Messrs. Mayer and Phelps. Enlarged models, twice the natural size, have been made by Mr. Thomas, seal engraver and medallist, 1, Great Pulteney Street, W.1, and from these casts of strong material can be made in any quantity required.

Mr. NEGUS said it was difficult to get labyrinth casts together in one piece, but this had

been done. It might be thought, as the cochlea was pointing inwards, the specimens were distorted, but they were correct and exactly as they came out of the skull. There would be found a different orientation of the canals in different subjects. The horizontal canal in the present specimen was more horizontal than was usually imagined. The Frankfort plane was

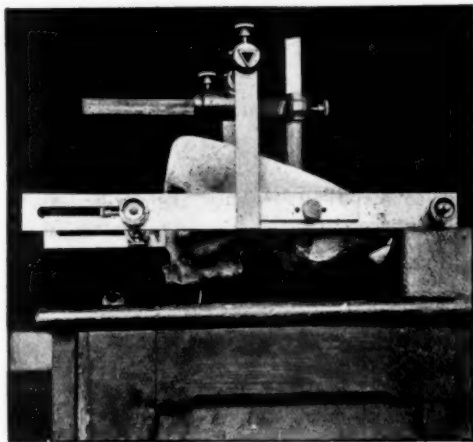


FIG. 2.—Method of orientating the frame on the Frankfort plane.

used, as it was the most fixed. Sir Arthur Keith tried to use the horizontal canal as a fixed plane for gibbons, but he found that the individual differences amounted to as much as 20 degrees. It was hoped to supply enlargements for teaching purposes. The models would be useful in testing for rotation and caloric tests.

Sarcoma of Left Occipital Region Simulating Mastoid Disease.

By W. S. SYME, M.D.

PATIENT, a woman, aged 50. First seen in April, 1924, when she complained of deafness in left ear and dull pain behind ear, passing upwards, and down towards shoulder. There was an eczematous condition of the external auditory meatus and thickening of the tympanic membrane, with other signs of chronic adhesive processes in the middle ear. There was some tenderness over the mastoid area and over the adjacent part of the occipital bone, but no definite swelling. The temperature was normal. The tenderness was thought to be due to a subacute, superficial, inflammatory condition associated with the eczematous condition of the auditory meatus. Her tonsils were very septic and she was advised to have them removed.

She was not seen again till January, 1925. She had lost weight considerably. She still complained of the dull pain behind the ear, the tenderness was more marked, and there was a definite swelling. Though this swelling was firm, it appeared softer in one part, but not actually fluctuating. One of three conditions seemed to be

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indicated : (1) chronic suppuration in mastoid cells abnormally far back, with superficial destruction of bone ; (2) a gumma ; and (3) new growth. The length of time was against mastoid disease, and the Wassermann reaction was negative. A general surgeon who saw the case with me inclined towards a diagnosis of mastoid disease. It was decided to explore. An incision was made over the swelling, somewhat further back than the usual mastoid incision. No pus escaped, but a mass of what appeared to be granulation tissue undergoing organization was found. Further exposure, however, showed that we were dealing with a new growth which had extensively invaded the bone and had spread to the dura mater. It was considered that further operative procedures were out of the question.

Histological examination of the mass removed proved it to be mixed-cell sarcoma.

Mr. R. GRAHAM BROWN showed a case of ? Epithelial Cyndroma invading the Temporal and Occipital Bones.

Section of Pathology.

President—Dr. J. A. MURRAY, F.R.S.

Researches on *Bacterium pneumosintes* (Olitsky and Gates). Improved Methods of Cultivation, with special reference to Vaccine Production.

By DAVID THOMSON, O.B.E., M.B., Ch.B.Edin., D.P.H.Camb.

(Hon. Director, "Pickett-Thomson" Research Laboratory, St. Paul's Hospital, London.)

INTRODUCTION.

Bacterium pneumosintes is a small Gram-negative anaërobic bacillus first isolated by Olitsky and Gates from the filtered naso-pharyngeal washings of patients suffering from influenza. The authors claim to have discovered this organism in November, 1918, during the great pandemic of influenza. They recovered it again during the recurrent wave of epidemic influenza in the spring of 1920, and still later in the epidemics of 1922 and 1923.

In September, 1924, Professor Mackie, of Edinburgh University, showed me a smear from a culture of *Bacterium pneumosintes* prepared by Dr. J. E. McCartney, who was working with Dr. Olitsky at the Rockefeller Institute in 1923. This smear, stained with polychrome methylene blue, showed minute rods and coccoid bodies, and two months later I received from Dr. McCartney a culture of *Bacterium pneumosintes* growing in Noguchi's medium. It is entirely due to Dr. McCartney's kindness in this matter that I have been able to undertake this work, in the course of which I have succeeded in devising certain culture media which appear to be more satisfactory for the growth of *Bacterium pneumosintes* than those used hitherto.

THE TECHNIQUE AND CULTURAL METHODS OF OLITSKY AND GATES.

The naso-pharyngeal washings from cases of influenza are passed through Berkefeld V or N filters, and the filtrates are inoculated in fairly large amounts (1-2 c.c.) into Noguchi's medium (human ascitic fluid containing a piece of rabbit-kidney), which is then incubated anaërobically at blood heat. A diffuse cloud appears in the medium and increases in density till about the seventh day. The germ may also be grown primarily from the filtrate by seeding the latter on 5-10 per cent. rabbit-blood agar plates, which are then incubated in an anaërobic jar.

According to McCartney colonies of the germ may be obtained on 10 per cent. rabbit-blood agar after seven days' incubation at 37.5°C., under strict anaërobic conditions. Other culture media have been used, viz., beef infusion broth containing 1 per cent. dextrose, 1 per cent. peptone, 0.5 per cent. sodium chloride titrated to pH 7.4 and enriched with fresh rabbit-blood, rabbit-kidney tissue, or the growth products of other organisms such as *Bacillus coli*. Solid and semisolid media may be prepared by the addition of agar in the proper proportion.

THE MORPHOLOGY AND CULTURAL CHARACTERS OF BACTERIUM PNEUMOSINTES AS DESCRIBED BY OLITSKY AND GATES.

When grown in Noguchi's medium the organism is a very small cocco-bacillus of regular form, with a length about two to three times its breadth and measuring from 0.15 μ to 0.3 μ in the long axis. When grown in enriched dextrose broth or on agar plates, it develops as a minute spindle-shaped bacillus, somewhat longer than the forms already described. The length may be increased to 1 μ , making the organisms obviously bacillary. Short chains and diploforms are found. On old

blood-agar plates involution forms have been observed. The longer, rod-shaped organisms have been fully identified with the original minute forms by serological tests and revert to the original minute forms on transfer to the original ascitic-fluid rabbit-kidney.

OTHER ORGANISMS RESEMBLING BACTERIUM PNEUMOSINTES.

Olitsky and Gates, 1922, found other Gram-negative anaërobic filter-passing bacilli, differing from *Bacterium pneumosintes*, coming from naso-pharyngeal secretions. One was a slender vibrio measuring $0.15\ \mu$ to $0.2\ \mu$ in thickness and from $0.2\ \mu$ to $2\ \mu$ long, with rounded ends and a curved shape which may approach a half circle. In fluid media it grew in flakes and not in a uniform cloud, and it did not emulsify readily.

The second was an extremely minute bacillus isolated from a patient with acute follicular tonsillitis. It showed greater variations in length and thickness than did *Bacterium pneumosintes*. It failed to give constant growth in fluid media.

The third variety obtained from normal throats showed long and short forms and failed to grow constantly in fluid media. Its colonies showed a sharp cone with irregular edge, and they were slightly brownish in colour.

These three varieties of bacilli were also distinguished from *Bacterium pneumosintes* by the fact that they were not agglutinated by an anti-pneumosintes serum.

TECHNIQUE AND CULTURAL METHODS EMPLOYED BY THE WRITER.

As already mentioned, I received a culture of *Bacterium pneumosintes* in Noguchi's medium from Dr. McCartney. Some time before its arrival Dr. J. Gordon Thomson and myself had been carrying out experiments with Boeck's culture medium, which had been claimed to grow the *Entamoeba histolytica*. (A preliminary account of this medium as devised by Boeck and Drbohlav for the cultivation of *Entamoeba histolytica* will be found in a note by Drbohlav in the *Transactions of the Royal Society of Tropical Medicine and Hygiene*, 1924, vol. xviii, p. 238.) The medium as employed for the cultivation of *Bacterium pneumosintes* was thus prepared:—

Whole eggs are broken into a sterile flask, mixed thoroughly, and to the mixture is added one-third of its volume of Ringer's solution. Inspissate for four to five hours on two days at 73°C . and on the third day at 76°C . Add the white of one egg to 1000 c.c. of Ringer's solution, and adjust to pH 7.4. Filter through Berkefeld and add sufficient of this filtrate to each tube of inspissated medium to cover the slope completely.

Inoculations were made from McCartney's culture in Noguchi's medium to three Boeck's culture tubes as prepared above. These were incubated in the anaërobic jar, and on the third day large numbers of minute Gram-negative bacilli appeared together with *Bacillus subtilis*. Later, however, a transplant from the Noguchi medium gave a pure culture of the Gram-negative bacillus free from *Bacillus subtilis*. The Noguchi tube showed no evidence of contamination, and the *Bacillus subtilis* probably came from the surface of the vaseline covering the medium. The growth of *Bacterium pneumosintes* in Boeck's medium showed as a fine white haze about the third day, which increased in density till about the seventh day of incubation. By centrifuging, an appreciable quantity of the pure germ deposit could be obtained.

The deposit stained by Gram (counterstain, basic fuchsin) showed minute Gram-negative bacilli, which corresponded with the photomicrographs of *Bacterium pneumosintes* as depicted by Olitsky and Gates when the germ is grown on solid media.

Subcultures from Boeck's medium were made on testicular-agar plates containing 10 per cent. of whole human blood, and minute colonies resembling those described by the above authors appeared on the third day of incubation. Smears from these

showed the same organism, but they appeared slightly longer than those obtained from Boeck's medium.

By continued subculture on solid media the organism grew more profusely, and began to appear even on the second day of incubation. It was also ascertained that it would grow on testicular-agar containing boiled human blood, and but very slightly on testicular-agar alone without blood. It refused to show any trace of growth whatever when incubated aerobically, nor, as Olitsky and Gates had informed me a year ago, would it grow in testicular mince broth.

There is every reason to believe, then, that these growths were cultures of *Bacterium pneumosintes*, such as existed in the Noguchi's tube sent by Dr. McCartney. Many subcultures from his original tube always yielded the same organism. It would appear, therefore, that Boeck's culture medium is very satisfactory for the cultivation of *Bacterium pneumosintes*. It would seem to be even more efficient than Noguchi's medium, and much more suitable, because the germ can be centrifuged from it entirely pure and free from tissue debris. This medium is therefore eminently suitable for the preparation of emulsions and vaccines of the germ. Cultures of *Bacterium pneumosintes* were made on Dorset's egg medium and on testicular-boiled blood-agar, and it was found that the latter was superior in growing power to the former. It was decided, therefore, to test whether *Bacterium pneumosintes* would grow on a medium (resembling in principle Boeck's medium) which had been used in our laboratory for over three years for the growth of pneumococci.

About three and a half years ago the writer had occasion to grow a large amount of *pneumococcus* types I and II. The yield from solid media was very poor, and an attempt was made to grow them in broth media. They did not grow well. It was found that they would not grow in ordinary broth unless blood-plasma or hydrocele fluid was added. When, however, they are growing in this latter medium, the protein of the plasma or hydrocele fluid precipitates out and becomes mixed with the germ sediment, rendering the standardization of the latter impossible. My laboratory superintendent, Mr. E. S. Dean, suggested that this difficulty might be overcome by mixing the plasma or hydrocele fluid with agar, then, after the latter had solidified, by adding the broth as well. This suggestion was immediately carried out. To a test-tube containing sloped blood-agar, sterile broth was added until it reached the top of the slope. It was found that the pneumococci grew well in this broth, and that no protein precipitate was formed. Testicular broth was used instead of the ordinary ox-heart broth, since it was found that the latter gave better growth. Subsequently, large quantities of pneumococci were cultivated in flasks containing testicular broth in contact with blood-agar or hydrocele-agar. Boiled blood was found to be practically as good as the unheated blood, and less liable to be contaminated. Although I had used this medium in the laboratory for over three years I had not realized its great virtues. By its aid I have recently cultivated from a case of measles three apparently new species of organisms, a Gram-negative ovoid diplococcus, a Gram-negative pleomorphic bacillus, and a very minute Gram-positive bacillus. It would seem, therefore, to be eminently suitable for the cultivation of many organisms which cannot be grown by the usual methods.

With regard to the solid culture media which I have employed I have long known that testicular-extract agar is much superior to nutrient agar prepared from extract of ox-heart. It is likely, therefore, that testicular-extract agar with blood would be superior to ox-heart extract with blood. This proves to be the case with *Bacterium pneumosintes*, since the colonies appear visible on the second or third day on the former, whereas according to McCartney they take seven to ten days to appear on the latter.

With a medium such as I have described above it becomes a comparatively easy matter to investigate and isolate strains of the *Bacterium pneumosintes* type from the sputum of patients.

During the past two years I have noted that in the respiratory passages a far larger variety of organisms is present than has hitherto been credited, and the more recent investigations carried out with these new types of media have amply supported this belief.

PRELIMINARY ATTEMPTS TO CULTIVATE BACTERIUM PNEUMOSINTES IN BULK FOR THE PREPARATION OF VACCINES.

By the aid of the media described above the writer has secured considerable quantities of *Bacterium pneumosintes* (McCartney's strain) and of the other strains of bacteria resembling it, recovered from cases of influenza. Ordinary toxic vaccines were prepared from these organisms in the usual way. These organisms are all extremely soluble in weak alkali, and it should therefore be very easy to detoxicate them, but the amounts so far collected are insufficient for this purpose.

The preliminary experiments as to dosage of the toxic vaccine of *Bacterium pneumosintes* (McCartney's strain) were carried out on my brother and myself. We each had successive doses of five millions, twenty-five millions and fifty millions respectively. There was little local reaction at the site of injection, but the general toxic symptoms were very noticeable after the doses of fifty millions. They produced a feeling of malaise and depression, with fleeting pains in the limbs and marked muscular weakness. The pulse was increased to about 90 per minute but the temperature was not raised. The feeling of weakness and depression lasted for fully four days, which in our experience is very unusual after small doses of vaccine.

The symptoms produced certainly resembled in a remarkable manner those experienced after an attack of influenza. Sample bottles of these vaccines, standardized to 100 millions per c.c., were then sent to several physicians for trial in the treatment and prevention of influenza. The doses for prophylaxis were not to exceed twenty-five to fifty millions, and the dose recommended for treatment was five millions at intervals of three or four days.

It will take a long time to ascertain whether these vaccines are of value in the prevention of the disease, but reports have already been obtained which appear to indicate that the small doses may be of value in the treatment of the troublesome cough which so often follows after influenza attacks.

INVESTIGATION OF THE SPUTUM FROM INFLUENZA PATIENTS DURING THE PRESENT RECRUDESCENCE OF THE DISEASE.

From two cases of influenza I have isolated in pure culture a minute Gram-negative anaërobic bacillus resembling *Bacterium pneumosintes* so far as mere morphology and growth in culture are concerned.

In the absence of specific distinguishing characters for this whole group of organisms one cannot at present offer any opinion as to the relationship of those isolated by me with *Bacterium pneumosintes* as defined and studied by Olitsky and Gates. Much further knowledge is needed on this point. My object in this paper has been merely to describe new methods for the cultivation of *Bacterium pneumosintes* received as such, and although I have so far been unable to cultivate this type of organism from the sputum of normal persons, I make no claim at this stage to have confirmed the work of the American writers.

SUMMARY.

A strain of *Bacterium pneumosintes* (Olitsky and Gates), hitherto propagated on Noguchi's medium, has been very successfully cultivated on media based on Boeck's work in connexion with the artificial culture of intestinal flagellates and *Entamoeba histolytica*.

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Section of Pathology.

President—Dr. J. A. MURRAY, F.R.S.

Some Morphological Factors governing the Incidence of Pernicious Anæmia.

By A. PINEY, M.D., M.R.C.P.

(*Institute of Pathology, Charing Cross Hospital Medical School.*)

THIS communication is intended only to serve as a short amplification of the thesis put forward elsewhere (Piney, 1925 [7]) that pernicious anæmia is a disease occurring only in certain congenitally predisposed persons. The earlier account was incomplete in that the nature of the predisposition had not been investigated with care.

The frequency of familial incidence in pernicious anæmia (Gulland and Goodall, Meulengracht) would seem good reason for supposing that there is some peculiarity inherent in the sufferers. In addition, the frequency of a familial incidence of achylia gastrica, which is an invariable concomitant of pernicious anæmia (Faber), would seem to indicate a similar conclusion. The distinctive blood-picture of the disease, viz., the presence of true megaloblasts with reticular (not "cart-wheel") nuclei (Piney, 1924 [6]), the "inversion" of the leucocytic blood-picture, and the great myeloid hyperplasia without adequate regeneration of the blood, all point to the existence of some factor different from almost all other diseases. I showed (Piney, 1924) that true megaloblasts could be found in the blood only in the embryo, in pernicious anæmia and in acholuric familial icterus; the only pathological states in which these distinctive cells appear are, therefore, the two "diseases of the blood" in which familial incidence is well recognized.

The purpose of the present communication is a description of the origin of the megaloblast, together with certain inferences to be drawn from the observations. In pernicious anæmia so much attention has been devoted to description of the hæmosiderosis of the liver that little else has been observed in this organ. It is possible, in these cases, to find small masses of nucleated red cells with reticular nuclei lying in the lumina of the lobular blood-channels of the liver. The ordinary process of hæmatopoiesis in the liver is an extravascular one, and results in the formation of normoblasts and granular leucocytes, whereas this intravascular process produces only megaloblasts. It is striking that the channels in which formation of megaloblasts is proceeding are not lined by endothelium (fig. 3).

It seemed that little more could be elucidated by observation of ordinary pathological material, and therefore the process of blood-formation in the embryo was investigated.

The commencement of blood-formation in the "blood-islands of Pander" on the wall of the yolk-sac is well known, but the nature of the cells has been the subject of much argument. Fig. 1, p. 6, shows that they are of similar type to those found in the capillaries of the liver in pernicious anæmia. It was now necessary to investigate the condition inside the body of the embryo, and it was found that only in the liver did such intravascular erythropoiesis take place. It was very striking that, in the embryo as in pernicious anæmia, this process was monophasic, i.e., resulted in the formation of only one cell type. At this early stage of development there were only nucleated red cells of megaloblastic character in the embryo, but very soon there was observed a further but different process of hæmatopoiesis in the connective tissue of the portal tracts where red cells with "cart-wheel" nuclei and granular leucocytes were formed. Fig. 2, p. 7, shows the appearances at a time when both

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intravascular formation of megaloblasts and extravascular normoblasto-granulocytopoiesis were proceeding. It soon became evident that, as the formation of normoblasts and granulocytes became more active, the production of megaloblasts became less intense and the capillaries of the liver developed an endothelial wall in place of their previously reticular one. Connective tissue elsewhere in the body is, of course, also hæmatopoietic although gradually the function of normoblasto-granulocytopoiesis becomes confined to the bone-marrow. But there takes place a continuous decrease in the number and size of the megaloblastic areas of the liver until, at about the end of the eighth month of intra-uterine life, there is no trace of it to be found, i.e., in the normal adult there is no sign of formation of megaloblasts.

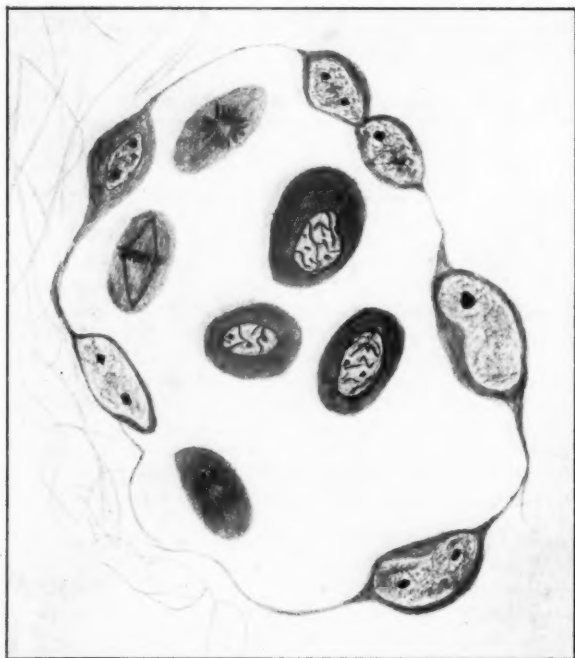


FIG. 1.—Blood-island from yolk-sac of human embryo. A group of megaloblasts is distinctly seen.

It is also clear that the totally different origin and character of the megaloblast and the normoblast, respectively, make it impossible to believe that megaloblasts can arise from normoblasts in pathological states.

In this connexion great interest attaches to the consideration of the incidence of pernicious anæmia in persons infested with *Bothriocephalus latus*—a subject which was first elucidated by Schaumann, who showed that only a small proportion of persons infested with this worm developed the hæmatological and other signs of pernicious anæmia. He also called attention to the comparatively great frequency with which relatives of persons with bothriocephalus pernicious anæmia suffered from the cryptogenetic form of the disease: he was even able to observe the develop-

ment of cryptogenetic pernicious anæmia in a person who, many years earlier, had suffered from the bothriocephalus form.

It must also be recalled that other diseases, which usually cause only a "secondary" anæmia, may produce the signs of pernicious anæmia, e.g., syphilis (Ausderau); and also pregnancy in some rare cases, has been the exciting factor of the malady (Beyer-Gurowitsch).

My own series of cases of pernicious anæmia, like those of other writers, has of course been collected from cases occurring in the adult, but, in spite of this, it has been surprising to note the great frequency with which it is possible to elicit a history of the individual not having been a full-term infant.

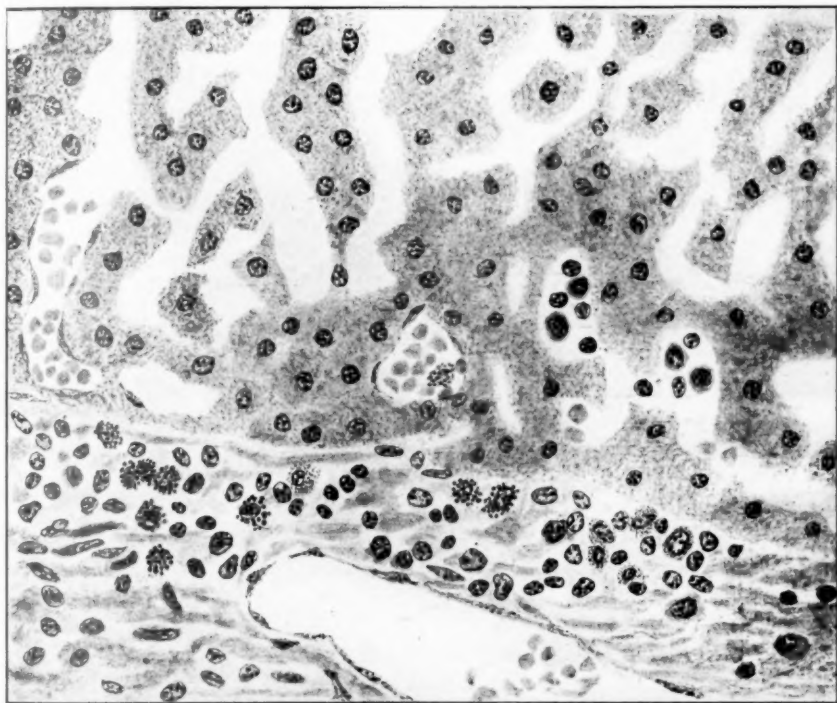


FIG. 2.—Liver from human embryo (15 weeks). The section shows a part of a portal tract in which normoblasts and granulocytes are being formed, but it also demonstrates the development of megaloblasts in vessels between the liver-cells.

It should be possible to attempt some sort of correlation of these observations.

The process of embryological development shows a definite "balance" between two different types of formation of red cells, and, while the primitive megaloblastic tissue atrophies, the definitive normoblastic one develops to its natural level. It seems justifiable to presume that interference with one of the processes will necessarily result in some correlated disturbance of the other, e.g., persistence of some part of the megaloblastic mechanism would be associated with imperfect formation of the normoblasto-granulocytic one. Such a view would account for the development of pernicious anæmia in predisposed persons only, but it would also illuminate

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other aspects of the disease. The very fact that the bone-marrow is much more bulky in persons with pernicious anæmia than in normal individuals, while the morphological composition of the blood is much inferior to normal, would indicate that the myeloid hyperplasia is defective in quality. The existence of the hyperplasia, together with the diminished production of granulocytes, cannot possibly be regarded as the result of a toxin, whereas, if the present view be adopted, the leucopenia with relative lymphocytosis can be accepted as a result of the primarily defective bone-marrow.

There is, of course, no evidence that the achylia gastrica, which invariably accompanies pernicious anæmia, is also a correlated developmental defect; but Weinberg was able to show that persons with constitutional achylia tended to present a

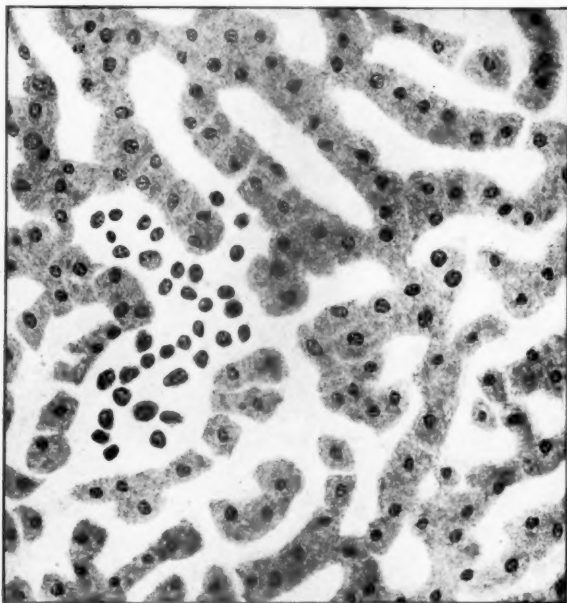


FIG. 3.—Liver from a case of pernicious anæmia. An intravascular collection of megaloblasts is seen and it is obvious that the vessel containing them is not lined by endothelium.

blood-picture resembling that of the slighter degrees of pernicious anæmia, i.e., it would seem as though there might well be close relationship between the two ontogenetic errors.

It is equally possible that the extraordinary changes in the spinal cord can be regarded as being of the nature of an abiotrophy, because in no other malady is there so complete an absence of reaction around the injured foci of nerve tissue.

It may, as a working hypothesis, be concluded that various agents capable of producing a "secondary" anæmia in ordinary persons will produce pernicious anæmia in such individuals as possess the necessary remnant of "megaloblastic tissue." On such a basis it should be possible to divide the cases into:—

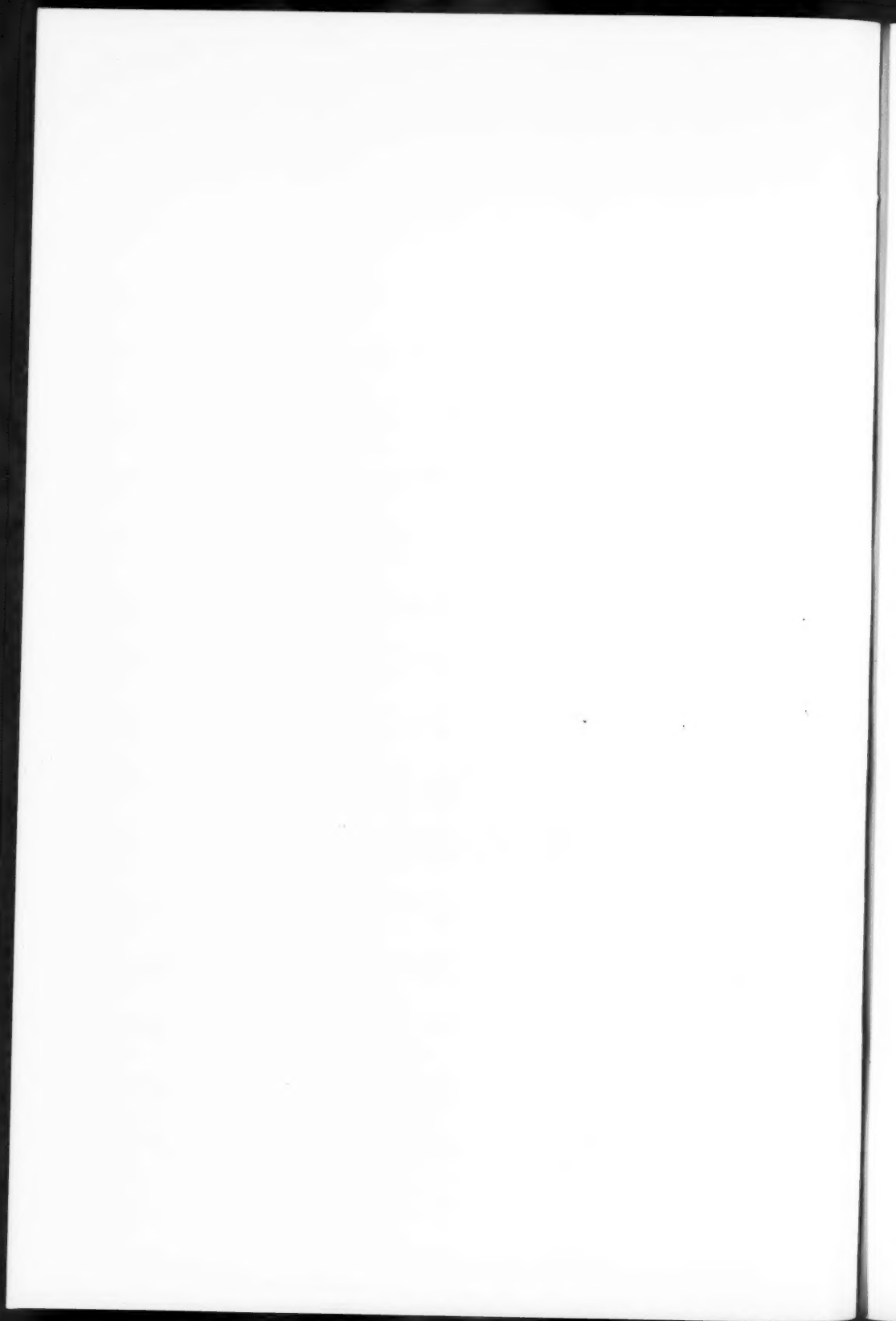
(1) Cryptogenetic cases in which the nature of the exciting cause is not discoverable. It is possible, however, that some of these may represent a simple failure of the ill-formed hæmatopoietic tissues under the stress of ordinary life.

(2) Infective cases in which the exciting cause can be determined. It would appear probable, if the present view of the genesis of the disease be correct, that this group of cases will increase at the expense of the former.

Again, one is forced to the conclusion that pernicious anæmia must be regarded as a clinical and pathological entity but not an ætiological one.

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Section of Psychiatry.

President—Dr. T. B. HYSLOP, F.R.S.E.

DISCUSSION ON PROPOSED CHANGES IN THE LAW REGARDING THE CRIMINAL RESPONSIBILITY OF THE INSANE.

Mr. DONALD CARSWELL.

THE subject for our consideration to-night is the law of England upon the criminal responsibility of insane persons with special reference to certain proposals that have recently been made for its amendment. Now I am not going to detain you with an exposition and criticism of the existing law. In a gathering like this I can safely assume that everyone is familiar with the substance of the so-called *Rules in McNaughton's Case*, and has some acquaintance—perhaps even a painful acquaintance—with their shortcomings. Moreover, in the matter of criticism, everything worth saying has already been said, every point—medical, legal and logical—has already been taken by Maudsley, FitzJames Stephen, Mercier, and many other acute thinkers, and for me to repeat them would be tedious and otiose. But there are two points (and only two) touching the credit of the McNaughton Rules that I would like to recall before passing to other matters.

(1) The McNaughton Rules are unique in English law in that they have acquired the force of law, and yet are neither parliamentary enactments nor judicial decisions. They are merely the personal opinions of fourteen out of fifteen Common Law judges upon certain general questions framed for them by the House of Lords in 1843. It is very doubtful if the judges expected their opinions to pass into the *corpus* of English law, which would be tantamount to investing the judges with legislative power—a usurpation which they would have regarded with horror. One judge did foresee the danger. Maule J., dissenting from his brethren, took the preliminary objection that by English law a judge's opinion is not binding unless it has been given (a) on a specific case and (b) after hearing counsel. Nevertheless, out of respect to the House of Lords, he drew up a series of answers in which, with much learning, he contrived to say nothing at all. Many years later, Stephen J., agreeing with Maule J., declared that though the rules had been followed by individual judges, they had never been regularized by the approval of a court sitting *in banc*. Since the institution of the Court of Criminal Appeal, that requirement has been in part at least fulfilled, for that Court has repeatedly affirmed the rules as the law of the land. I doubt, however, if Maule J.'s two conditions have been fully satisfied. The Rules have never been

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argued before the Court of Criminal Appeal, and that Court has never affirmed them on their merits. They have always been taken for granted as authoritative and binding, and the only question for argument has been whether they have been modified by subsequent judicial decisions. To this day, then, the Rules remain untested by argument.

(2) All this is bad enough, but it was left to the Puckish ingenuity of Mercier to point out what nobody had previously noticed, viz., that the questions set by the House of Lords were not the questions that the judges answered. The judges did not like the House of Lords' questions, which they could not answer, so they substituted another set of their own devising which they thought they could. Like the fabled undergraduate, "Far be it from us," they said, "to disparage the minor prophets, but in our opinion greater interest attaches to the kings of Israel, who were as follows."

Well, the kings of Israel have been with us for eighty years in undisturbed possession, so I suppose they must now be allowed a good title. I only mention these circumstances in order to emphasize the fact that the McNaughton Rules began as squatters and it is no use pretending, as some lawyers do, that they are an old landed family. But the fact remains that the Rules are now undoubted law. They may have become so by mistake, but *communis error facit jus*, and there is an end of it. The question is whether the law ought to be altered.

The initial objection to any interference with the McNaughton Rules is on the score of the necessity of authority. The argument runs thus: Granted that the Rules are as bad as they are said to be, granted even that they are unjust in their operation, still they are certain; and is it not generally agreed that certainty in law is even more important than justice? And even from the point of view of justice is rough justice not better than no justice at all? The whole conception of responsibility involves difficult, subtle, obscure considerations. Once you step aside from the sure footpath afforded by the McNaughton Rules, you will find yourself floundering in a metaphysical bog from which there is no escape. Anything is better than that.

This argument is plausible, but it involves several assumptions. It assumes, first, that responsibility is necessarily and exclusively a question of law, which is a *petitio principii*, for responsibility is quite capable of being treated as a question of fact. Secondly, it assumes that the McNaughton Rules secure certainty, a point on which many of us are not at all satisfied. The third assumption—and the only one with which I am for the moment concerned—is that without some more or less arbitrary guide like the McNaughton Rules one is bound to get into a hopeless muddle. The long-drawn and barren debates between lawyers and medical men do lend support to the idea; but when you come to look at them they prove no more than that the topic lends itself to irrelevance. I may be wrong, but my own conclusion is that the question is a comparatively simple one. Its apparent difficulty arises from the fact that there are two false steps that one may take almost at the outset. Let me indicate them briefly.

First, there is the error that is embodied in the vulgar saying, "Everybody is a little bit mad," or "Nobody is *perfectly* sane." From one point of view it would be difficult to conceive a sillier remark. Strictly speaking it means nothing; and I imagine that that sorely-tried man, the alienist, must feel his own mental stability imperilled when he is assured—as he is every day—that it must be true because "he himself has said it," though the question whether it is to his credit is left in some doubt. What the alienist has said of

course is that "insanity" is a relative term, which is quite true. This is taken to be the same thing as saying that it is just a question of degree, in which case where is the line to be drawn? How many grains of corn make a heap, &c.? People who argue in this fashion imagine that reality can be reduced to a dialectic process. But, apart from that, the proposition is inaccurate. To say that a thing is "relative" is *not* exactly the same as saying that it is a question of degree. The latter implies an abstract measurement; the former points to a practical end—relative to what? In the case of sanity and insanity, the latter implies that there exists somewhere an archetype—a Platonic *idea* of the sane man of which we mortals are more or less imperfect copies. But such metaphysical flights are not part of the alienist's business. He takes no stock of Platonic absolutes. He regards only his general experience of mankind, and it is enough for him that, relative to that experience, the condition of a patient who is suffering from general paralysis of the insane requires a lunacy certificate.

The second error that besets the question of criminal responsibility is similar to the first, and indeed is often combined with it. How, it is asked, can one define the criminal responsibility of the insane without inquiring into the concept of responsibility in general, which is a matter without conclusion? Scientific investigation, it is argued, leads to an *impasse*, for science is bound to be determinist, and determinism means that there is no such thing as responsibility. The saint and the sinner, the ascetic and the voluptuary, the patriot and the traitor, the sane man and the lunatic all act as creatures of cause and effect. How then can you praise one and punish the other? Why punish anybody? This is supposed to be a *reductio ad absurdum*, and so it would be if only the reasoning were correct. But why should a just statement of criminal responsibility involve a roving commission over the whole field of ethics. If there is one thing clear about social punishment it is that it is awarded in accordance with the ethical standards that prevail from time to time. At one time all criminals, sane or insane, may be punished indiscriminately; at another time unsoundness of mind may be regarded to a greater or less extent as an excuse. There is no eternal and absolute standard; and the notion that a just law of the criminal responsibility of the insane cannot be framed without a solution of the final problems of ethics is quite irrelevant. Ethical theory has nothing to do with the case. Criminal responsibility, it is true, does involve ethical considerations, but they are strictly practical. The question we have to answer is: Do the McNaughton Rules, according to our standards, do justice to insane persons charged with crime; and, if not, how is such justice to be secured? In this connexion I cannot do better than quote the words of Mercier. After defining the term "responsible" as "rightly liable to punishment," he proceeds:—

"By rightly liable to punishment I mean liable to punishment on grounds that appear fair and just to the ordinary man *when they are explained to him*—grounds that commend themselves as equitable and right, not to the faddist, the pedant, or the enthusiast, but to the common sense of the common man of *this time and this country*."

These are wise words. You will notice the emphasis placed on "the common man" and likewise the qualifications of the term. The common man is to have the grounds *explained to him*, and he is not an abstract "common man," but "the common man of this time and country." Having gone so far, it is curious that Mercier did not take the step that by logical

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necessity follows, viz., to deny the possibility of legal rules for the determination of a lunatic's criminal responsibility and to declare boldly that the question is one for the unfettered decision of a jury. This step was in fact taken last year by the Medico-Psychological Association in the memorandum which they submitted to Lord Justice Atkin's Committee. That memorandum is a document not only of great interest but of permanent value; and although its argument was not accepted by Atkin, L. J., and his colleagues, I venture to say, with all respect, that it has not been answered. I cannot deal with it in detail, but its conclusion can be stated in a single sentence, viz., "No man should be liable to punishment for a criminal act committed during and by reason of insanity; and if it be proved that the act was committed during insanity there is a presumption, which it is for the prosecution to rebut, that it was committed by reason of insanity." To put it in another way: The jury should be directed that where they are satisfied that the prisoner was insane they must not convict unless the prosecution have satisfied them that the insanity had nothing to do with the crime.

There is nothing, to my mind, very startling in such a proposal. It is simple, straightforward and just. But the Atkin Committee have detected a fatal flaw in it. "It is all very well," they say, "but if your proposal were adopted, how on earth are we ever to get juries to convict lunatics?" That in substance is what they say. If you do not believe me, read the Atkin Committee's Report. You will agree, I think, that it carries its own refutation on the face of it, so I do not propose to waste your time by discussing it further.

My own view, as you may have gathered, coincides generally with that of the Medico-Psychological Association. I have approached the matter from the lawyer's standpoint. I have given it long and anxious thought, and my conclusion is that the McNaughton Rules and all rules on the same model are juristically bad—that they are useless, anomalous and calculated to defeat the ends of justice. I am bound to admit, however, that I am in a minority. The great mass of lawyers insist that rules of the kind are necessary, and some doctors are prepared to agree with them. No one regards the McNaughton Rules as perfect, but it is said that in principle they are sound, and with a little tinkering they might be made to pass. The bit of tinkering that is in favour at the moment is an addition to the rules to the effect that it should be a good defence to show that the accused was by reason of mental disease *irresistibly impelled* to commit the act with which he is charged. This notion was first suggested by Stephen as a gloss upon the McNaughton Rules; but Stephen himself would not vouch for its legitimacy, and it has since been rejected by the Courts. The British Medical Association, however, have put it forward as the only possible and acceptable amendment. It has been approved by the Atkin Committee, and within the last few weeks it has been included in Lord Darling's Criminal Responsibility (Trials) Bill, which generally embodies the substance of the Atkin Committee's recommendations. Now let me say at once that this doctrine of "irresistible impulse" has achieved a larger measure of agreement between lawyers and doctors than anything else in the vexed question of criminal responsibility; and what these lawyers and doctors are agreed upon is this—that it is utterly meaningless and impracticable, and the sooner it passes into the limbo of vain imaginings the better for everybody. I am not qualified to explore its medical absurdities, but I ask leave to quote some words that I wrote two years ago, which I think are to the point. I leave you to correct the errors and supply the omissions.

"Certain insanities (such as epileptic insanity and some conditions of puerperal mania) are characterized by a high degree of impulsive action, usually suicidal and sometimes homicidal also; but where there is a very high degree of impulsive action, it is difficult to say that the patient knows what he is doing, and consequently such cases are generally capable of being brought within the scope of the McNaughton Rules. I write subject to correction, but so far as my own observation goes, the cases in which the defence of 'irresistible impulse' is set up are seldom, if ever, cases of true 'impulsive insanity.' Usually there is ample evidence of deliberation. The defence, relying on the popular psychology of action, can think of nothing better to suggest than that the culprit was irresistibly impelled to commit the criminal act, but as all the circumstances negative any such idea, the jury, very naturally and properly, refuse to accept an explanation that insults their intelligence."¹

In the foregoing citation I make no mention of obsessional states. These are often relied upon as hard cases under the existing law. Even Maudsley makes much of them, and thereby, I think, weakens his case. Obsessions, as we all know, can be and are resisted and controlled by the patient, and it is an abuse of language to talk of them as constituting "irresistible" impulses. Powerful they may be, and a man may yield to them, which simply means, not that they are irresistible, but that they have not been resisted. So far from being a reason for exempting from punishment, mere obsessions are a powerful argument for penal sanctions. The soldier going into action has an impulse to run away which would be irresistible but for his knowledge that if he yields to it he will be shot. The penalty is ordained for the very reason that without it the impulse would not be resisted. Such is the main legal criticism of the doctrine of irresistible impulse, and it is so conclusive that I feel it unnecessary to enter upon any discussion of the many subsidiary objections which may be urged against it.

In justice to the Atkin Committee it must be said that in proposing to allow the doctrine of "irresistible impulse" they do not contemplate that it would cover any case that is not capable of being brought within the existing law. All they seek to do is to fill out what they describe as "a logical insufficiency" of the McNaughton Rules—in other words it is a mere drafting amendment without practical consequences. Why the waters should be troubled in order to cure a "logical insufficiency" it passes my wit to tell.

There are many more things I might say, but I will conclude by repeating my submission, viz., that the criminal responsibility of a person alleged to be insane cannot justly be determined by means of a set of invariable rules, but is a matter to be decided by a jury in accordance with prevailing normal standards after due consideration of all the relevant facts, including the evidence of medical witnesses, on the general mental condition of the accused. There is no need for "rules." The question of fact for the jury would be a very simple one, viz.: "Are you satisfied, on a fair view of the evidence, that the prisoner's act proceeded from a diseased mind? If so, you will acquit him. If, on the contrary, you think his act proceeded from pure wickedness of mind, you will find him guilty." I can conceive of no question more fit for the decision of a jury. Questions quite as difficult are submitted to juries every day. Are there any rules of law that juries must observe in determining what is fair comment? What is even more to the point (for it involves the very question of unsoundness of mind) is the testamentary capacity—a pure jury matter. Surely if a jury can be trusted to pronounce on the testamentary capacity of

¹ "Insanity and Criminal Responsibility," *Fortnightly Review*, August, 1922.

an alleged lunatic they can be trusted to pronounce on the analogous matter of his criminal responsibility. I can see no difference in principle between the two cases.

To those who still have doubts, and who foresee red ruin and the breaking up of laws as the inevitable result of the abrogation of the McNaughton Rules, I would say: Read the case of *R. v. Shipley*—you will find it in *State Trials*, vol. xxi—and if you have read it before read it again. Some 140 years ago Dr. Shipley, Dean of St. Asaph, was prosecuted for publishing a seditious libel. He was defended by the great Erskine, then rapidly ascending to the zenith of his fame at the Bar. The law at that time was that it was for the judge and not for the jury to find whether the matter complained of was a libel—i.e., that libel was a question of law—and that all the jury had to find was whether it had been published. Erskine disputed the law, but was overruled, and the Dean was convicted. Erskine, in a speech which is one of the classics of forensic argument, moved in the King's Bench for a new trial. Here is what Lord Mansfield said in discharging the rule. After stating the law, as it then stood, his Lordship proceeded:—

“In opposition to this, what is contended for? That the law shall be in every particular cause what any twelve men, who shall happen to be the jury, shall be inclined to think, liable to no review and subject to no control, under all the prejudices of the popular cry of the day. . . . Under such an administration of law no man could tell, no counsel could advise, whether a paper was or was not punishable. I am glad that I am not bound to subscribe to such an absurdity, such a solecism in politics. Agreeable to the uniform judicial practice since the Revolution, warranted by the fundamental principles of the Constitution, of the trial by jury, and upon the reason and fitness of the thing, we are all of opinion that this motion should be rejected.”

And yet, a few years later as a direct result of the Dean of St. Asaph's case, Fox's Libel Act declared this absurdity, this solecism in politics, to be the law of the land; and so it has continued to this day. And here we are to-night all alive and comparatively well. I am almost encouraged to imagine that we might equally well survive the abolition of the Rules in McNaughton's case.

Dr. R. H. COLE

said Lord Justice Atkin's Committee was to some extent hindered by having two separate medical reports, one from the British Medical Association and one from the Medico-Psychological Association. In 1896 the feeling against the McNaughton Rules was very strong, and the Medico-Psychological committee of that date set to work fully intending to advise that they be abrogated. But after giving a good deal of consideration to the matter they decided the rules could not be improved upon. Dr. Maudsley was the first medical man to attack the rules, and Maudsley's point was that at the back of the intellectual faculties of mind were the instinctive and emotional activities, which were largely responsible for criminal acts. If any attempt was to be made to tinker with the McNaughton Rules, he (the speaker) suggested that the question of defective reasoning should be left out, the question being restricted to one of the degree of actual disease of the mind. He personally agreed that the rules should be abolished, and though eventual justice was done to the prisoner, he thought temporary injustice had been done to insane persons who committed criminal acts, in the performance of the ritual of the death sentence attaching to a prisoner condemned on the capital charge, the judge meantime well know-

ing the ultimate goal was Broadmoor Criminal Lunatic Asylum. The Court of Criminal Appeal and Criminal Lunatics Act did much to undo the errors of juries, for the latter frequently attached little or no weight to medical evidence. In many cases the McNaughton Rules hampered the medical man in giving his answers to questions. Logic required that degrees of insanity should be recognized, and that in minor degrees of mental disease the person was not altogether irresponsible. The law must apparently draw a hard-and-fast line, but the medical man could not say exactly where health ended and disease began. Doctors were not agreed on the subject of irresistible impulse, but it would help to have it included in the law of the land; the judges would see that it was not abused, especially as they were now more learned in medico-psychological matters than in time past.

Mr. A. H. TREVOR (Commissioner of the Board of Control)

expressed his agreement with practically everything said by Mr. Carswell in his paper. Under the McNaughton Rules as to responsibility it had first to be shown that the accused did not know the nature and quality of his act, or that, if he did, he did not know that it was wrong. There were innumerable instances of persons, undoubtedly insane and irresponsible, committing crimes, who well knew the nature and quality of their acts and that it was both morally and legally wrong; moreover, they were often capable of much cunning and forethought in carrying out their schemes. These rules, as tests of responsibility, were incomplete. Lord Justice Atkin's Committee proposed a further incomplete test, that of "irresistible impulse," which would only give rise to further hair-splitting arguments. All these incomplete tests should be done away with. He (the speaker) observed that as soon as insanity was undoubtedly proved the whole question of responsibility went by the board.

He did not desire to see any substantial alteration of the Trial of Lunatics Act, 1883, or of the Criminal Lunatics Act, 1884. Under the former the defence had to show that the accused was "insane so as not to be responsible," and not so insane as not to be responsible. Under the latter, as soon as the question of insanity was raised with reference to a person under sentence, it became the duty of the Home Secretary to have him medically examined, and if his insanity were confirmed he was then dealt with as a lunatic. No person found to be insane now underwent capital, or indeed any other, punishment. He was ordered to be detained as a lunatic patient either in Broadmoor or one of the ordinary mental hospitals. So far, therefore, as the general public was concerned, the principles laid down in the McNaughton Rules were only of academic interest, though they caused infinite friction between the legal and medical professions. He, and those who thought with him, considered the rules archaic and would like to see them abolished.

Dr. J. CARSWELL (Ex-commissioner, Scottish Board of Control)

confined his remarks to two points. The first was that the claim made before Lord Justice Atkin's Committee by the Medico-Psychological Association, based, as he contended, upon the considered judgment of those best entitled to speak on the matter, amounted to this: that the *fact* of insanity should be put to the jury. In two recent and important cases this was not done; the question put to the jury was: Was the accused person insane *so as not to be responsible in law*? Secondly, he did not think Lord Darling's Bill would settle

8 Hyslop—Carswell: *Criminal Responsibility of the Insane*

this controversy, nor, indeed, would any other measure on similar lines. It was professedly based upon the recommendations of Lord Justice Atkin's Committee. But what was that Committee's report? It was nothing more than a contention for the continuance of a contention.

Dr. T. B. HYSLOP (President)

said that insanity was incapable of definition, so also was sanity; attempts to define one or the other revealed a narrow comprehension, as the terms were undefinable, hence there could be no line of demarcation between the two. The same held good with regard to responsibility. Until a definition of responsibility was agreed upon it could not be said what irresponsibility consisted of. Unfortunately, the questions of insanity and responsibility were intermingled. Doctors were questioned in courts of law as to sanity or insanity, but they were not allowed to express any opinion as to responsibility, yet in that lay the crux of the question. Learned judges, in charging the jury, held that it was for them, the jury, to deal with the question of responsibility. All who had had experience of the courts would recognize that as the truth. With regard to the McNaughton ruling, one had often encountered cases in which this ruling had been applied to the criminal who did not come within its provisions at all, and the criminal, having been found guilty, suffered the extreme penalty of the law. He asked what the McNaughton ruling had to say about somnambulism, the dream-state, epileptic automatism, and various types of epilepsy, and he replied that it had nothing whatever to say about them. It was pathetic to sit in court and hear a jury being charged to apply rules which had no bearing whatever on the case; it was quite a frequent occurrence. This early Victorian ruling of 1843 was applied indiscriminately to all and sundry. The ruling should be banished from the courts, and every case treated on its merits, medical witnesses being allowed to give evidence on responsibility, not on insanity alone. This subject could not be dealt with conclusively until Harley Street was recognized on the question of responsibility.

Mr. DONALD CARSWELL,

in acknowledging a vote of thanks for his paper, referred to a case which occurred twenty years ago, involving an obvious miscarriage of justice from the application of the McNaughton Rules. He felt very strongly that these rules ought to be annulled.

Section of Psychiatry.

President—Dr. CHARLES S. MYERS, F.R.S.

PRESIDENT'S ADDRESS:

On Consciousness.

By CHARLES S. MYERS, M.D., F.R.S.

(ABSTRACT.)

THE present classification of modes of consciousness into cognitive, affective and conative modes is recognized as unsatisfactory. The cognitive consciousness, in so far as it involves self-activity, is itself conative. The affective consciousness comprises far more than affective tone—pleasure and displeasure. (Indeed, I consider it doubtful whether these two are as primitive and elementary as has hitherto been supposed.) It may cover every kind of feeling, not merely emotional, but also intellectual—feelings of familiarity, certainty, relationship, &c. A more acceptable scheme, if it included feeling, would be to classify the modes of consciousness on the basis of a distinction between doing and what is done, between the act or process and its product, e.g., between the act of remembering and what is remembered, the process of sensing and the sensation, &c. According to this scheme, consciousness is classifiable along the lines of self-activity and the outcome of self-activity.

It is fairly obvious that if only one response to a given stimulus were possible, consciousness would be of little value and would hardly have arisen. Its utility arises when (a) alternative reactions are possible, (b) when some sort of choice has to be made between them for the benefit of the organism as a whole, and (c) when, as in an important emotional situation, it is essential that no other conflicting reaction be made by the self to another simultaneous experience.

Consciousness is thus a selector, by the individual, of alternative responses. It is also a selector of alternative stimuli. If, owing to uniformity of external conditions or to the immobility of the organism, the organism were unable to alter its relation to the environment, consciousness again would hardly have arisen. One of its most important functions is to maintain an environment that is favourable and to avoid one that is unfavourable, whether that environment be a physical one, say in the form of external nourishment or temperature, or a mental one, say in the form of appropriate or noxious thoughts.

These, then, are the main functions and conditions of consciousness; and in realizing them we realize still more clearly how all consciousness must form part of "self-consciousness," by which I mean not necessarily the knowledge, the awareness of the self, but the involvement of that highest integration of activities which is known as the self within the indivisible, "individual" organism. Consciousness thus functions as the co-ordinator of all the past and present experiences of the organism so as to give direction to the selection of its future activities and environment, in compliance with the organism's sanctions and ends.

It has been too common an error to regard the simple in life as prior to the complex. We are apt to forget that the most lowly unicellular organism eats, breathes, secretes, excretes, reproduces, and exhibits irritability, contractility, and even apparent choice and ability to learn by experience, whereas in the higher organism such functions are specialized in its different tissues. The vague complex,

in other words, precedes the differentiated simple complex. So it is largely in the life of consciousness. Simple sensations are not the first experiences. The first post-natal or even pre-natal experiences are vague affections of the self—or of what will come in time to constitute the self, as later it becomes differentiated from external situations, and as later external objects come in turn to be differentiated from external situations. At quite an early period many of the infant's experiences, especially the visual, become "projected" first as situations, then as objects, instead of being, as at the very outset, little more than affections, so to speak, of the self. It is only gradually that the separate, simple sensations, say of whiteness, softness or sweetness, are differentiated from these objects. But sensations, we must remember, are not wholly projected. Red, for example, clearly resides in the object, but pain lies in ourselves, while such sensations as those of temperature and taste occupy a half-way position.

The observations of Head and Gordon Holmes, in conditions of thalamo-cortical interference, indicate that this projection may be lost in lesions of such sort. A prick may be no longer projected as such, but described by the self as a characteristic change in an affection of itself. Indeed, under normal conditions, the less projected the sensation, the more it approximates to an affective modification of the self. Titchener is doubtless right in believing that sensations (hence cognitive states in general) have become evolved out of a consciousness resembling feelings.

I believe that this power of projection, the ability of the self to regard its own change of state as something outside itself, is of far greater importance than is generally supposed. It surely culminates in the self looking down, not merely on external independent objects, but also on its own *other* selves who come to be regarded as acting under its jurisdiction. Thus, the most consummate actor is said to be he who, though he feels the emotions he portrays, experiences them in such a way that it is as if he were looking upon another self that actually felt them. Thus, too, "sportsmanship," "fair play," tact, &c., become possible. Something of the same effect, though doubtless of different causation, is observable in that transient, slightly pathological condition known as "depersonalization," common to large numbers of us, wherein the external environment appears for the moment as strange, and we seem to be looking on another instead of on ourself as really experiencing it and acting on it. It occurs still more strikingly, of course, in the more definitely morbid condition of loss of reality. Similar processes may account for the alternation of personalities behind which there lies a continuous personality that knows the acts and experiences of the others. The well-known limits of suggestibility in the post-hypnotic state indicate the same preservation of a higher, dominant, however dormant, self. The integrity, the intactness of this supreme self may prove, I even suggest, to be the future criterion between so-called psycho-neurotic and psychotic conditions.

While the simple is, so to speak, distilled from the wider complex, nevertheless synthesis goes on as well as analysis; and many instances will easily occur to my audience in which new experiences are dependent on an integration of stimuli or of more primitive experiences. On the one hand, where a reflex occurs or a habit is acquired, consciousness is useless, since the stimulus inevitably releases one and only one reaction. On the other hand, where an instinct appears, consciousness (let us call it *instinct feeling*) is essential, because intelligence can be brought to bear so as to improve by growing experience the instinctive reaction to the *situation*. Where emotion enters, the number of alternative conflicting, instinctive reactions to a stimulus has become manifold, e.g., in the case of fear, flight, rigidity, flaccid palsy, crying, clinging to the parent, fighting at bay; and their respective instinct feelings become integrated about a common *object* on this higher plane to create *emotional feeling*. Where sentiment enters, a number of alternative emotional feelings have

become integrated about a common *idea*, and a new *sentiment-feeling*, e.g., that of love or hate, emerges.

The importance of the integration of such alternative, often conflicting mental states, as the creator of new ones, can hardly be overestimated. Rivers has, from the ethnological standpoint, attributed new cultures to the clash of immigrant with indigenous ones; and it seems possible that the creations of the inspired genius may be the unconscious product of similar conflict.

Let us now consider what we know of the activity of living substance. It exists in two forms—(i) intensive and momentary, (ii) moderate and prolonged. The contractions of striated muscle illustrate intensive and momentary vital activity; the reactions of heat and cold spots offer another example. There appears to occur a firing-off of already-prepared, explosive material, followed fairly rapidly by fatigue. The contractions of unstriated muscle illustrate the more moderate and prolonged form of activity, where tone and long-continued adaptation seem to replace the explosive force and consequent fatigue characteristic of the first-mentioned form. Again, the tone and posture in striated muscle, both of them moderate, long-continued and relatively indefatigable, illustrate the same form of activity. They involve a directive balance, a delicate nervous co-ordination between two opposing muscles, flexor and extensor. The sensations of warmth and coolness depend on a similar mechanism. In contrast to the *spot* system subserving heat and cold, this *diffuse*, spotless sensibility involves a close co-ordination between the mechanisms for warmth and coolness, as is exemplified first in the set state of balance that occurs in the form of "sensory adaptation"—a kind of stationary "posture," as it seems to me, between the opposing mechanisms of warmth and coolness, when the skin is exposed for some time to a warm or a cool environment—and, secondly, in the action resulting from disturbance of that balance, as occurs in the form "sensory contrast," when that environment is suddenly replaced, say, by a neutral one. Such phenomena as adaptation and contrast do not occur in the spot system; there we have merely sudden, ungraded reaction and fatigue.

Thus we come to contrast powerful, energetic, explosive acts, followed by a loss of available material for the allowance of further acts, on the one hand, and the more moderate, more graded activities, on the other hand, involving reciprocal inhibition and facilitation, and finally yielding a long-continued "set" or state of adaptation or attitude.

May we not usefully distinguish these two forms throughout mental activity, even up to the highest conscious processes? In other words, have we not, on the one hand, the momentary, relatively fatiguable acts of apprehension, recall, decision—of action, in general—and on the other the long-continued, relatively persistent sets or attitudes—mental *postures*, if you like—in which those varying acts take place? On the one side we have the more mechanical acts, on the other the more directive attitudes, though, of course, the acts themselves are far from being devoid of a certain co-ordination and direction. We recognize thus in mental activity a more mechanical factor and a more directive factor, each involving the expenditure of work; but whereas we have some conceivable idea of the nature of the former, we have none whatever of the nature of the latter.

We might well pause, did time permit, to consider what is the effect of profound morbid changes in attitude on the consciousness of acts. Two obvious and opposite directions of change at once present themselves. At the one extreme, attitude is unusually persistent and unvaried: according to the old dictum, *semper idem sentire ac non sentire idem est*. At the other, attitude is to all intents and purposes non-existent; the mental acts follow one another over a vast field whose meaning is changing with bewildering speed.

The early neglect of the importance of affects has resulted, I think, in an

exaggerated swing of the pendulum of opinion to the opposite extreme. It is, I think, ridiculous to suppose that the energy informing our actions is derived solely from our affects. We may, however, reasonably consider the view that, using the term in its widest sense, our attitudes serve as keys that unlock the energies resident in our acts. It is also ridiculous to suppose that our acts depend for their energy on drainage from one set of channels to another. The modern studies of repression alone suffice to prove that censoring, as the Freudians term it, involves actual work in the imposition of resistance. The mind is not comparable, as according to McDougall, to a vast sewerage system, in which the active channels drain off energy from those which are *ipso facto* rendered inactive; inhibition involves as much work as excitation.

The same exaggerated importance of the affective consciousness has led to the attribution of all forgetting, every slip of the tongue, to emotional conflict and inhibition. Surely prolonged laboratory experience in learning large numbers of senseless syllables, or in rapidly naming long series of familiar objects, suffices to show the extravagance of this view. Deterioration or disorder in cognitive processes is not always dependent on affective factors. We must recognize that the act may suffer through excessive exercise, as well as through inhibition; at the same time fully admitting that the attitude which suffers mainly through flagging interest or conflicting feeling may also thus influence the act.

That some central factor of general intelligence exists depending on the functioning of the highest system of mental activities known as the self, there can be little reasonable doubt. But its conception is also commonly bound up with that of localizing the various conscious processes, which are *not* those of general intelligence, in different regions of the brain. For generations past, it has been customary to believe in special centres for the various motor, sensory and perceptual activities involved in speech, and to regard them as "seats of consciousness," connected with one another and presumably with that highest central centre of the self or ego. It is interesting to find that the recent researches into aphasia by Head have enabled him to produce cogent evidence against such a view. For my own part, in my Cambridge lectures, I was long accustomed to protest against it by means of the following illustration: if I wanted to travel by rail from Cambridge to King's Cross, it would be essential for me to pass through Hitchin. A block at Hitchin would prevent my arrival at King's Cross. But I should not be justified in confusing Hitchin with King's Cross and in transferring the block at Hitchin to King's Cross. So too, if a certain occipital area must necessarily function in order that, say, an apple may be perceived as such, I should not be justified in describing that area as a "visuo-psychic centre," because I fail to apprehend an apple when that area is disorganized. All that I can legitimately infer is that that occipital area is essential for visual perception, just as Hitchin is essential for me to reach King's Cross. Seats of different consciousness must not be thus fallaciously localized in relatively small different areas of the cerebral cortex.

A similar error endangers Head's earlier conjecture (in his work with Gordon Holmes) that the thalamus is the "centre of consciousness for certain elements of sensation",¹ which he infers from the results of interference of the normal connexions between the thalamus and the cerebral cortex. No doubt he would himself admit that if we interfere with a small part of the central nervous system, it is impossible to suppose that that part remains the seat of conscious processes. Consciousness depends on the self. The activities of the thalamus can only affect consciousness by forming part of those activities which contribute to those of the self. All that we can safely infer is that when the thalamus is "separated" from the cerebral cortex its activities affect the self in a manner different from that in operation when its

¹ *Brain*, 1911-12, xxxiv, p. 181.

normal relations to the cortex are intact. We cannot posit a "centre of consciousness."

This brings me, in conclusion, to a brief study of the nature of consciousness itself. There was a time when mind was regarded as the product of the brain just as bile is regarded as the product of the liver. This was succeeded by an age when mind and living matter were considered to be so absolutely different in character that for this, if not for any other, reason, it seemed absurd to compare mind with bile as a secretion of living substance. Instead, there grew up, on one side, the theory of psycho-physical parallelism—that mental and neural processes are two different aspects or reflexions of one and the same unknown activity—and, on the other side, the theory of interactionism,—that so far from being parallel they are independent and may each, according to circumstances, influence and control the other.

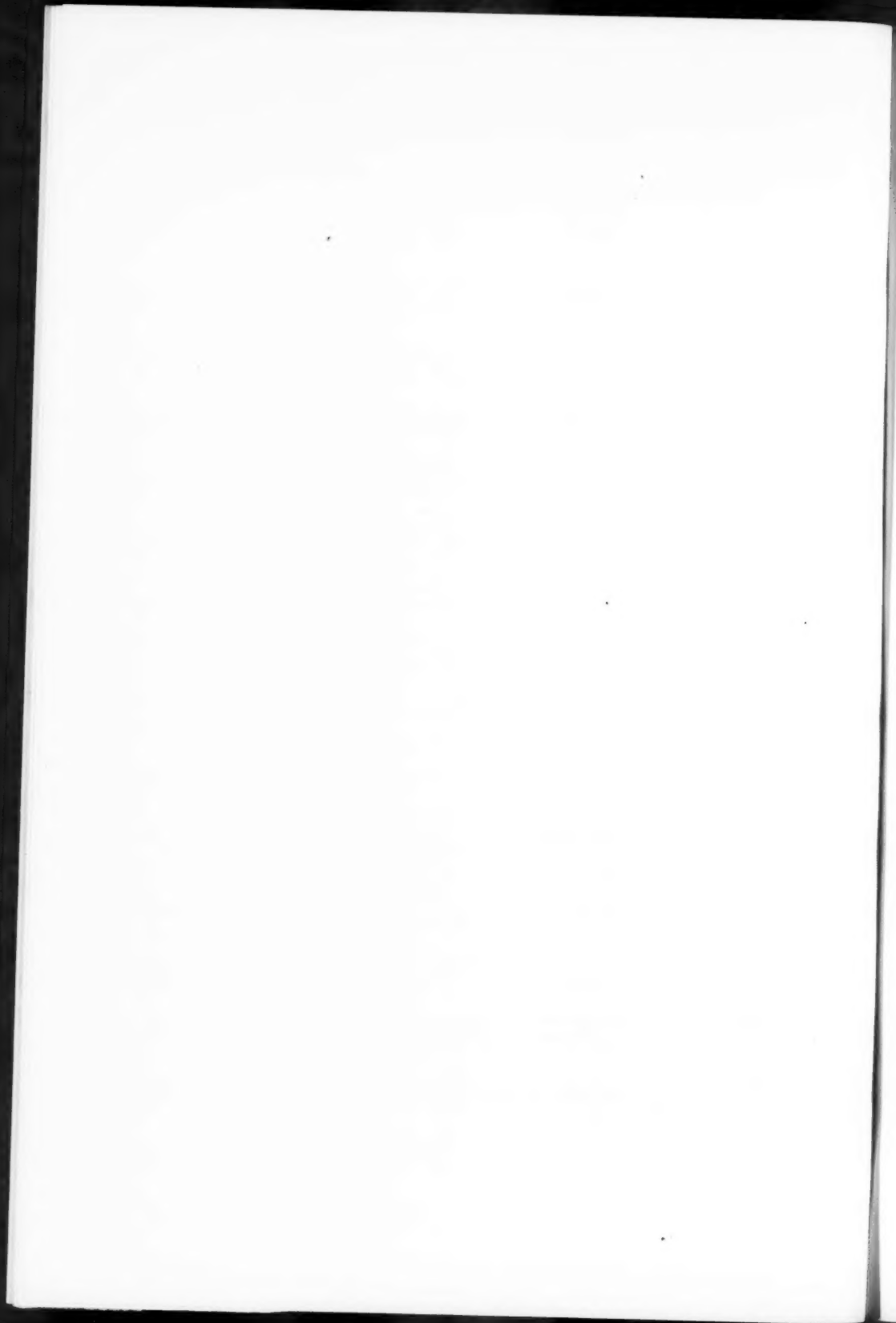
Times have now changed. Substance proves no longer as "substantial" as it appears. We are no longer content to regard matter as composed of solid atoms. The structure of the atom is now revealed to us as a constellation of ions, each of which appears to be merely a *point d'appui*, of definite size, for a charge of electrical energy, thus endowing the "point" with inertia, mass, weight, &c.

What now of mind? Might not this likewise be a manifestation of energy? Is the difference between mind and matter so fundamental as it appeared a hundred years ago? Are not the hidden *activities* of mind and matter of greater import than their more obvious *products*? Are not the respectively material and mental characters of these products due ultimately to the activity of mind itself?

When we come to consider the difference between mind and *living* matter, the distinction is reduced almost to vanishing point. For there are essentially the same purposive, directive, plastic and constructive characteristics, distinguishing living from dead matter, which, raised to a still higher power, distinguish both nervous and mental activity from the activity of other living tissues. The problems of life that confront the physiologist are precisely those of mind that confront the psychologist. Life and mind must ultimately be solved in similar terms. From each we can abstract the mechanical, comparable to what we know of the activities of lifeless matter. But it may well be that the apparently blind mechanism of which physics treats is only an abstraction from a purposeful direction that plays its part in the larger universe regarded as an organism, just as we are bound to conceive of such direction even in the lowest living individuals, even in the lowest physiological levels of the higher, living organisms.

With progressive evolution of these various levels have "emerged," according to the recent terminology of Lloyd Morgan and Alexander, the various levels of mental activity. It may be that the term "differentiation" will often be found more useful than that of "emergence," so often is the new really pre-existent—though in a primitive, vague, undifferentiated state—in the old. But, admitting also the "creation" of new forms with the progress of evolution, may we not sometimes regard the mechanical forms of activity as being really a degradation of still higher forms? Because electrical energy is the only energy in which that of the nervous system reveals itself to us, can we deny the possibility that this is a degradation of some higher, what I may term "psycho-neural," energy, which assumes a more psychical character in the highest levels of the nervous systems of the most highly organized individuals whose wider and more plastic areas are more complexly integrated together to function as a single entity?

These are among the problems which seem to be worthy of consideration in the broader study of consciousness.



Section of Surgery.

President—Mr. HERBERT J. PATERSON, C.B.E., M.C.

The Lure of the New : PRESIDENT'S ADDRESS.

By HERBERT J. PATERSON, C.B.E., M.C.Cantab.

IN occupying this Chair for the first time it is my very pleasant duty and privilege to thank you most sincerely for the great honour you have conferred upon me by electing me as your President for the coming year. I can assure you that I esteem it as one of the highest distinctions that has fallen to my lot. I am fully conscious of the difficulty—nay the impossibility—of living up to the standard set by my immediate and distinguished predecessors, but I will do my best, and endeavour to further in every way the interests of the Society and of the Section of Surgery; and I ask for your whole-hearted co-operation in efforts to increase, not only the interest in our discussions but the attendance at our meetings.

I am reminded of a stanza attributed to Rockefeller:—

"A wise old owl lived in an oak,
The more he saw the less he spoke,
The less he spoke the more he heard,
Why can't we all be like that bird."

I do not lay claim to the wisdom of that old owl, but I should have been glad to have emulated his silence, had not your Council decreed that there should be a Presidential address. Theirs, therefore, is the responsibility, and it is for me to claim your indulgence. This much at least I can promise, that during the remainder of the session I will as far as possible follow the good example of that old owl by keeping silent and learning from the wisdom of others.

I often wonder what some of the great surgeons of forty or fifty years ago would think, could they visit a modern surgical clinic to-day. Many of you may remember how Erichsen in the introduction to his text-book of surgery—still one of the best text-books in existence—takes upon himself the dangerous rôle of a prophet.

"Operative surgery," he wrote in 1884, "like any other art, can only be carried to a certain definite point of excellence; there cannot be always fresh fields for conquest by the knife. There must be portions of the human frame that will ever remain sacred from its intrusion—at least in the surgeon's hand. That we have nearly, if not quite, reached these limits there can be little question. To my mind it appears as if we had already reached something like finality in the mere manipulative part of surgery, and in this direction the progress of modern surgery is nearly barred."

Yet at that period the practice of antisepsis was in its infancy; its principles were accepted by few and scoffed at by many. Operations for appendicitis and adenoids which have so materially added to the surgeon's hard-earned gains were unthought of. Gastric surgery was but newly born, the operative treatment of perforated gastric ulcer had been suggested, but it was not until eight years later that the first successful case was reported, and nine years elapsed before Doyen employed gastro-jejunostomy as a substitute for excision in the treatment of gastric ulcer. Yet he would be a bold man who ventured to say that at the present time there are not still further fields for conquest.

NEW LAMPS FOR OLD.

Huxley cynically remarked that the "customary fate of new truths is to begin as heresies and end as superstitions." Nevertheless we are ever seeking new lamps for old. Some of our new lamps have a dazzling brilliance, but their endurance is transient; others shine as beacon lights on the pathway of progress. Although every inventor or originator is not necessarily an Aladdin, he plays a part of supreme importance in surgery, as in every other field of human activity. Whether his ideas be trivial or important, he stimulates thought. Like Lister, he may change the whole practice of surgery, or he may merely make some small suggestion which ultimately may prove part of an epoch-making discovery. His new lamps may be useless for the purpose for which they are designed, but in other directions they may prove of extreme value. Sayre's plaster jacket was intended for the treatment of scoliosis. The method was sound, but its application to this particular disease was erroneous; but by drawing attention to this subject in time it led to the enunciation of the correct principles on which lateral curvature should be treated. As an illustration of the way in which sometimes we cling to new lamps, even though they give but little light, it is interesting to notice that although Sayre's treatment was condemned almost at once, it flourished in spite of adverse criticism for nearly twenty years.

The birth of modern intestinal surgery may be attributed to an invention long since discarded—Murphy's button. The introduction of this appliance demonstrated the possibilities of intestinal anastomosis and, by focussing interest on this subject, led to methods which have superseded the use of mechanical appliances; nevertheless, although no longer used, Murphy's button is a landmark in the progress of abdominal surgery.

THE MISUSE OF NEW LAMPS.

Not seldom the new lamps given to us by inventors are spoilt by injudicious modifications or improper use. A case in point is Murphy's method of continuous proctoclysis. The whole secret of its successful use depends on having a rectal tube of large calibre so as to permit of a free flow, not only from the can into the rectum, but from the rectum back into the can, if there be any expulsive efforts in the intestine. I remember telling the late Dr. J. B. Murphy, during one of his early visits to this country, that his method was being adopted extensively. "Yes," was his reply, "it has often been shown to me, but I would not have recognized it had it not been for the label." When Sir Arbuthnot Lane introduced his method of plating simple fractures, some surgeons employed it for the treatment of compound fractures, with unsatisfactory and often disastrous results. They were putting a valuable procedure to a use to which its originator stated it was totally unsuitable. I do not suggest that a new invention cannot be improved, but before attempting to make improvements it is wise to ascertain whether our ideas have already been tried without success. When, more years ago than I care to calculate, I introduced the method of nasal anaesthesia, some anaesthetists thought the apparatus could be improved by the addition of an expiratory valve, and it was interesting to watch the various experiments which were made, which had already been tried and found wanting.

NEW LAMPS HAVE DISADVANTAGES AS WELL AS ADVANTAGES.

I think it will be conceded that even the greatest discoveries have disadvantages as well as compensations. Had Nelson, at the battle of Copenhagen, been in wireless communication with the Admiralty at Whitehall, he would not have been able to plead his blind eye as an excuse for disregarding the signal, "Cease action." The two greatest blessings given to surgery during the past century, anaesthesia and the

principles of asepsis, are not exceptions to this generalization. Both have tended to prolong operations unnecessarily, so that to this extent they have been harmful, although the sum of the benefits they have conferred is priceless. In pre-anæsthetic days the surgeon tried to complete his work as speedily as possible, because he was inflicting pain. With the introduction of anæsthesia the surgeon could be more deliberate in his methods and so unconsciously prejudiced the interests of his patients. In this respect the war was a bad training-ground for young surgeons. Many long and very severe operations were performed for abdominal injuries, but the natural resistance of the patients was very different to that of the ordinary patient of civil life. The fighting men were in splendid physical condition and could stand almost anything, and so the young surgeon gained a very exaggerated idea of what an ordinary patient can stand. Such operations in military work were inevitable, and many of them brilliantly successful, but if attempted in civil life would almost certainly prove fatal. One of my former house-surgeons resected two separate portions of ileum and sutured six or seven perforations in the stomach and intestine. Most of the mesocolon of the transverse colon was destroyed, but with rare judgment he decided to risk gangrene of the colon, and so wisely refrained from adding resection of the colon to what the patient had already undergone. His judgment was justified and the officer made a splendid recovery.

In his fascinating Presidential address two years ago Mr. James Berry emphasized the value to the young surgeon of a thorough training in the making of post-mortem examinations. I would add to this wise advice that, in addition, some experience as an anæsthetist should form part of the training of anyone who aspires to practise surgery. He would learn not only many useful surgical lessons, but he would understand more of the shock produced by operations. The drugs used to produce an æsthesia are highly toxic, and we are apt to forget that, apart altogether from the effect of the operation, the anæsthetic also has a harmful influence on the patient. Every experienced anæsthetist knows that even the strongest patient begins to flag perceptibly after an hour, and after an hour and a half this change is still more accentuated. Dr. R. T. Morris, of New York, relates how a house-surgeon who had served under two surgeons of about equal capability, was asked his reason for the difference in their statistics. His reply was: "About ten minutes." There is a wealth of wisdom in this seeming exaggeration. The expert anæsthetist, not being a politician, keeps our patients in such a state of tranquility that we are tempted to be almost too deliberate, and to forget the importance of rapid or rather expeditious operating. For rapidity must not be confused with hurry. Our aim should be the quickness that comes from knowledge of what to do and how to do it, the experience which teaches us to come to quick and right decisions, and the long practice which enables us to complete our task without unnecessary moves and needless manipulations. An expeditious and careful operation preserves the patient's strength and conserves his immunity. As Monod, of Paris, said more than 150 years ago, the adage "*tuto, cito, jucundo*" applies to surgery as much as to medicine. We should operate "safely, quickly and pleasantly."

There can be no doubt also that the practice of asepsis has tended, sometimes unduly, to prolong operations. I happened to be house-surgeon at my old school at the dawn of the antiseptic period. My chief was the first surgeon to robe himself in a clean white gown, and we started the practice of boiling not only our instruments but our ligatures. Previously all the surgeons used to operate in old frock-coats, which were kept in a dark and dusty cupboard under the gallery, coats so plastered with the blood and pus of years that they were as stiff as if they had been starched. One afternoon one of the junior surgeons, a recent convert to antiseptic principles, was preparing to operate on a very fat woman suffering from an enormous strangulated umbilical hernia. She was duly anæsthetized and although she was profoundly

ill from toxæmia, the surgeon, who was of noble presence and athletic build, proceeded to spend a considerable time in pounding the abdomen vigorously with a slab of soap and a liberal supply of water. During this strong-man demonstration, Sir William Savory, one of the bitter opponents of the new antiseptic methods, walked into the theatre. Ceasing for a moment from his ablutionary labours, the surgeon looked round with a smile and said, "at any rate, Sir William, you don't object to this?" "Object to what," replied Sir William, "the massage?" Like the over-conscientious actor who blacked himself all over for the part of Othello, we may be too thorough. Some of you may remember what elaborate methods used to be adopted in the attempt to get rid of sepsis from the abdominal cavity. It was irrigated with many pints of water, some surgeons even left the wound open, and put the patient in a bath with tubes "Medusa-like" projecting from numerous incisions. These measures were euphemistically termed the "toilet of the peritoneum"! They might have been described more aptly as the massacre of natural resistance. It was many years before surgeons realized that Nature could deal with infection if her proper powers of resistance were conserved and not injured by prolonged operation and unnecessary manipulation. I remember at one of the discussions at the old Medico-Chirurgical Society venturing to remark that my experience as an anæsthetist had taught me that irrigation of the peritoneal cavity was not only futile but by causing severe shock seriously prejudiced the recovery of the patient. I was somewhat seriously handled for my presumption by one of the learned seniors present.

Even as late as the year 1907, Jacobson, in his text-book of operative surgery, disagreed in kindlier manner with my opinion regarding the inadvisability of irrigation in operation for perforated gastric ulcer. To-day, we wonder why such heroic measures were countenanced for so long. I refer to them now to suggest that we ponder our present practices. Maybe, even now, we are employing some procedure in technique which our descendants may regard as foolish and futile, as we do some of those which were practised in the past. Let us each ask ourselves whether our technique may not be simplified without loss of efficiency. It is not, however, my intention to criticize others. I am too conscious of my own mistakes. For the onlooker it is always easy to criticize, and hard to realize all the difficulties.

"If four play whist,
And one looks on,
They make blunders,
He makes none."

THE LURE OF REFINEMENT.

By way of illustration may I give two examples of how I myself have strayed from the *via media* of common sense. For many years it has been my practice to change my gloves just before sewing up the abdominal wound. In a clean case there is justification for this practice, but in a septic case, when the abdomen is filled with septic fluid and the abdominal wound is already saturated with septic material, surely it is futile, unnecessary and harmful. I dare say no one here has committed such folly, and I wondered why I continued it for so long. It prolongs the operation for two or three minutes, in cases in which every minute lost diminishes the resistance of the patient. So, too, with regard to the burying of the stump of the appendix in appendicectomy. In clean cases, there is no objection to it, it is neat and possibly may diminish the risk of adhesions. It is however, quite unnecessary from the point of view of safety. In acute cases it delays the completion of the operation by two or three minutes. These little delays soon mount up. We cannot pay too much regard to Murphy's dictum: "Get in quick, and get out quicker." For several years I have been content in acute cases simply

to tie a ligature round the appendix at its base and cut it off, and I have not experienced a case of leakage. I think, too, some of us may have over-elaborated our pre-operative treatment, not the duration of it, but the vigour of it. For instance I used to give repeated small doses of calomel, having regard to its value as an intestinal antiseptic. But purgation and loss of fluid are a bad preparation for an operation. For some years I have given up the use of all purgatives both before and after operation, even the classical enema regarded with such veneration by the nursing profession has been abandoned, and I rely entirely on liquid paraffin. The cleanliness of the intestinal tract can be secured by a diet of sterilized milk for several days. Since adopting these less vigorous but equally efficacious methods, the gain in the patient's comfort and well-being after operation, and freedom from anxiety to myself have been immeasurable. No doubt we should aim at the ideal, but in our efforts to do so we must beware of the lure of refinement, over-elaboration in unnecessary detail. We must not make a goddess of technique and forget that it is of no less importance to assist Nature by conserving the patient's resistance. In our attempt to secure perfect asepsis we may so prolong the operation that the patient's chances of recovery are prejudiced. Better incomplete technique and a live patient than a perfect technique and a dead patient. I am not sure that a good case could not be made against the use of rubber gloves. At any rate Sir Harold Stiles without them has achieved results surpassed by none. Lawson Tait, too, showed what could be done with a simple technique and expeditious operating. His results were so good that the surgeons of his day took refuge in regarding their accuracy with an attitude of unphilosophic doubt.

THE LURE OF SHORT CUTS.

Mr. Berry has defined a good surgeon as "one who always knows when to put in and when to take out a drainage tube." It would indeed be difficult to make a more searching test of a surgeon's judgment. Of late years the use of drainage tubes has not been fashionable. The old dictum of Lawson Tait, "when in doubt, drain," is considered out of date and is regarded as a confession of ignorance on the part of an indecisive and stagnant mind. A school has arisen which regards drainage as rarely necessary, and a procedure to be avoided as far as possible. This heresy—for heresy I feel strongly it is—was fostered by the surgery of the war. The excision and primary suture of septic wounds became a recognized procedure at the front, and the results were stated to be superior to other methods. I think most would agree that a casualty clearing station is not a good observation post for taking a long view. Operations were performed, the patients sent to the rear, and in the rush and turmoil of fighting it was not possible for the operator to learn what was the final result of his work. Possibly we have not yet the material available for a final judgment, so I can only state my own experience—an experience which I know is shared by many others. I saw very many patients who had had excision and primary suture of wounds, and I know of no case in which one of two things did not happen; either the wound broke down, or it had to be opened up for purposes of drainage. I confess I have not yet been able to ascertain what are the objections to drainage, except that there is a possible risk of hernia at the site of the tube. This, after all, is a small matter. A living patient with a hernia is better than a corpse with a perfect closure. The advocates of non-drainage in septic abdominal conditions are careful to point out that there is no special "sepsis resisting" virtue inherent in the peritoneum; in this respect it behaves just as other tissues. Having emphasized this point then they proceed to give elaborate details for the drainage of the abdominal wound. Surely what is sauce for the abdominal wound goose is sauce for the peritoneal gander! If the abdominal wound requires drainage, it follows, if their premises are correct, that the peritoneal cavity should be drained also.

Some principles in surgery may be regarded as immutable, and surely this is one of them, that sepsis requires drainage. In this I prefer to follow that great master of the surgical craft, Dr. W. J. Mayo, whom I have heard say: "Take no chances, when in doubt drain." Beware of the lure of short cuts. Drainage does no harm and I believe it might be employed to advantage more often. May I add an aphorism of my own. Drainage tubes are sometimes left in too long, but they are more often taken out too soon.

THE LURE OF THE LABORATORY.

To the surgeon of to-day there is perhaps no greater lure than the various methods of investigation of disease which have been placed at his disposal during recent years. The modern student and young practitioner are so ready to look to these outside helps, that there is a real danger of clinical observation becoming a lost art. A few years ago my house-surgeon rang me up one evening to tell me that he had just admitted a young woman who required immediate operation for acute appendicitis. After some preliminary questions I inquired whether she was tender over the appendix area. "I have not examined her abdomen," was the reply, "but I have done a blood count, and she has a leucocytosis of 13,000." No one would wish to belittle the value of scientific methods; to do so would be as absurd as to depreciate the wisdom which comes of experience. It may not, however, be amiss to suggest that none of these methods are infallible. Laboratory methods are an aid but not a substitute for clinical observation. If they confirm the opinion formed after a careful and thorough examination of the patient, they are of great value; if not, they must be received with caution, even with scepticism. Especially, we must be on our guard against making the results of our observation fit in with the laboratory reports. If once imagination be allowed to come into play, our observations may be easily so distorted as to lead us astray. I remember, when a boy, reading the story of the burning of the Tropical Annexe at the Crystal Palace. The horrified spectators watched a monkey which had gained a position on the roof, but whose escape was cut off by the flames, being slowly roasted to death. Some of the spectators even heard the poor beast's agonizing screams. Later, it was discovered that the monkey who had excited such compassion was nothing but a piece of tarpaulin! Do we not sometimes forget that patients have tongues, for inspection, if for no other use? There are occasions when a glance at this unruly member will afford more information of value than pages of laboratory reports. To the ultra-modern surgeon it is too simple; like Naaman of old he prefers something more dramatic, although if he only realized it, less effective.

THE LURE OF THE BIG IDEA.

Not one of the least dazzling lamps given to medicine in recent years is the hypothesis that intestinal stasis is responsible for many of the ills to which our flesh is heir. It is alluring in its simplicity and fascinating from the vista of hope which it opens up. It has been given to few to originate two such startling proposals as the operative treatment of simple fractures and the surgical treatment of intestinal stasis; and Arbuthnot Lane has not escaped the criticism and bitter opposition which falls to the lot of all pioneers. Few subjects have excited greater controversy than the statement that intestinal stasis produces toxæmia. It has been assumed that such teaching is unscientific, because we do not know the nature of the toxins produced or even of their existence. This argument need not perturb the seeker after truth. The malaise and debility which accompany tuberculosis were recognized long before the discovery of the bacillus which produces the toxins which are the cause of the constitutional symptoms. This much at least we do know, that patients who are the victims of a disorder which impairs their mental and bodily vigour, which

causes such constant discomfort and pain as to reduce them to a state of misery, and which renders them incapable either of work or enjoyment of the pleasures of life, may be relieved by treatment directed to the removal of existing intestinal stasis. This is a truth established by the work of surgeons all the world over. When, however, we consider to what extent surgery should interfere, there is room for wide divergence of view. We must examine the suggested remedy not only from the point of view of immediate mortality but of remote risks. The risk of subsequent acute obstruction from the numerous adhesions inevitable after such an extensive operation as colectomy is so great that it seems too hazardous a procedure, especially as the same effect may be gained by simpler operative measures. This does not detract from the merit of Lane's work. Whether all his teaching be true or not, time alone can show, but without doubt his lamp has cast light in hitherto dark places, and has led to a new conception of abdominal diseases. We know now that many of the symptoms formerly attributed to gastric disease are due to conditions elsewhere in the abdomen. Indeed, I doubt whether there is such a thing as primary disease of the stomach. There are good grounds for regarding gastric ulcer, duodenal ulcer, and possibly even gastric carcinoma, as secondary to some infection from the intestinal tract. *Magna est veritas et prevalebit.* If Lane's teaching be false, it will die, if it be true it will remain a beacon light in the surgery of the twentieth century.

THE LURE OF SPECIALISM.

During recent years the lure of specialism has cast its spell over the profession. In the medical schools there are so many special departments that the student can hardly hope to attend them all. In a recent examination the candidates were asked to describe the appearance of a Hunterian chancre. It was evident from the answers that few of them had ever seen one. There is a danger that in the future specialists will not have that solid foundation of general knowledge upon which the superstructure of specialism should be built. In our own Society many deplore that surgery should be split up into so many sections. Surely it would be to the good of all if discussions on orthopaedic and proctological surgery were part of the work of general surgery, and there are some who look forward to seeing these offshoots re-grafted to the parent stem.

I hope that during the coming session there may be at least closer union and interchange of ideas between us and the kindred Section of Orthopaedic Surgery and our own Sub-section of Proctology. It is, I think, of happy augury that the President of the Society (Sir StClair Thomson) has honoured us with his presence this evening.

THE LURE OF CUTTING OUT.

At the risk of being considered old-fashioned, I should like to refer briefly to the lure of cutting out. A battle has been raging between the "ectomists" and the "ostomists," and for the present numerical victory, at any rate, is with the "ectomists." Cholecystectomy is more fashionable than cholecystostomy, gastrectomy than gastro-jejunostomy, colectomy than colostomy, nephrectomy than nephrotomy. I would ask you to pause and ask yourselves whether there is a solid justification for this fashion? Consider for a moment one of these thorny questions, gastro-jejunostomy *versus* partial gastrectomy. Originally the reason given why gastrectomy should be preferred to the simpler gastro-jejunostomy was the assertion that in 70 per cent. of the cases of cancer of the stomach the cancer is grafted on a simple ulcer of long standing, and, further, that in all ulcers larger than 2.5 cm. in diameter cancer cells can be observed in the edge of the ulcer. I need not discuss the reasons given for these statements, as I am concerned here only with the teaching based on these allegations. For many years I have been asking this plain question: If it be true that gastric ulcer is so frequently the precursor of gastric cancer, why

is it that a large number of patients do not die from cancer after gastro-jejunostomy for supposed simple ulcer? Unfortunately the late Admiral Sir Percy Scott's intelligent midshipman has not come to the rescue, for I have as yet received no answer. This is not surprising, as the only two possible answers impale the opposition on the horns of a dilemma. Patients rarely die from cancer after gastro-jejunostomy for supposed simple ulcer because, either the ulcers are rarely malignant or because gastro-jejunostomy cures cancer. Those who teach that gastric cancer is usually grafted on simple ulcer can take what comfort they can from either answer. But of late years we have not heard so much of this hypothesis, and the preference for partial gastrectomy is now based on the alleged failure of gastro-jejunostomy to cure the ulcer. I am not prepared to admit that an operation is a failure which gives relief in nearly 90 per cent. of the cases. Further, failure is by no means always due to non-healing of the ulcer or recurrence of ulceration. Probably the number of cases in which these conditions obtain is under 5 per cent. Surely, then, it would be wiser to perform gastro-jejunostomy in the first instance and to reserve partial gastrectomy for those cases in which failure is due to persistence of, or recurrence of ulceration.

"Safety first" is a good slogan in surgery, and although in the hands of a few experts the mortality from gastrectomy may not be much higher than that from gastro-jejunostomy, nevertheless there is no doubt that partial gastrectomy is a much more severe operation than gastro-jejunostomy, and in the hands of the average surgeon has a much higher mortality.

It is significant that in spite of the multiplication of specialties, the general physician and general surgeon in their outlook and attitude of mind are converging rather than diverging. The physician of to-day must be a surgeon except as regards the craftsmanship of our common art, and if I, as a surgeon, may say so, there are physicians whose surgical judgment cannot be excelled. Equally, the modern surgeon, if he is to do his work with satisfaction to himself and with advantage to his patients, must have a wide knowledge of general medicine. He is no longer a carpenter, to operate at the behest of another. He is rightly expected to make his own diagnosis, and to form his own judgment. Indeed, a surgeon may be defined as a physician who has learned to use his hands.

CONCLUSION.

Art has been defined by Ruskin as "the operation of the hand and the intelligence of man together." Surgery is an art, and although in addition it is slowly and surely being built up into a science, it seems to me that the science must ever be subservient to the art. The words of Buckle, written some sixty years ago, are still true:—

"The greatest physiologists and chemists which the medical profession possesses are not necessarily the best curers of disease. If medicine were a science they would always be the best, but medicine being still essentially an art, depends mainly upon qualities which each practitioner has to acquire for himself, and which no scientific theory can teach."

With all the aids that modern science can give, there still remains the personal factor, call it judgment, clinical acumen, intuition or what you will. We must ever be on our guard that our judgment is not warped by the lure of the new. New lamps may add to our light but they cannot altogether replace the old.

Each new lamp offered to us must be tried and tested by the touchstone of experience. We must not let its dazzling brilliance blind us to its limitations or to its imperfections. Even when its worth has been proved by time, it must be used with discretion in its proper place and with that—

"Good sense which only is the gift of heaven
And tho' no science, fairly worth the seven."

In spite of the progress which has been made since Erichsen penned the words I have quoted at the commencement of this address, can we doubt that the Science and Art of Surgery is incomplete and incompletionable? We are aiming at a goal which we may never reach. Although to few comes the God-given privilege of giving new and imperishable lamps for old, most of us may place some small offering on the altar of progress, for as "Nature exists in leasts," so the humblest of us may discover a new truth, a new phenomenon, or make some seemingly trivial observation which in the future may play an important rôle in the promotion of human welfare. All of us at least may see to it that we use the lamps we have both wisely and well. The true wisdom of surgeons is experience. As Napoleon so aptly said: "The world is very old; so we must profit by its experience. It teaches that old practices are often worth more than new theories."

Section of Surgery.
SUB-SECTION OF PROCTOLOGY.

President—Mr. F. SWINFORD EDWARDS, F.R.C.S.

**A Case of Hernia of the Small Intestine through the Colostomy
Wound twelve years after Abdomino-perineal Excision of
the Rectum for Carcinoma.**

By Sir C. GORDON-WATSON, K.B.E., C.M.G., F.R.C.S.

PATIENT, a female, aged 56, married. In September, 1912, I performed abdomino-perineal excision for carcinoma of the rectum, from which she made a good recovery. Since then she has led an active domestic life, carrying out a daily wash-out of the colon. The colostomy has caused her no inconvenience, and she has been able to do household work as well as she could before the operation.

Twice during the past twelve years the even routine of her life has been interrupted: in 1920 she noticed that the abdomen was increasing in size, and in August, 1920, she was readmitted with a large abdominal swelling. I then removed a large ovarian cyst containing 11 pints of fluid. I had the opportunity at that time of inspecting the pelvic floor, eight years after abdomino-perineal excision. It was interesting to note that the pelvic floor was perfectly lined with smooth peritoneum, and that the pelvic cavity was free from adhesions. In fact, nature had so improved on my rough surgical toilet that it was difficult to realize that the pelvis was not fashioned as it was then found. The patient developed a considerable gap between the recti after this operation, and was obliged to wear a belt. She developed no symptoms of ventral hernia.

In September of this year (1924) she began to suffer from abdominal pain, which was located in the region of the colostomy, and for the first time she experienced difficulty in washing out the colon. She was readmitted to hospital, and it was found that there was a definite hernia through the colostomy wound. I operated on October 22, and found a hernial sac on the mesial side of the colostomy opening which contained some adherent omentum and a loop of small intestine. The hernia was reduced and the sac obliterated. Since the operation the colostomy has functioned well, and the patient is now in her usual good health.

Case of Prolapse of the Rectum of the Second Degree.

By J. P. LOCKHART-MUMMERY, F.R.C.S.

THIS is a rare form of prolapse, and is distinguished by the fact that a finger can be passed up between the prolapse itself and the rectal wall for some inches when the prolapse is down; in other words, this is a prolapse of the upper part of the rectum through the lower part, and operations such as my own operation for prolapse, which is effective in cases of prolapse of the first degree, would not be effective here.

I should be glad of opinions as regards the best methods of treating the case. The patient is a young man who has had the prolapse for about two years, and

[November 12, 1924.]

it came down for about 4 in. My own opinion is that the proper way to treat this case is by sigmoidopexy and fixing the sigmoid into the left iliac fossa, after turning up a flap of peritoneum over the left iliacus muscle. Resection of the prolapsing portion of the sigmoid would probably be effective, but would be a much more dangerous operation.

Case of Sacculitis of the Pelvic Colon with Localized Abscess. Death due to Portal Pyæmia.

By HAMILTON DRUMMOND, F.R.C.S.Ed.

History.—Patient, aged 64, very stout, was in his usual health on September 4, when he was suddenly taken with a severe chill and shivered violently. He sent for his doctor, who found him in the following condition: He was flushed; temperature 104° F., respirations 25; pulse-rate considerably increased. He had just had a severe rigor. He did not complain of abdominal pain, and it was thought that he was about to develop an attack of pneumonia.

Two days later no physical signs had developed, his temperature and pulse had returned to normal, and he felt much better. Several days later he developed bronchitis with rhonchi at the base of both lungs, and brought up a large amount of sputum, accompanied by a rise in temperature.

Twelve days later he had another rigor, and complained of abdominal pain, referred to the left iliac region, where there was slight tenderness on pressure. He became generally distended, and he vomited.

There was little change in his condition for the next few days, except that he had several rigors, his temperature and pulse at intervals returning to normal, and he was able to take food. The abdominal distension disappeared and his bowels were moved daily.

Bacteriological examination of blood and urine negative.

It was not until one month after the attack in the course of a repeated examination of the abdomen that a mass was felt in the left iliac fossa.

Condition on Admission to Hospital.—Appearance generally satisfactory; pulse 112, temperature 101° F. Tongue very dry, showing advanced chronic superficial glossitis. Jaundice suspected; urine very highly coloured, containing small trace of bile. Abdomen distended; patient complained of tenderness on deep pressure in left iliac fossa, where a large mass was palpable; his extreme obesity rendered abdomen difficult to palpate.

Per Rectum.—No mass, neither was he tender. Owing to patient's stoutness the examination was of little value. Wassermann reaction negative, blood showing marked leucocytosis.

Operation October 3, 1924.—A long muscle-cutting incision in the left iliac fossa exposed the mass, which consisted of an œdematous pelvic colon; this was pushed upwards by a mass in its mesentery; there were no intestinal adhesions, and as far as could be made out through the incision, the remainder of the peritoneal cavity was free from any inflammatory trouble. The mass lay between the leaves of the mesentery, and tended to bulge inwards more than outwards.

The abdomen was packed off with swabs and the abscess drained with a tube, which was put down into the mesentery of the colon on its outer aspect. Nothing more was done, and the abdomen was closed in layers of catgut sutures.

During the next few days he appeared to be somewhat better, but on the fourth day after operation he had a slight rigor, the temperature rising to $101^{\circ}6'$ F. On the following day he developed retention of urine, which persisted up to the time of his death. He had another rigor on the tenth day after operation (and at this stage

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became very drowsy and excreted less urine), and another one on the fourteenth day after operation; this last was most severe, and he died an hour later.

Post-mortem Examination.—General peritoneal cavity clean; abscess found lying between leaves of mesentery of short pelvic colon; this had burrowed upwards and downwards for a considerable distance.

Liver distinctly fatty, containing several small pin-point abscesses. Portal vein patent, but small vessels in region of abscess thrombosed.

DESCRIPTION OF SPECIMEN.

Outer aspect of pelvic colon showed no evidence of gross inflammatory change or sign of growth or sacculitis; the mesentery was the only place that showed any change (suppuration).

On opening pelvic colon multiple small sacculi were seen, some extending into the appendices epiploicae, and one, opposite mesenteric border of gut, showed a perforation directly communicating with an abscess outside the bowel between the leaves of the mesentery. The pus had laid bare the vessels which were exposed in the same way as the blood-vessels are exposed in a tuberculous cavity in the lung.

The abscess in the mesentery contained about 2 oz. of foul-smelling pus.

A Case in which Pathological Sacculi of Large and Small Intestine were Present in the same Person.

By HAMILTON DRUMMOND, F.R.C.S.Ed.

THIS case is brought before the Sub-Section because the sacculi were generalized throughout the large and small intestine—so bearing out the theory held by many that pathological sacculi are largely due to a congenital muscular weakness of the hollow viscera, the sacculi themselves being acquired, not making their appearance until adult life.

I recorded this fact at the Discussion on Diverticulitis by this Sub-Section in January, 1920¹; in the course of investigation of 500 consecutive post-mortem examinations on old persons, there were twenty-two cases of pathological sacculi in the large intestine, in four of which there were in addition sacculi in the jejunum. This case makes the fifth such case.

Patient, a feeble man, aged 70, had been subject to attacks of cramping pain in the abdomen complained of as in the epigastrium, for several years. This pain had a definite relation to the taking of food, coming on shortly after eating, and associated with a feeling of fullness in the stomach. At times he could feel a lump in the epigastrium. For the last two months the attacks of pain had been much more severe. For many years he had suffered from constipation; he had never passed blood *per rectum*, nor had he lost weight or vomited. His general condition was not good; he suffered from recurrent attacks of asthma.

Abdominal Examination: A hard mobile mass moving freely on respiration and not tender found in the right hypochondriac region.

Per rectum: Negative. A barium sulph. enema showed that the salt flowed through the large bowel as far as the splenic flexure, where it remained.

A diagnosis of chronic intussusception of the colon was made. The abdomen was opened through a long middle-line incision, the mass proved to be the right lobe of the liver. There was no growth in the colon, but the upper jejunum was the seat of multiple sacculi opening into the leaves of the mesentery, three of which were much larger than the rest and were situated a few inches from one another. They

¹ *Proceedings*, 1919-20, xiii (Sect. Surg., Sub-Sect. Proct.), p. 74.

were all thin-walled and much distended with intestinal contents; the lowest of the three showed recent signs of acute inflammation and there was lymph on its surface as if it was about to perforate.

The loop of jejunum about 14 in. in length containing these sacculi was excised, and a lateral anastomosis was done. The remainder of the sacculi, which were much smaller than these, were uncomplicated and were not interfered with. In the course of a routine examination of the large bowel the pelvic colon was seen to contain many multiple sacculi (typical sacculitis). Many contained hard stercoliths, which lay in the appendices epiploicæ.

Description of the Specimen.—Fourteen inches of the upper jejunum containing three well-developed sacculi, the largest is about the size of a golf ball and the remaining two somewhat smaller. Each sacculus appears on the mesenteric side of the bowel wall and opens directly between the leaves of the mesentery. They are all thin-walled, and microscopical examination of a portion of the wall of the largest one shows it to be composed entirely of peritoneum.

A window has been cut into the bowel opposite each sacculus to show the size of the communication between the intestine and the sacculus, which varies in size from a sixpence to a shilling.

Traumatic Rupture of the Rectum into the Peritoneal Cavity.

By HAMILTON DRUMMOND, F.R.C.S.Ed.

PATIENT, a youth, aged 14, attempted to adjust two old bicycle wheels to an iron rod serving as an axle. He fixed one wheel and then placed it on its side with the axle standing upright, and put the second wheel into position and sat upon it to drive it home. The point slipped and the axle penetrated the seat of his trousers entering the anal canal. Another boy who witnessed the accident drew out the missile, an iron rod 20 in. in length with a diameter of $\frac{1}{2}$ in. He walked home a distance of 150 yards complaining of great abdominal pain, and shortly afterwards he was sick. Twelve hours later he was seen by his doctor, who straightway ordered him to hospital.

On admission to hospital sixteen hours after injury, he looked ill and lay on his right side with his knees drawn up on the abdomen; pulse 120; temperature 99° F.; the mouth and teeth were dry. The lower half of the abdomen showed no movements on respiration, and there were rigidity and tenderness in this situation. The anus showed nothing abnormal with the exception of a small swelling at the anal margin the size of a pea, having the appearance of an external thrombosed pile. *Per rectum* there was a roughened area on the mucous membrane on the anterior rectal wall just above the prostate gland; no actual perforation could be definitely felt. The bowel above this point was acutely tender: there was no hæmorrhage on the finger and the rectum contained soft faecal matter.

A soft catheter was passed and four ounces of normal urine withdrawn from the bladder.

The diagnosis suggested a perforation of the abdominal cavity through the rectum, and laparotomy was done eighteen hours after the injury.

Operation.—Median sub-umbilical incision. The peritoneal cavity contained a quantity of turbid fluid which welled up from the pelvis when the peritoneum was opened. The pelvis contained several ounces of thick chocolate-coloured fluid, which smelt strongly of faeces, and the loops of small intestine in this region were bound together by thick plastic lymph. There was no actual perforation of the peritoneum itself. There was a single perforation of the peritoneum overlying the rectum

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in the middle line, two and a half inches above the recto-vesical pouch. The wound was half an inch in length; the edges of the torn peritoneum showed gross infection and there was extensive retroperitoneal œdema from hæmorrhage.

A stout probe was passed through the rent in the peritoneum over the back of the bladder, and it led directly down to the recto-vesical pouch through a hole into the rectum. There was a tear of the posterior bladder wall, but its cavity had not been opened.

The hole in the peritoneum was sutured by a separate catgut suture and the lower abdomen was washed out with saline. The abdomen was closed by layers of catgut and deep supporting silkworm gut sutures. The sphincter ani was dilated and a large rubber tube was sutured into the lower rectum after flushing out with saline. A soft catheter was sutured into the bladder.

There was no further spread of the recto-peritoneal sepsis or of the peritonitis, and he made a straightforward recovery, leaving hospital eighteen days after operation.

Mr. LOCKHART-MUMMERY congratulated Mr. Drummond on the very excellent results of the operation he had performed, and said he considered it exceedingly lucky for the boy that he came into Mr. Drummond's hands. The injury was a very rare one and was as a rule fatal.

The Treatment of Certain Atonic and Atrophic Conditions of the Cæcum.

By LIONEL E. C. NORBURY, F.R.C.S.

THE subject I have chosen for my paper has interested me during the past few years, and is most important, both from the point of view of diagnosis and of treatment. It is with the latter that I wish specially to deal. The pathological appearances of the cæcum, in the class of case to which I refer, are most instructive, and in well-marked cases the cæcal wall shows a complete absence of muscular tissue with marked fibrosis. Other characteristic changes are also present; to these I will refer later.

In many cases the cæcum is unduly mobile, a long meso-cæcum being present; but this is not always so. Colopecty in these cases of atonic cæcum is not likely to be followed by any benefit, and I would suggest that the failure of the operation of colopecty to relieve symptoms in certain cases of mobile cæcum and ascending colon is due to the fact that the wall of the bowel in these particular cases is toneless, and that one is merely fixing an atonic piece of bowel which is unable to empty its contents, and which constitutes an abdominal cesspool.

The operation of colopecty or cæcoperxy is without doubt of great benefit in certain cases of mobile cæcum and colon, but in my experience resection of the cæcum, and, if necessary, of a part or the whole of the ascending colon, is strongly indicated in those cases in which the cæcum and ascending colon are grossly atonic. I think that it is the experience of most surgeons that the operation of colopecty is not always followed by the good results expected, and I am confident that in a certain percentage of these the cause lies in the presence of extensive degenerative changes in the bowel wall.

For the treatment of this condition I have removed the cæcum with or without a portion of the ascending colon in some twelve cases, and up to the present the mortality has been *nil*.

CLINICAL HISTORY OF THE CASES.

Age.—My youngest patient was aged 16 and the oldest 57. The commonest age at which the patients came for treatment was from 30 to 37. It must, however, be

borne in mind that in most cases abdominal symptoms had already been present for many years.

Sex.—In all my cases the patients, with one exception, were females.

Symptoms.—In cases of atonic cæcum, in which the organ is unduly mobile, the symptoms are similar to those described by Waugh¹ in his treatise on the "Mobile Ascending Colon"; but in those cases in which the cæcum and ascending colon are surrounded by inflammatory adhesions (the so-called Jackson's membrane), fixing the bowel to the posterior abdominal wall, the symptoms conform to what he describes as the "right iliac fossa type."

The symptoms in a typical case of atonic cæcum are the following: indigestion, bilious attacks; loss of appetite; loss of weight; severe constipation with occasional attacks of diarrhœa; headache and all the chain of symptoms of ill-health which are associated with "intestinal toxæmia."

There is a feeling of heaviness and of fullness in the right iliac region and often a dull aching pain in this situation. The patients usually complain of short, sharp attacks of pain at frequent intervals in the cæcal region, and these attacks are without doubt due either to a distended mobile cæcum rotating on its pedicle and producing a temporary volvulus, or else to distension of a toneless cæcum without such twisting of the pedicle. These sharp attacks of pain are often accompanied by a temporary localized swelling in the right iliac fossa which is noticed by the patient and which disappears very often after massage of this region or after the administration of an enema. These repeated attacks of pain in the right iliac region have in many cases been diagnosed as due to chronic appendicitis, and in two of my cases the appendix had been previously removed, once by myself and once by a colleague, but with only temporary benefit in one case and with no benefit at all in the other. Vomiting is also occasionally a symptom. Nervous symptoms are usually pronounced.

Physical Signs.—The patient looks ill, and wasting is often a marked feature. These appearances are doubtless the result of intestinal toxæmia, and the physical signs associated with colonic stasis are present in greater or less degree.

Locally, there is often marked fullness in the cæcal region. The bowel can sometimes be palpated as a soft flabby swelling with attendant "gurgling." Tenderness is sometimes elicited, especially if the patient be examined soon after a sharp attack of pain or at a time when the cæcum is distended. A definite localized swelling may be present—the result (a) of twisting of the cæcum on its pedicle, with consequent distension of the bowel by gas; or, the result (b) of distension of a toneless organ surrounded by adhesions, as already mentioned.

These physical signs are similar to some of those exhibited by a mobile, non-atonic cæcum and colon. It is only by direct inspection and palpation of the bowel at the time of operation that a reasonable differential diagnosis between a tonic and an atonic condition of the cæcum can be arrived at, and in cases of only moderate severity this is not always easily attained.

PATHOLOGICAL APPEARANCES OF THE SPECIMENS REMOVED.

From an examination of the specimens removed it is possible to recognize three types of case:—

- (I) Those showing marked fibrosis in the submucous tissue.
- (II) Cases of fibrotic atrophy involving all the coats of the bowel-wall.
- (III) Cases with marked fibrotic atrophy associated with pigmentary changes.

Type I.—The changes chiefly affect that portion of the submucous or retiform layer situated between the muscularis mucosæ and the true contractile muscle of the cæcal wall (fig. 1). Here fibrosis is marked and extensive. The mucous membrane is catarrhal. The subperitoneal fibrous tissue is increased, and this gives an opaque

¹British Journal of Surgery, 1919-20, vii, p. 343.



FIG. 1. Type I.—Cæcum showing submucous fibrosis with contraction.

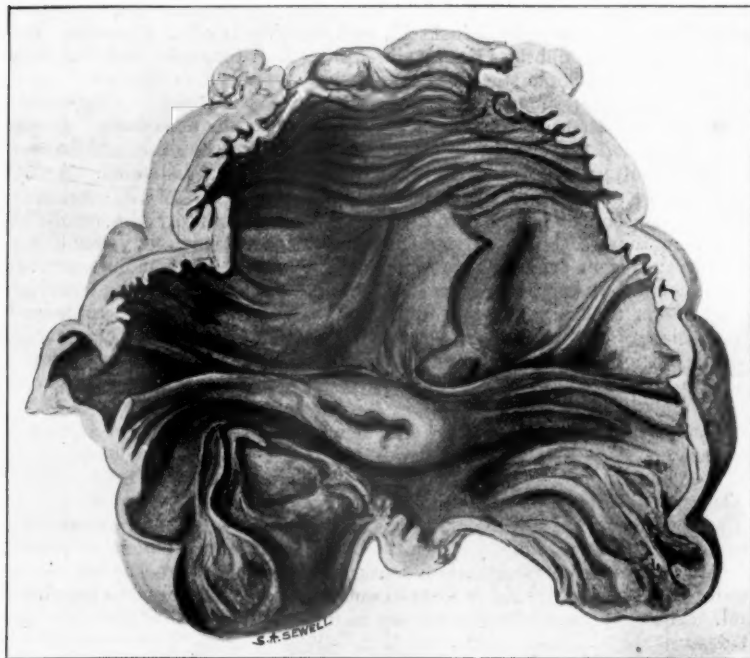


FIG. 2. Type II. Group B.—Cæcum: Fibrosis of all coats with distension.

whiteness to the peritoneal surface of the bowel. The true muscular wall is unaffected. The affected tissues are stiff, as the result of fibrosis, thus tending to make any distension of the gut present a permanency.

Type II.—The condition found in this class of case is a pure muscular atrophy with replacement by fibrous tissue. (The atrophy of the muscular coats was complete in two of my cases.) Edema and secondary changes in the submucous coat are present. The fibrosis affects the blood-vessels in the cæcal wall and also the peritoneal coat. The inflammatory nature of the fibrosis is manifest in the peritoneal adhesions which are often present.



FIG. 3. *Type III.*—Pigmented cæcum: Replacement of all coats by fibrous tissue.

There appear to be two varieties of this type of cæcum:—

(a) One in which fibrosis is associated with contraction of the bowel-wall. Here an atonic but rather contracted cæcum results.

(b) One in which the muscular atrophy is associated with chronic distension of the cæcum (fig. 2). In one case the cæcum was a loose atonic structure, the wall having rather the appearance and consistence of a bag of parchment paper, and being translucent. Similar changes were also present in the terminal portion of the ileum and in the appendix.

Type III.—This is the "pigmentary type" of atonic cæcum. In this class of

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case there is oedema of the mucous membrane and of the submucous layer, with loss of the muscular tissue and some degree of general fibrosis throughout all the coats of the cæcum. The mucous membrane shows catarrhal atrophy, but the most remarkable feature is the alteration in the type of cell present in the subepithelial layer of the mucosa. In this layer large pigment-cells containing a dark brown granular pigment abound. The pigment is not blood-pigment but is closely related to melanin. The pigment-cells resemble large endothelial cells, in appearance not unlike the normal macrophage which is associated with absorptive processes in this region.

It should be noted that it is only the quantity of these cells that is abnormal, since an occasional pigment-cell of this nature may normally be found in this situation as elsewhere in the subepithelial tissue of the large bowel.

Two of my cases are good examples of this type:—

(1) In which the cæcum showed an entire absence of muscular tissue with fibrosis throughout the entire coats (fig. 3). The mucous membrane was chocolate in colour due to a

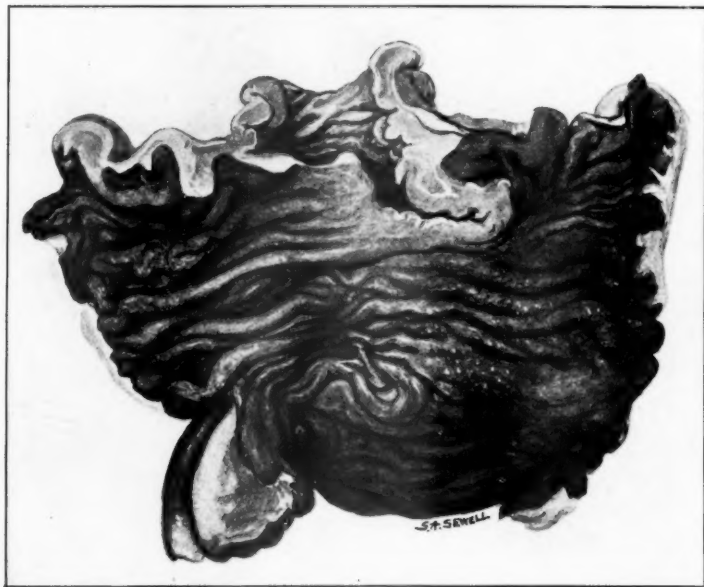


FIG. 4. Type III.—Pigmented cæcum showing fibrosis of all coats. From girl aged 16 years.

very large accumulation of pigment-cells in the subepithelial tissue. Similar but less marked changes were present also in the terminal portion of the ileum.

(2) In a girl aged 16 years. This was the youngest patient in my series. The specimen removed shows marked loss of muscular tissue with fibrosis throughout the cæcal wall. Pigment-cells are numerous though not in such numbers as in the previous specimen. The mucous membrane has a dusty brown appearance as contrasted with the deeper chocolate colour of the previous specimen (fig. 4).

Sir Arthur Keith, in remarks on the examination of colons or portions of the colon removed for intestinal stasis,¹ classifies the colons removed into two groups:—

¹ "Discussion on the After-results of Colectomy," *Proc. Roy. Soc. Med.*, 1921-22, xv. (Sect. Surg. Sub-Sect. Proct.), pp. 60, 61.

(1) Those showing an intense degree of inflammatory infiltration, a condition described by Sir Berkeley Moynihan as the "soggy colon." He states that in these all the coats of the bowel are affected, the incidence of the disease falling particularly on the interglandular retiform tissue of the mucous coat, the normal open cellular network being crowded by proliferation of its constituent cells; the mucous cells lining the absorbent surface of the bowel as well as those within the tubular glands are irregular, but ulceration is rare. Fibrous tissue is increased in the submucous layer. The muscular coats show inflammatory changes, the cellular elements of Auerbach's plexus being often degenerated. The peritoneal coat is thickened and the seat of inflammatory changes. *The changes are not confined to the cæcum, the lower end of the ileum and the cæcum being most markedly affected.*

The state is such as to suggest that recovery "without great structural and functional impairment" is doubtful.

(2) *The Pigmented Group.*—Signs of chronic inflammatory changes are present, but only to a slight degree. The chief changes are again seen in the retiform tissue of the mucous coat. The lymph-vessels lying in this tissue between the tubular glands are seen to be dilated, particularly as they approach the muscularis mucosæ. On the walls of the lymph-vessels or -spaces, especially just above the muscularis mucosæ, are to be seen large cells laden with a dark brown granular material arranged in clumps or clusters. The pigmented material is closely allied to melanin and to adrenalin.

The muscular coats and Auerbach's plexus show certain structural changes similar to those seen by Lieutenant-Colonel McCarrison in animals fed on a diet deficient in vitamins.

From the discovery of the action of adrenalin on the motor functions of the bowel and the effect of a deficient dietary on the adrenals, there may be a relation between "intestinal stasis" and the presence of these pigmented cells in the retiform tissue.

"In this second group the structural changes do not seem to be of a kind which preclude complete recovery."

In two of my own series, however, showing marked pigmentary changes, there was an entire absence of all muscular tissue in one, and an almost entire absence in the other, and it is difficult to conceive how recovery is to be expected in cases showing such extensive degeneration.

ÆTIOLOGY.

It is to my mind a question as to whether the atrophic state of the cæcal wall in the cases under consideration is a primary atrophy or an atrophy and fibrosis secondary to inflammatory changes the result of a spread of infection by way of the mucous membrane. I think that there is a certain amount of evidence to show that there is both a predisposing cause as well as an exciting cause for the atonic and atrophic condition found in these cases. Constipation and intestinal stasis are such common complaints that it is doubtful if these alone will account for the pathological appearances found in, at any rate, certain types of atonic cæcum. One would then expect the condition to be far more common than it apparently is. Most of my patients were young adults, and one was a girl of 16.

Personally, I think that there is some congenital deficiency in the muscular coats of the bowel, at any rate in the second type of case, i.e., the type showing absence of muscular tissue with fibrotic replacement; and also in certain of the third or pigmentary type, where the muscular tissue is entirely absent or plainly deficient. Such cases are comparable to cases of varicose veins in which there is, no doubt, some primary congenital mal-development or weakness of the muscular coats. If this be so, it is manifest that intestinal stasis will then act as an exciting causative factor. When once the cæcum has become atonic, then constipation and

intestinal stasis will obviously be increased. The origin of the pigment-cells in the deeper layers of the mucous membrane is doubtful.

Sir Arthur Keith¹ does not believe that these occur in healthy people. He considers that the cells are endothelial in origin and that the pigment results from intestinal stasis. There are always greatly dilated lymph spaces around the clumped pigment cells, and there seems to be a positive obstruction to the outflow of lymph. Sir A. Keith regards the presence of these cells, engorged with pigment, as symptomatic of a grave disorder in the economy of the colon. Large pigment-cells are not found in the submucous tissue of the small gut; they occur however in the submucous tissue of the large bowel, but only sparsely in the normal colon, decreasing in frequency from the cæcum onwards. In cases of intestinal stasis they are mostly in the cæcum, gradually decreasing along the course of the colon, though they may be found even in the iliac colon. In cases in which the cæcum and colon are involved, the terminal portion of the ileum is also affected.

DIAGNOSIS.

This is determined to some extent by the symptoms complained of and already described, namely: severe and increasing constipation, irregular and often frequent attacks of pain in the right iliac region, chronic discomfort and a sensation of "dragging" in this situation, dyspeptic symptoms and those usually associated with intestinal toxæmia, loss of weight, &c.; also by a local distension of the cæcum, especially during an attack of right iliac pain.

These symptoms, however, occur in cases of mobile cæcum and ascending colon as described by Waugh, and, clinically, I do not think it possible to differentiate between a mobile non-atonic cæcum and a fibrotic atonic cæcum.

X-ray examinations in my experience have not been of much assistance in the diagnosis of these conditions.

When the bowel is handled at the time of operation it is often possible to form a fairly accurate opinion as to whether the bowel belongs to the atonic or non-atonie variety of mobile cæcum.

Types II and III can be recognized with some degree of certainty at the time of operation. In these the muscular wall is almost, if not entirely, absent. The bowel wall is flabby and has very little resilience. The cæcum is usually freely mobile, but may, as already stated, be surrounded by peritoneal adhesions, partly anchoring it to the posterior abdominal wall; in such cases it often contains a quantity of soft pultaceous faecal matter.

Type I often presents difficulty in diagnosis at the time of operation. The bowel-wall is not absolutely toneless, but is not so resilient as normally. In such a case the clinical history should be taken into account before deciding as to whether the operation of colopexy or resection of the cæcum should be performed.

I have had several cases in which I performed colopexy for a loose cæcum and colon, cases in which I felt afterwards the patients would probably have derived more benefit if the cæcum had been removed. I think that these cases belonged to this particular group.

Cases of atonic cæcum have also to be diagnosed from those of chronic appendicitis. In certain of these the symptoms are suggestive of chronic appendicitis, and the differential diagnosis is difficult; but if such cases are seen during an acute attack of right iliac pain, the absence of marked tenderness and rigidity and the absence of an inflammatory mass will negative the diagnosis of acute appendicitis. On the other hand it is obvious that the two conditions may be co-existent. In two of my cases the appendix had been previously removed for supposed appendicitis, and in one the appendix was pronounced to be normal by the pathologist.

¹ *British Journal of Surgery*, 1920-1921, viii, p. 458.

TREATMENT.

I think that most cases of cæcal atony will respond to medical treatment if this is commenced early enough and be carried out systematically, since it is well known that the bowel has extraordinary powers of recovery from injury and disease. It is only when such measures fail after a prolonged trial, and in marked and advanced cases of cæcal atony associated with severe intestinal stasis, that I would advocate surgical treatment.

If one can diagnose, when the abdomen is open, that the cæcum is definitely atonic and atrophic, and if it is manifest that such a lesion may account for the symptoms complained of, then I am sure that the best method of treatment is resection of the cæcum together with—if thought advisable—a portion of the ascending colon.

If the cæcum and colon are unduly mobile, as is often the case, one may be tempted to perform colopexy, but, as I have already stated, in cases in which the cæcal wall is obviously degenerative, I cannot see that fixation of this organ serves any useful purpose, but, on the contrary, its presence is distinctly harmful to the individual. I would therefore advise its removal in such cases.

In those cases in which the large bowel appears for the most part to be in an atonic state, I do not think that complete colectomy, or even an extensive hemicolectomy, is often indicated. It has been shown that in such cases the cæcum is the portion of the large bowel which is most affected, and it is often so degenerate and functionless that the patient stands a much better chance of recovery with ordinary general and medicinal treatment after its removal.

As regards the steps of the operation, I think that the right paramedian incision is the most useful, especially as before the abdomen is opened one cannot be certain as to what lesions may be present or what operation one may be called upon to perform.

Again, most of these cases occur in females, and it is important to examine the pelvic organs at the same time. In one of my cases in which an absolutely degenerate cæcum was present, I had also to remove an ovarian cyst the size of a large orange.

If it is found that the cæcum is not absolutely atonic, it may be considered advisable to perform colopexy, and this can readily be done by enlarging the incision without risk of damaging the nerve-supply to the rectus abdominis. This is difficult, if not impossible, to avoid when a long incision is made to the outer border of the muscle.

If it is decided to resect the cæcum this can be easily done through a right paramedian incision. In my cases the method of procedure has been to place two clamps on the lower end of the ileum, about two or three inches from the ileo-cæcal junction and to similarly clamp the ascending colon about its middle. In cases of freely mobile cæcum without peritoneal adhesions, it is only necessary to place two or three artery forceps on the meso-cæcum, which is often represented by a narrow pedicle. In cases with much pericolicitis, adhesions must be freed first of all. The portions of bowel between the clamps are divided and the cæcum removed. The divided ends of the bowel are closed and a lateral anastomosis of some two and a half inches in length is made between the ileum and ascending colon close to the blind ends. It is important to close the opening accurately in the mesentery which is left as the result of removal of the bowel, and also to cover over any portion of the posterior abdominal wall which has been denuded of peritoneum.

It has been stated that it is of great importance to the economy of the bowel that the ileo-cæcal valve should be preserved, and it has been suggested that if the cæcum be excised a small portion of the cæcal wall should be retained for the anastomosis. It has, however, been shown that the terminal portion of the ileum often participates in the fibrotic atrophy of the cæcum, and in two of my cases the terminal ileum

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showed fibrotic changes similar to those in the cæcum, and so it would serve no useful purpose to retain this diseased portion of the bowel. Again, it has also been stated that removal of the ileo-cæcal valve is the cause of persistent diarrhoea following ileo-colic anastomosis. I have not yet had a case of diarrhoea other than of very temporary duration, following operation of excision of the cæcum, nor after ileo-colic anastomosis performed for any cause. In one case of this series, rather severe diarrhoea commenced a few days after the operation and continued for three or four days, but within ten days of the operation it had ceased.

RESULTS.

(1) *Clinical*.—There is usually slow but steady improvement in the general health of the patient. As already stated, transient diarrhoea may be present for a few days after operation, but in my experience this is unusual. A certain degree of flatulence, indigestion and constipation continues for a time, and I think that these symptoms indicate that the remainder of the large bowel is affected to some degree. These symptoms, however, usually respond in due course to medical treatment, such as the regular use of liquid paraffin, &c., and also to abdominal massage. One must not expect an immediate return to good health in patients after resection of the cæcum; but in my opinion, in picked cases, one has removed the most degenerated and functionless portion of the affected bowel, and less marked degrees of degeneration in more distant portions of the colon are capable of considerable regeneration as the result of subsequent medicinal and general methods of treatment.

(2) *Pathological*.—It has been found by X-ray examination in certain cases that the ileum adjacent to the anastomosis is often empty, the delay being observed in a loop of ileum more distant. The anastomosis often enlarges and atrophic changes are said to occur in the wall of the adjacent small intestine. In complete colectomies, this distension and atrophy are most evident, but the changes seem to vary in degree according to the amount of large bowel resected, and to be least marked in cases of local excision of the cæcum.

In most of my cases the patients have been too recently operated upon for one to dogmatize as to the ultimate results of resection of the cæcum for this condition. I may say, however, that in all there has been slow but steady improvement in the general state of health. The patients have begun to put on weight—constipation has been greatly relieved, and the digestive functions have distinctly improved. Abdominal pain and discomfort have disappeared either entirely or to a marked degree, and several patients have lost the troublesome "neurasthenic symptoms" from which they had previously suffered, within a comparatively short time.

These results are, I think, encouraging, although, as already stated, prolonged post-operative medical and general treatment is essential if one is to expect a complete return to health, and complete relief from the troublesome constipation which is one of the main symptoms of the disease.

[DISCUSSION.]

MR. LOCKHART-MUMMERY said that Mr. Norbury had brought forward a very interesting series of cases and that he thought he had certainly made out a very good case for resection of the cæcum. He quite agreed with Mr. Norbury that on the whole fixation of the cæcum was not a very satisfactory operation except perhaps in the typical cases of floating cæcum where there was no degeneration of the muscular structure of the bowel wall. Too often colopexy, whilst giving what appeared to be very satisfactory results immediately after the operation, relapsed in a short time. He thought that Mr. Norbury's method of resecting the cæcum would prove much more effectual and that the risk was probably very little greater.

He (Mr. Lockhart-Mummery) did not agree with Mr. Norbury that X-rays were useless in the diagnosis of atony of the cæcal wall. He quite agreed that some photographs were of little value, but the method that he (Mr. Lockhart-Mummery) adopted in such cases was

to screen the caecum when the barium was in it and look for contraction of the caecal wall. The administration of a 1 c.c. dose of pituitary extract would, in those cases in which the musculature of the bowel wall was still satisfactory, show well-marked contractions within a few minutes of the injection, and when repeated examinations failed to reveal any contraction of the caecal wall—even when pituitary extract was given—he considered that this was proof of the degeneration of the muscular tissue and of atony. This was a valuable method in such cases of making a distinction between the atonic cases and those in which recovery was possible.

He hoped that Mr. Norbury would at some future time report the after-results of the cases so that there might be an opportunity of estimating the success of the operation.



Section of Surgery.

President—Mr. HERBERT J. PATERSON, C.B.E., F.R.C.S.

DISCUSSION ON THE DIAGNOSIS OF MALIGNANT DISEASE OF THE THYROID GLAND.

Mr. JAMES BERRY.

ANY surgeon who has seen much of malignant disease of the thyroid gland can scarcely fail to have been strongly impressed with the great difficulties that often attend the correct diagnosis of this, fortunately not very common, disease. Most of us have seen patients who have allowed a malignant goitre, erroneously supposed to be an innocent and harmless swelling, to grow to a considerable size before any medical advice at all has been sought. Also, it is not very uncommon for a patient to be treated for months with iodine or thyroid preparations without any suspicion being raised in the mind of the doctor attending, that the growing tumour was anything worse than an ordinary goitre. Especially is this likely to be the case in districts where goitre is common, and there are many such in England. Here both patients and doctors are so familiar with simple goitre, that they are apt to overlook the true nature of the rarer disease, until striking signs and symptoms appear.

The reason for this is partly the rarity of the disease, but partly the fact that most malignant thyroid tumours for a long time produce no symptoms beyond the discomfort of a growing lump. It is only in rare cases that any impairment of general health is noticed before the disease has reached an advanced, and usually an inoperable, stage. Pain, dysphagia, dysphonia and fixity of the tumour are usually all late accompaniments which show themselves only after the malignant growth has already penetrated the capsule of the gland and begun to affect surrounding parts. On the other hand any and all of these signs and symptoms may occur in a thyroid tumour that is not malignant at all, especially in swellings of inflammatory origin.

Difficulties in diagnosis are by no means limited to the very early stages of the disease—those stages alone at which surgical treatment can hold out any hope whatever of a cure. Many of us, I am afraid, must, like myself, occasionally have made serious mistakes in the diagnosis of malignant tumours of the thyroid. We shall all, I think, admit that there are a good many cases in which a *certain* diagnosis before operation is really quite impossible. Inasmuch as malignant disease commonly, but by no means always, attacks a pre-existing innocent goitre, it may have the opportunity of growing to a large size before the glandular capsule is reached and penetrated. Especially is this likely to be the case if the disease begins in the centre of the goitre.

Surgical diagnosis consists, in the main, of the application of common sense to a good foundation of anatomy and pathology. Anatomy teaches us that the thyroid is in contact with the trachea over a wide area. A malignant tumour penetrating

the thyroid capsule on its inner side, quickly becomes incorporated with the trachea, as many of these specimens show. Although still following its movements in deglutition, these malignant goitres cannot be moved upon the trachea, as innocent goitres, unless inflamed, almost invariably can.

Anatomy tells us that the thyroid is in close relation with the recurrent laryngeal nerve and pharynx. Hence the importance of paralysis of the vocal cord and of dysphagia, and these signs may occur while the tumour is quite small, if the disease should begin at the posterior part of the gland.

At the outer side lie the great vessels of the neck, which may involve the operator in grave difficulties, if penetration of the thyroid capsule has taken place in this direction. Anatomy should also teach us that thyroid tissue may be, exceptionally, very widely distributed in the neck, anywhere from the upper part of the thorax to the back of the tongue and, laterally, outwards into the posterior triangles of the neck. When malignant disease affects any of these outlying parts, as it occasionally does, diagnosis becomes correspondingly difficult. In more than one case of malignant or semi-malignant papilliferous tumour, I have seen these outlying portions of the thyroid (not lymphatic glands) affected simultaneously with the main thyroid gland. It is when malignancy begins in the anterior portion of the thyroid gland and the tumour is free to grow forwards, away from the recurrent nerve and pharynx, that the patient is most likely to be free from distressing symptoms. In such cases it is not unlikely that a malignant tumour may attain a large size before surgical advice is sought.

Pathological anatomy teaches us that among the commonest degenerations of innocent goitres are fibrosis and calcification. These give rise to hard, irregular masses within a goitre. One of the difficulties then, in the diagnosis of malignant goitre arises from the fact that one great sign of malignancy, namely, the presence of a hard lump, is apt to be simulated by the perfectly innocent condition of fibrosis or calcification. The clinical history is very often quite misleading. A calcified thyroid tumour may exist for years, especially if it lies partly behind the clavicle or sternum, without the patient having any knowledge of its existence, until one day he, or his doctor, discovers it by chance. I may relate two typical examples out of many.

An elderly patient assured me that he had noticed, for a fortnight only, a lump at the lower part of the neck. Lying partly behind the clavicle was a tumour as large as a walnut, freely movable and of stony hardness, with a rough mammillated surface, obviously an innocent calcified thyroid tumour. It had doubtless been there for at least twenty or thirty years. It caused no symptoms and no treatment was indicated.

In 1921 I saw an intelligent man, aged 50, who, only a few days before had noticed a similar hard swelling in the same region. In this case there was a certain smoothness about the hard lump which seemed unlike malignancy. An X-ray picture showed clearly a completely calcified thyroid cyst. No operation was necessary, and the patient is now, three and a half years later, in good health, without any change having taken place in the tumour.

The presence of a recently noticed, small hard lump in the thyroid of a middle-aged or elderly person should always raise a suspicion of malignancy, even if no other sign or symptom is present. It can often be clearly established that such a tumour is not malignant, as in the above cases. It must be remembered that the great majority of small hard tumours in the thyroid are *not* malignant; they are usually either old fibrous or calcified masses, or they are cysts.

In one of the hospital museums of London is an interesting specimen of an encapsuled multilocular cystic adenoma as large as a duck's egg, which had been removed by operation many years ago, from a patient aged 45. The catalogue tells us that "the hardness of the growth led to a diagnosis of carcinoma," although the patient had had the tumour for thirty years and it had caused no symptoms. Mr.

Warren Low has told me of another similar case in which at yet another London hospital he had, years ago, seen a surgeon remove a calcified cyst of considerable size which had been supposed to be malignant.

Small, tensely filled cysts, especially if deeply seated in the thyroid, may easily give rise to a strong suspicion of malignancy. Such cysts are very common. They can, however, almost always be distinguished without difficulty, by their feel, from malignant tumours. They are often globular and smooth. They lack the peculiar dense, wooden feel of a malignant new growth. It is not uncommon to find another similar, but smaller, lump in some other part of the gland, perhaps on the other side.

A good example of a thyroid cyst being mistaken for a malignant tumour is afforded by the case of a man, aged 68, who was sent to me two years ago by Mr. Hayward Pinch, of the Radium Institute. The patient had just been sent to him by a well known hospital surgeon, of special experience in cancer, as a case of inoperable malignant tumour, suitable only for radium treatment. In the left thyroid region was a large, rounded, somewhat fixed, soft mass some 4 or 5 in. in diameter. *The left vocal cord was paralysed.* The larynx and trachea were much displaced. There was pain up the side of the neck, and stridor. But there was no dysphagia. The history was that until two months previously the patient had not been aware of any lump in his neck. Five weeks later the tumour had grown nearly to its present size, and he consulted his doctor: since that time it had grown still larger and for two weeks he had had hoarseness and had suffered pain. The diagnosis made by us was recent hæmorrhage into a soft adenoma or cyst, and two or three days later, with the full consent of the surgeon who had considered the tumour to be malignant, I removed a large, perfectly innocent, cystic tumour of the thyroid, containing a quantity of recently extravasated blood. It had originated in the back of the left thyroid lobe in an old goitre, of the existence of which the patient had never been aware. The vocal cord continued paralysed but when I last saw the patient his voice had become almost normal.

While on the subject of hæmorrhage into thyroid tumours, which is extremely common, I should like to say emphatically that this admits of no presumption at all that the tumour into which it occurs is malignant. I have removed, mainly for pressure symptoms, scores and scores of cystic and solid adenomata into which hæmorrhage had occurred—often very extensive hæmorrhage. But I have never, to my knowledge, seen any considerable spontaneous hæmorrhage within a malignant thyroid tumour. Clinically, hæmorrhage into or around a thyroid tumour may easily produce a superficial resemblance to malignancy, because it is likely to cause a thyroid tumour to become, rather suddenly, large, hard and often painful, and may lead to severe pressure symptoms.

DYSPHAGIA.

It is somewhat remarkable how seldom patients with *innocent* goitres complain of any serious difficulty in swallowing. Even with goitres that pass round behind the larynx and displace the pharynx and œsophagus, it rarely happens that dysphagia is a prominent feature. It is quite common to see such a goitre produce a marked inward bulging of the pharyngeal wall, yet, in such a case, it is but rarely that the patient complains of dysphagia. With malignant disease it is otherwise. When the disease begins at the back of the thyroid gland, dysphagia is often an early and important sign. Instances of this are so common that I need not give examples. But this symptom may also lead to error, as in the following case:—

Some years ago I was consulted by a lady, aged 63, on account of a rather hard, rounded, unilateral, rather deep-seated goitre, about as large as a small Tangerine orange. It had existed some thirty years, but recently, she told me, it had *increased considerably in size*. For three weeks she had lost her voice and had some dysphagia and dyspnoea. The right vocal cord was paralysed. A laryngological colleague confirmed the diagnosis of paralysis of the cord, and added that he could find "no evidence of malignancy." I noted that the tumour was "probably merely a cystic adenoma with pressure on the recurrent nerve

but that malignancy could not be wholly excluded." As it was causing a good deal of pain and discomfort, I removed the tumour. It proved to be cystic, but unusually adherent to both pharynx and recurrent nerve by what appeared to be recent inflammation. The microscopic report was "old, cystic adenoma with some calcification and recent inflammation." The wound healed without any trouble, but the dysphagia never completely disappeared. When I saw the patient again six months later she had, what I ought to have suspected at first, a carcinoma of the œsophagus, of which she eventually died.

One of the main objects which I had in mind when I suggested the holding of this discussion was once again to draw attention to a not uncommon class of innocent thyroid swelling which is frequently mistaken for malignant disease—namely, the hard, painless, generally bilateral, inflammatory swelling of the thyroid, which is most commonly seen in middle-aged or elderly women and which often grows with considerable rapidity up to a certain point. These tumours are often taken for carcinoma, and are sometimes treated erroneously by removal of the whole thyroid gland, an operation which, in my opinion, should *never* be performed for any thyroid condition whatever—even for true carcinoma. I have never myself performed a total thyroidectomy nor have I ever seen a case in which I desired to do so, nor any case in which I regretted not having done so. It is quite unnecessary and often harmful.

The type of case to which I refer is that of the woman (or occasionally the man) who, when first seen by the surgeon, has a densely hard swelling, generally of moderate size, involving uniformly the whole of the thyroid gland and moving freely on deglutition, although often more or less fixed to the trachea.

It is generally painless or nearly so, and is usually unaccompanied by any marked symptoms, beyond a little discomfort and perhaps a very slight amount of difficulty in swallowing or breathing. The vocal cords are not affected. By the time the case is first seen the swelling is usually already bilateral, but if seen very early it may be unilateral, spreading very rapidly to the opposite lobe. After removal the appearance on section is of a dull white with faint lobulation shown by fine lines of grey connective tissue permeating the gland.

To my mind the main points in the diagnosis are the smoothness of the surface of the gland and the aspect of the patient. There is usually in these cases at least a trace of that waxy pallor which denotes commencing thyroidal atrophy. This is nearly always absent from the patient with true malignancy not only at an early stage, but also in cases of quite advanced and extensive disease long after extensive infiltration of extrathyroidal tissues has occurred. In the elderly patients, in whom this disease usually occurs, dyspnoea rarely becomes severe enough to demand any operation for its relief, because the trachea is sufficiently resistant to withstand the slight pressure of the growth. In younger patients, however, say in people under forty, in whom the trachea is more compressible, dyspnoea may occasionally be sufficient to demand some operation, generally removal of a part of one lobe.

[Mr. BERRY then proceeded to discuss the subject of the so-called malignant adenomata of the thyroid and of some of the rarer forms of very slow-growing malignant disease. He thought that the doctrine that all adenomata of the thyroid should be removed, whether they were giving trouble or not, merely because they might *possibly* become malignant, was being carried a good deal too far in certain quarters, notably in America. The likelihood of any given adenoma, even a solid adenoma, becoming malignant was extremely small. He had been through the whole of his operative thyroid statistics up to date, and had found only 373 cases in which he had removed solid adenomata of the thyroid for one reason or another, usually for pressure symptoms. Among these tumours there were only some three or four which were really malignant. The total number of malignant thyroid tumours of all kinds that he had treated by removal was only about thirty. Many other cases illustrating the

above and other points were then related, a large number of specimens, photographs and drawings being demonstrated by means of the epidiascope.]

Dr. SCOTT WILLIAMSON

exhibited and demonstrated a large collection of museum specimens and microscopic sections illustrating various forms of malignant disease of the thyroid gland.

Mr. WILFRED TROTTER.

I should like to call attention as emphatically as possible, to one small difficulty among the many that the subject presents. This is the distinction between carcinoma of the cervical œsophagus, and malignant disease of the thyroid. It is, I think, far from being generally understood that there is here any serious diagnostic problem at all. In most authoritative writings it will be found that carcinoma of the œsophagus is formally mentioned as a possible source of error in the diagnosis of malignant disease of the thyroid, but it will also be found that the difficulty is dismissed with the statement that the former condition is to be distinguished by its causing early dysphagia. Such a statement is wholly incorrect for the majority of cases.

Carcinoma of the cervical œsophagus is remarkable for the fact that it usually has a very characteristic evolution, in the course of which it approaches so nearly to the appearances of malignant disease of the thyroid as to be capable of misleading the most cautious. I venture to be very emphatic in this matter, because to confuse the two conditions may lead to one of the worst possible of surgical failures. To operate on a case of carcinoma of the œsophagus under the delusion that one is dealing with a thyroid tumour, may result in the formation of a malignant œsophageal fistula.

With regard to the usual course of carcinoma in the cervical œsophagus, it is commonly a disease of middle-aged men. In a great majority of cases, the first symptom is a weakening of the voice, leading up to a paralysis of one vocal cord. Sometimes the paralysis of the cord is of abrupt onset. In any case it usually precedes any œsophageal symptom whatever, by many weeks or months.

The second symptom, which is not usually long in following the paralysis of the cord, is an enlargement of the thyroid. This begins as a firm but ill-defined induration in the hinder part of one of the lateral lobes. It spreads forwards with an ill-defined margin, until the greater part, or the whole of the gland is affected. The enlargement of the thyroid is never very great, is always firm and rather indefinite, and is characteristic in giving the impression that it has originated far back in the gland. It is only after the paralysis of the vocal cord and the thyroid enlargement have been evident for some considerable time, that the definitely œsophageal symptoms appear. Even then the most prominent of such symptoms may be less those of a definite obstruction, than of a fistula from the œsophagus into the trachea. In such a case, the profuse blood-stained expectoration may be wrongly attributed to the ulceration into the trachea of a thyroid growth.

It will be seen that the resemblance of the clinical picture of carcinoma of the cervical œsophagus to that of malignant disease of the thyroid, is very close and deceptive. In my experience, the two conditions are about equally common. To avoid mistakes, it is necessary to be fully aware of their resemblance, to pay special attention to the evolution of the symptoms and the nature of the thyroid swelling, and particularly to bear in mind that *in carcinoma of the cervical œsophagus, obstruction is usually a very late symptom*. If there is the least doubt about the true nature of a given case, no operation should be undertaken unless radiographic and œsophagoscopic examinations have been made.

Mr. A. J. WALTON.

The whole subject of carcinoma of the thyroid presents so many curious anomalies that no certain statements can be made which are true for all varieties. It is often stated that the disease is so malignant that once the diagnosis can be definitely made the prognosis is hopeless; but in more recent work there has been a tendency, in the statistics at least, to demonstrate that the outlook is quite good. These diverse statements are clearly due to the inclusion of several very different conditions, and therefore, before it is in any way possible to discuss the diagnosis, it will be necessary briefly to consider the frequency of the disease and the various pathological and clinical types.

Malignant goitre is usually found in patients of middle age, cases most frequently occurring between the ages of 40 and 50. The youngest patients in my series were two males aged 21 and 37 and one female aged 34. The oldest was a female aged 79. Most investigators find that the condition is twice as common in females as in males, this being the ratio in Wilson's series [13]. In my own group of thirteen cases there were nine females and four males.

In the majority of my cases the diagnosis has been proved by microscopic examination: two have not been operated upon owing to the advanced condition, but both of these have pursued the characteristic course of advanced carcinoma, and the diagnosis, therefore, can be accepted. In the same period of time there have been 402 cases of innocent tumour of the thyroid which have been subjected to operation. Thus in this operative series the frequency has been about 3·2 per cent. There are, however, not included in these figures a large number of cases of colloid goitre and small localized adenomata which have been seen in the hospital out-patient department, and of which—unless a special series is observed for the results of treatment—I keep no private notes: hence the frequency in the total number of cases is probably somewhere about half of this figure, i.e., 1·6 per cent. It is in this question of frequency that such varying figures are found in the literature. Balfour [1] found 103 cases of carcinoma in 6,359 of goitre, a percentage of 1·6, whereas Speese and Brown [11] found the frequency as high as 4·6 per cent. They believe that this difference may be accounted for owing to the fact that their cases came from a non-goitrous region, whereas Balfour's were obtained from an endemic goitrous area. Wilson [13] in his investigations found that the incidence of malignant disease was as high as 1 in 15.

In my own experience the morphological characters, whether micro- or macroscopic, have been of little or no help in determining the clinical course of the disease or in estimating the measure of its malignancy. Pemberton's [8] classification into sarcoma, carcinoma, malignant adenoma and malignant papilloma, although of pathological interest, is of little or no help, for these different varieties do not give rise to constant clinical types. Of two of my cases showing very similar enlargements of the thyroid, and both described as "mixed-celled carcinoma," one patient died of extensive recurrence after five months, whilst the other, although developing a local recurrence which was treated with X-rays, is alive and well over three years after the operation. De Quervain [5] has also laid stress upon these difficulties, and he states that the term "malignant goitre" must still be used, because it is impossible to differentiate the varying histological types clinically; indeed it is not possible to distinguish cancer from sarcoma. With the latter part of his statement all will agree, and indeed this subject presents enormous difficulties even to the special pathologist; for, as Wilson has pointed out, the sarcomata are peculiar in that besides the connective tissue elements, of which any form of sarcoma may be the dominant one, there are almost invariably present large or small groups of parenchymatous cells which are also proliferating. He states that it is therefore difficult to say whether the original tumour was formed of proliferating carcinoma

cells with a later growth of the connective-tissue elements, or whether the connective-tissue change was primary, and there was a secondary stimulation of the parenchymatous cells. At the London Hospital all these mixed cases are reported as "carcinoma sarcomatoides," of which there were four in my own series. To my mind, the almost constant appearance of the carcinoma cells in the cases of sarcoma is a strong argument in favour of the view that these tumours arise from groups of embryonic cells, for in their growth such cells might be expected to differentiate along the connective tissue or epithelial cell-path, or even along both.

The difficulties are increased by the fact that many lesions of the thyroid which ultimately pursue a clinical course that is clearly innocent show a microscopic picture which would be passed as malignant. The close simulation that microscopic sections of some cases of exophthalmic goitre show to carcinoma is well known, and in one of my cases (No. 1. A) such a diagnosis was made because the cells had spread widely into the lymphatics. The appearance of the thyroid, clinically and at operation, also suggested this diagnosis, but the subsequent progress has given no evidence of the presence of any carcinomatous change, and I doubt very much, therefore, whether I ought to have included this case in the list of carcinomata. The difficulties that have arisen in the cases of the proliferating goitre of Langhans, or the toxic adenoma of Plummer, to which attention will later have to be given, are also well-known. For these reasons, then, any pathological classification must in the present state of our knowledge be regarded as unsatisfactory, and there will have to be recourse to a clinical grouping.

In my own cases three quite distinct clinical varieties can be recognized:—

- (1) Those occurring secondarily to adenoma or a colloid enlargement.
- (2) Those which are apparently carcinomatous from the onset.
- (3) Those showing a slowly growing, primary lesion with large secondary deposits.

In such cases the primary lesion is frequently overlooked.

(1) *Those Occurring Secondarily to an Adenoma or Colloid Enlargement.*—In my own series there were five such cases, four being females and one a male. The presence of such a group has long been recognized, but very different figures have been given as to the frequency of any previous lesion. Speese and Brown [11] in their careful investigation of twenty-five cases of carcinoma and three of sarcoma found that there had been a previous adenoma or colloid goitre in 78.5 per cent. of the cases, and the average time at which a simple lesion had been present before the onset of malignancy was 12 $\frac{6}{10}$ years. Wilson [13], Plummer [9], and Balfour [1] believe that the association of malignant disease with a previous benign lesion is almost invariable, and Berry [2] has shown that a careful examination will often reveal the presence of some evidence of previous disease. Chambers [4] states, however, that taking into consideration the slow growth of some cases of carcinoma it is possible that in many instances the condition is rather one of continuous slow development of a malignant goitre than of malignant disease superimposed on a benign growth.

My own cases in this group all show, as will be seen by the table, a prolonged period, ranging from two and a half to thirty-five years, in which a tumour having only benign characters had been recognized, and it is not possible to believe that over such long periods the tumour was already malignant. It is in this group that the first evidence of malignancy is often shown by the presence of some hyperthyroidism due to a cellular proliferation within the capsule of the gland, and I believe that too much stress cannot be laid upon the work of Plummer and Wilson in this respect. The latter [14] found that of 97 cases in which the patients had died of the disease or had had recurrence after operation in the Mayo Clinic there were fifty whose clinical history before the first operation contained no suggestion of malignancy, and twenty-three of these thyroids were even passed by a pathologist without suspicion of malignancy. He believes that the first evidence of proliferation

is hypermetabolism with cardiac disturbances—the toxic adenoma of Plummer. If the proliferation continues it penetrates the capsule, invades and metastasizes and is then frankly malignant. Histologically it may be impossible to distinguish the foetal adenoma from carcinoma. Hence he states that any sizable adenoma composed of embryonal tissue, if in a patient of cancer age, is potentially malignant. With this my own experience agrees and I would therefore regard the onset of hyperthyroidism in any adenoma as a strong piece of evidence of the onset of carcinoma, and would advocate early operation in any such case. Later, when the cells break through the capsule more definite evidence of carcinoma arises, but it is interesting to note that for a time such cases are relatively slow in their progress, and hence the prognosis may still be quite good.

Usually the first evidence of such change is a more rapid growth of the localized nodule, but the mere presence of such a change must not in itself be taken as definite evidence of carcinoma. There are a considerable number of other conditions which will give rise to such a rapid increase. Perhaps the most frequent of these is the presence of an intracystic hæmorrhage, and the fact that this may cause a rapid enlargement and a sudden increase in the dyspnoea is well-known. The enlargement due to a hæmorrhage is often, however, much more sudden than that due to the onset of carcinoma, although the rate of increase will naturally depend upon the amount of bleeding. More suggestive of carcinoma is the rapid increase in size which occurs with an inflammatory change. This will give rise to considerable difficulty in the diagnosis, for not only is the mass increased in size but its outline becomes more ill-defined and it may become firmly adherent to the trachea and sometimes even to the surrounding structures. Its outline, however, is, as a rule, less nodular than in the case of a carcinoma. Another difficulty may arise owing to the fact that some of the tumours which have undergone a definite carcinomatous degeneration may not always show a rapid onset in the growth. Wilson [13] found that such an increased rate of growth was only present in one-third of the cases, whereas another one-third had noticed symptoms of continued growth for ten years or more, and even of those that were inoperable at the time of their examination one-quarter had noticed symptoms which might have been interpreted as indications of the presence of malignancy for ten years or more.

An important change which is usually seen at the same time as the increase in size is an irregularity of the outline and a firmer consistency of the mass. Similar changes may be seen in an adenoma which has been cystic and calcareous, but unless there be coincident inflammation there will not be the same measure of fixation. With rapid increase in size due to inflammation or hæmorrhage the growth may be equally hard but will rarely be so nodular. It must also be remembered that an adenoma in a patient who has become pregnant not infrequently becomes larger and considerably harder, and hence may be mistaken for a carcinomatous change. As a general rule no operation should be performed for goitre on a pregnant patient if it can possibly be avoided. In all the above conditions there is rarely the same irregularity the presence of which is distinctly suggestive of the onset of carcinoma. Care must, however, be taken not to mistake one large adenoma with several small outlying adenomatous patches for the irregularity of one neoplastic mass.

An early and suggestive symptom is the presence of pain. Radiating and neuralgic pain passing from the lobe of the thyroid up the corresponding side of the neck to the back of the head is in most cases due to the presence of carcinoma. It may, however, occur when a fairly acute, inflammatory change has taken place in the thyroid. If it be due to a sudden increase of tension, such as occurs with hæmorrhage, the pain is much more likely to be localized to the site of the swelling. With the onset of pain the mass will become fixed. It is important to remember that a simple adenoma is freely movable both upon the trachea and the surrounding tissues. If it has become carcinomatous it usually first becomes fixed to the trachea and may be

definitely malignant although it is still freely movable on the surrounding tissues. Stress is not sufficiently often laid upon this fact, and fixation to the trachea is often regarded as a character of all thyroid tumours. Care must therefore be taken in the examination to fix the trachea and to estimate the mobility of the mass upon it. With the continued spread of the growth beyond its capsule the next structures to be involved are as a rule the pretracheal muscles; as these become infiltrated there will clinically be a further amount of fixation and the nodular appearance on the surface will become much more evident. A distinct increase of pressure upon surrounding structures giving rise to dyspnoea, dysphagia, hoarseness and coughing, or clear evidence of interference with the recurrent laryngeal nerves, are all suggestive, but as with the other symptoms any one of these alone must not be taken as clearly indicative of carcinoma. It used frequently to be stated that a paralysis of one side of the larynx was diagnostic of carcinoma. It is now known, however, that a simple adenoma may project posteriorly and interfere with the recurrent laryngeal nerve sufficiently to cause a similar weakness of that side of the larynx. It is rather on the presence of several of these physical signs than of one alone that a diagnosis of carcinoma will be made. In the later stage infiltration of the skin, with redness, marked dilatation of the veins and the presence of the blood-stained sputum, will make the diagnosis evident. Enlargement of the lymphatic glands is also suggestive but is not certain evidence of carcinoma, for an inflammatory lesion may also cause glandular enlargement. The nature of the enlargement is of more importance. In the former case the glands are harder and more sharply defined unless the carcinoma has broken through them and involved the surrounding tissues. These changes are usually quite distinct from the softer and more tender enlargement which is inflammatory in nature. The presence of such glandular enlargement may be found relatively early and is thus not in itself a necessary counter-indication to operation.

The difficulties of making a diagnosis in the early stages are perhaps less in this than in the other groups, but are very considerable. The presence of a certain amount of hyperthyroidism I would regard as of great import, for even if it is not due to the actual onset of carcinoma it will give rise in time to severe toxic changes, and thus is an indication for operation even in elderly and feeble patients. It was present in three of the five of my cases in this group. Of the other symptoms rapid growth, increased hardness, irregularity and tracheal fixation may all occur early, and although each one alone may occur with other conditions the presence of two or more is very suggestive. If operation be performed in these earlier stages the prognosis is not so absolutely hopeless as it is often thought. Of the patients in my five cases the first, who perhaps should not be included, has developed myxœdema but is alive and otherwise well ten years after operation; the second is quite well seven years after operation; one had a recurrence which has been kept under control and has indeed decreased in size with X-rays, the patient being alive and feeling in perfect health three years after operation; the fourth is free of any evidence of recurrence two years after operation and the fifth died with evidence of recurrence a few months after operation.

(2) *Those which are apparently Carcinomatous from the Onset.*—This group in my series was slightly more common than the last, there being seven such cases, five being females and two males. In nearly every case the first symptom noticed was a swelling in the neck which grew rapidly and within a short time of the onset was associated with pain or the onset of pressure symptoms. Porter [10] states that as a rule this variety occurs in people beyond middle life, usually as a unilateral and less frequently as a bilateral goitre, and grows steadily or rapidly. It will be seen, however, that in my series one of the patients was only 21 at the time of onset, although the majority were over the age of 40. The longest history obtained was that of one year, and in this case (No. B. 2) the growth was relatively benign in that even at the end of this period the tumour on clinical examination still

appeared to be limited to the thyroid gland. It was, however, of special interest from the fact that at the operation it was not only adherent to the trachea but the whole of the right lobe was found to be involved and the growth had spread through to the pretracheal muscles, so that these structures were infiltrated with a material resembling sago-pudding in its consistency. Nevertheless a wide removal of the right lobe and of the overlying pretracheal muscles was followed by a rapid cure, and the patient has since remained free from all evidence of recurrence, a period of nine years. In three of the cases the tumour was limited to one lobe and showed the general appearance of an adenoma, but in all of these the mass was considerably harder than would have been expected had the lesion only been an adenoma. In one it was nodular in outline and in the third it was adherent to the trachea, that is to say, in two of the three cases there was fairly definite evidence of carcinoma on clinical examination, so that the physical signs taken with the short history and rapid growth made the diagnosis probable. In the remaining cases the growth was more diffuse, a whole lobe or more of the gland being involved. It is in this group that a considerable amount of difficulty will be found in making a correct diagnosis, and this will lie chiefly in the direction of mistaking non-malignant conditions for carcinoma. This variety of growth usually presents a short history; there is a diffuse enlargement of one lobe or more of the gland, the enlarged mass is hard, is often irregular and is fixed to the trachea. There is often a considerable amount of pain, probably owing to the rapid growth. The mass is not only fixed to the trachea but there may be a distinct amount of dyspnoea and often some difficulty in swallowing. Although these changes are very suggestive of carcinoma—I have had three such in which a positive diagnosis of growth was made—an operation was not advised either because of the patient's age or because the size and fixation to the trachea gave rise to the belief that the condition was too advanced for an operation to hold out a reasonable hope of cure. The after-progress of these cases has, however, been so slow, or the condition has so definitely regressed, that it is certain that there is no carcinomatous element present. Unfortunately in such cases the correct diagnosis remains a matter of conjecture, for no portion has been removed for section and it is only possible to believe that the enlargement was due to some inflammatory change. It is always advised that in these cases a Wassermann test should be carried out, and there is no question that in all such doubtful cases it should be performed. I have, however, only once seen a definite case of syphilis of the thyroid. Here there was a localized swelling in every way resembling a simple adenoma, but owing to the strong syphilitic history a Wassermann test was performed and found to be positive, the lesion yielding to appropriate treatment. In not one of the doubtful cases was the test positive. In these inflammatory conditions the swelling is, as a rule, less sharply defined and not quite so hard and irregular.

In this group, as in the last, there is the onset of pain at an early date. At first limited to the site of the lesion, it soon begins to radiate more widely, and frequently passes up the side of the neck to the ear and the side of the head, the patient frequently complaining of earache. Another point which I believe is frequently of value in making a positive diagnosis is that the carotid vessels may be pushed outwards, whereas it is more common in the innocent varieties for these structures to lie deep to the thyroid, which tends to expand forward as it grows. In the later stages the diagnosis becomes the more certain as the prognosis is more hopeless. Dyspnoea increases, and there may be the expectoration of blood-stained mucus. Dysphagia is present and there is serious interference with the recurrent laryngeal nerve. The mass, and with it the trachea, becomes more firmly fixed as the growth invades the surrounding tissues. The lymphatic glands above, and not infrequently those in the posterior triangle, become enlarged and hard, and later

they in turn become fixed. The superficial veins are dilated and tortuous, and the skin may become adherent and reddened.

The important difficulties in diagnosis, therefore, lie in the early stages, when there is still some hope of bringing about a cure from surgical interference. In the localized group it may be said, as in the first variety, that there is no one sign which is certain evidence of carcinoma. The short history, the steady growth, the hardness, and especially a nodular hardness which distinguishes this condition from a calcified adenoma, and a certain amount of fixation to the trachea, will make a positive diagnosis almost certain. In my own experience any definite degree of hyperthyroidism has been absent in this group.

In the diffuse variety the difficulties are much greater, but fortunately the tendency is to mistake innocent lesions for carcinoma rather than carcinoma for inflammatory changes; here again, however, the combination of the short history, the steady progress, the hard mass and the nodular surface are the important factors. In neither case do I believe that the local removal of a small portion of the growth will be of any value in arriving at a diagnosis. So often both neoplastic and inflammatory changes are combined that the absence of carcinomatous changes in so small a portion would still leave the diagnosis in doubt. It must be remembered here that the younger and more healthy the patient the more readily will operation be advised in all doubtful cases. It is in old, feeble patients, on whom any operation of this sort might be a grave danger, that such care has to be exercised in excluding inflammatory lesions from the diagnosis.

In this group, as in the other, the prognosis will depend upon the time at which operation is undertaken, but even here there is a great variation in the rate of growth. In one of my cases, showing a small mass of only six weeks' duration, recurrence took place four months after the operation, and death occurred four months after the recurrence, while another patient with a year's history was well nine years after operation. Of the seven cases three were inoperable when first seen, two patients died of recurrences within a few months, one was lost sight of after eight months when she was still apparently well, and one remained free from all trouble to date, nine years after the operation.

(3) *Those Showing a Slowly-growing Primary Lesion with Large Secondary Deposits.*—There is a curious variety of carcinoma of the thyroid in which the primary lesion is so slowly-growing or is so small that it is frequently overlooked entirely, and this even upon careful examination. Scattered throughout the literature are accounts of cases in which metastatic deposits have occurred, and these most frequently in the long bones. The patient has been subjected to an operation, very often an amputation, in the belief that the condition was one of primary sarcoma of bone, and after operation a microscopic examination revealed the fact that the tissue was apparently carcinoma of the thyroid gland. The most remarkable, perhaps, of these cases is that reported by Sir Frederic Eve [7], which occurred at the London Hospital.

This was the case of a woman who had a large pulsatile tumour in the upper end of the femur for which an amputation was performed at the hip-joint. At the time of the operation no lesion could be found in the thyroid gland, but some few months after the amputation the gland became enlarged. This enlargement had steadily but very slowly progressed, so that at the time of the report, three years after the operation, the thyroid was considerably enlarged, but the patient was in other respects in perfect health.

There are several points of interest in this group of cases. First, there is the fact that the secondary deposits are more common in the parietes, whereas usually the other varieties, when advanced, give rise to secondary deposits in the viscera and especially in the lungs. The other point of importance is that the primary growth is relatively benign and slowly growing. De Quervain [5] has laid stress upon this,

Case no.	Date	Name	Sex	Age	Swelling	Progressive symptoms	Hyperthyroidism	Condition of thyroid	Pressure symptoms	Fixation
A. 1	Oct. 26, 1914	L. S.	F.	34	Four years	Pressure and inconvenience six months	Nil	Diffuse swelling	Slight	Nil
A. 2	Sept. 14, 1914	E. T.	M.	42	Six years	Increase in size and pressure one year	Marked	Diffuse enlargement	Nil	Nil
A. 3	Oct. 25, 1921	G. R.	F.	46	Five years	Rapid increase six months, dysphagia, dyspnoea	Nil	Large swelling, right lobe and isthmus	Dyspnoea, dysphagia, alteration in voice	Complete to trachea
A. 4	Oct. 4, 1922	H. S.	F.	69	Two and half years	Severe pain ten months. Dyspnoea and dysphagia	Moderate	Firm, hard mass, right side irregular	Dyspnoea, dysphagia	To deep structures
A. 5	June 21, 1917	M. A.	F.	67	Thirty-five years. Right side	Left swelling three months. Pain and dyspnoea	Moderate	Large irregular masses, left, very big	Much dyspnoea	Nil
B. 1	Nov. 8, 1914	A. S.	M.	21	Six months	Change in voice two months. Dyspnoea	Nil	Enlargement left lobe, nodular	Dyspnoea	Nil
B. 2	April 20, 1915	L. T.	F.	48	One year	Increase in size and pain one month	Nil	Localized swelling right lobe	Change of voice	Present
B. 3	Sept. 5, 1916	F. F.	F.	44	Ten months	Rapid growth since onset. Difficulty in swallowing. Loss of weight	Nil	Large, soft mass, right, skin red and injected	Great pressure on oesophagus	Absolute
B. 4	Dec. 31, 1917	W. B.	M.	64	Six weeks	Swelling in neck. Steady progress since onset	Nil	Rounded swelling in isthmus	Nil	To trachea
B. 5	May 1, 1919	H. S.	F.	55	Nine to ten months	Swelling in neck. Steady progress. Pain five months	Nil	Hard mass right lobe. Displacement of carotid	Dyspnoea, dysphagia, aphonia	To trachea
B. 6	June 2, 1921	M. S.	F.	79	Two weeks	Swelling in neck. Steady progress. Pain	Nil	Hard mass, right lobe	Some dyspnoea	Firmly to trachea
B. 7	June 5, 1923	P. R.	F.	61	Three weeks	Swelling in neck, right side. Steady progress	Nil	Enlarged, hard nodular, right side	Some dyspnoea. Voice hoarse	Absolutely to trachea
C. 1	Aug. 8, 1922	C. M.	M.	38	Four years swelling in front of left ear. Removed elsewhere	Two years recurrence of lump in front of ear. Eighteen months swelling below right clavicle	Nil	Nothing abnormal found	Nil	Nil

	Secondaries	Condition at first operation	First operation	Pathological report	After progress	After treatment	Later progress
	Nil	Oct. 28, 1914. Colloid shape very hard	Double V-resection	Carcinoma	Thyroid very large and hard for two weeks. Hypo-thyroidism after five weeks	Thyroid extract after six weeks	Myxœdema ceased. Swelling rapidly disappeared. At end of year thyroid not felt. Still myxœdema controlled by extract. Diffuse alopecia after two years: improved. Still myxœdema after seven years: controlled. Married, with one child. After seven years quite well
	Nil	Sept. 16, 1914. Diffuse enlargement like Graves' disease	Removal of right lobe and isthmus	Acinar, papillary cystic, cylindrical, cubical giant-celled carcinoma	Steady improvement in one year	—	Gradual improvement. Three years later quite well. Still small swelling upper border of thyroid
	Right supra-clavicular	Oct. 26, 1921. Right lobe and isthmus and glands enlarged	Removal of right lobe and isthmus, right supraclavicular glands	Solid mixed-celled, carcinoma. Secondaries in gland	Good immediate recovery. Three months later one large gland high up, right side	Given X-rays	—
	General, right	Oct. 6, 1922. Large, hard, nodular mass right lobe, not infiltrating	Right hemithyroidectomy	Solid tubular carcinoma or thyroid	Good immediate recovery. Quite free two years after operation	—	—
	Nil	June 21, 1917. Trachea displaced to right. Left lobe very large, adherent to sheath and muscles	Left hemithyroidectomy	Carcinoma sarcomatoides. Multi-nucleated cells and many mitoses	Good immediate recovery	—	Died of recurrence four months after operation
	Nil	Nov. 13, 1914. Ill-defined mass Right lobe. ? adenoma	V-resection unilateral	Solid acinar trabecular and papillary cystic carcinoma	Good immediate progress	—	Well eight months, then failed to report
	Nil	April 21, 1915. Diffuse enlargement. Adherent to trachea and muscles	Right hemithyroidectomy	Medullary carcinoma chronically inflamed in thyroid	Uninterrupted recovery	No further treatment	Perfectly well after eighteen months. No recurrence since
	Nil	Sept. 6, 1916	Gastrostomy	Nil	Died two days later	—	—
	Nil	January 3, 1918. Mass in isthmus, adherent to trachea	Isthmus incised, dissected off trachea	Mixed-celled scirrhous carcinoma sarcomatoides	Local recurrence Apr. 1918. Died Aug., 1918	—	—
	Nil	June 8, 1919. Right lobe enlarged and fixed	Wide dissection. Removal down to mucosa and pharynx	Solid diffuse mixed-celled carcinoma sarcomatoides	Good, immediate recovery. Pain and dysphagia after three months. Local induration. Later dyspnea	—	Operation, low tracheotomy Aug. 18, 1919. Died five weeks after second operation
	Nil	June 2, 1921. Advised X-rays	—	—	—	—	Died six months later of recurrences in lung
	Gland only	X-rays not advised. (Dr. Jupe)	—	—	Steadily progressed. Rapid increase of glandular involvement	—	—
	In front left ear. Below right clavicle	August 10, 1922. Apparently sarcomatous masses in front of ear and in right pectoralis	Masses dissected out	Secondary carcinoma sarcomatoides. ? primary of thyroid	Good immediate progress. Recurrence in glands of axilla after one year	Given X-rays	Some improvement axillary swelling

and states that the mildest form of carcinoma of the thyroid is presented by those cases which look innocent but are associated with secondary deposits. In many of these cases the rate of progress is so slow and the metastases are so limited in number—it is in fact very common to find only one such deposit in a bone—that it is not uncommonly possible to remove a secondary deposit and then to operate upon the condition in the thyroid with a very good chance of obtaining a radical cure. Unfortunately in a certain number of these cases, even when a metastatic deposit has been removed and the diagnosis of its origin has been made certain by a microscopic examination, it is still impossible to operate upon the thyroid, for the primary lesion is so small that it cannot be discovered.

There occurred in my series only one example of this type of case, that of a male, aged 36, who four years before he was seen had developed a swelling in front of the left ear which had been removed elsewhere. For eighteen months the swelling in front of the ear had recurred, and a second swelling had developed below the right clavicle. The latter in its clinical characteristics suggested a sarcoma in the pectoralis major muscle. Both swellings were excised, the lower with a surrounding area of healthy muscle, the diagnosis of both masses being doubtful. On microscopic investigation it was found that they were formed of a secondary carcinoma sarcomatoides of the thyroid-tissue type, but a most careful examination failed to reveal the presence of any lesion in the thyroid gland. There was no local swelling and no general enlargement. A year after operation he developed enlargements in the axillary glands for which X-ray treatment was advocated. Shortly after this he developed dyspeptic symptoms which suggested that there might be secondary deposits in the liver, but no enlargement of that viscus has yet been determined. With the use of X-rays the condition is, for the time being, held in check, and there is still no palpable enlargement of the thyroid gland.

These cases present extreme difficulties in diagnosis, but they are difficulties which are of very little practical importance; for even if the nature of the metastatic deposit is not recognized an operation is usually undertaken, and with so slowly-growing a primary lesion and a single metastasis this should be regarded as correct treatment even if the condition be recognized. In every case where any change is manifest in the thyroid gland the affected lobe should also be excised.

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Mr. T. P. DUNHILL, C.M.G.,

said that the cases of malignant goitre which had come under his observation had fallen, more or less clearly, into one of three categories.

First, there were those which were, as a rule, plainly malignant when first seen. The goitre was not necessarily large. Its shape was usually irregular. It might or might not have involved the skin. The margins might be well-defined in places, but in other parts of the same tumour the margins were ill-defined and could be felt

involving the surrounding tissues: this meant that the mass was fixed. There might be movement together with the trachea, but the mobility was not such as occurred in a non-malignant goitre. There was usually some interference with respiration; sometimes considerable cyanosis; sometimes, but not so frequently, there was interference with deglutition. In some patients paralysis of the recurrent laryngeal nerve occurred; and occasionally there was spitting of blood, or frank hæmorrhage, due to penetration of the trachea by the tumour, even though the external evidence of the growth was slight. Frequently involvement of the skin of the neck occurred.

Sometimes the gland was quite small but the local discomfort attracted the patient's attention and on examination the extreme hardness—local or general—and fixation were detected. Occasionally symptoms simulating Graves' disease led the patient, or his physician, to realize that all was not well. The signs and symptoms varied in degree; but there was a considerable number of patients in whom there could be no reasonable doubt of the diagnosis at the time when the patient was first seen.

A few rare cases were seen in which evidence of malignancy appeared to be almost conclusive—a large, hard, immobile mass associated with pressure-symptoms and extreme cyanosis; X-ray examinations showed marked extension into the thoracic cavity with apparently ill-defined margins. This was seen in a woody type of sub-acute inflammation with great destruction of thyroid cells and overgrowth of fibrous tissue throughout the gland. It was a condition which had to be dealt with surgically on account of the pressure-symptoms, and necessitated oral administration of thyroid gland subsequently, not because the thyroid gland was all removed but because what was left was atrophic.

(Lantern slides were shown illustrating the following conditions: (1) A large irregular mass in the neck; (2) Involvement of the skin; (3) Extension into the thoracic cavity of malignant and non-malignant goitres; (4) Photomicrograph of a specimen removed at operation in the woody type of chronic inflammation.)

Secondly, there was a group in which the diagnosis was only made by microscopical examination after the tumour had been removed. This group consisted of malignant adenomata. In some of these malignancy had not been suspected before operation: this proved how essential it was that all material removed should be examined by a competent microscopist. It had occurred to him (Mr. Dunhill) at least five times that malignancy had been detected in a specimen when it had not been suspected in an examination of the patient.

It might be submitted that pathological examination of thyroid material was not yet on a sound enough basis for malignancy to be diagnosed in a person who had shown no clinical evidence of it. His reply was that one of these five patients had since died from recurrence. A second had had recurrence in the skin of the neck a considerable distance from the site of the original adenoma. In a third case there had since been wide extension into the neck. The slow evolution of the malignancy in these cases was interesting. In the case of the patient who died some thyroid material had been removed at least ten years previously, and it was then pronounced malignant. In another case an adenoma which had been removed eleven years before was stated to be malignant. Last year this patient had come with a recurrence in the scar, again making a ten-year interval between the diagnosis of malignancy and the onset of recurrence.

(A series of lantern slides of photomicrographs illustrating these recurrences was then shown.)

The third group (Mr. Dunhill said) was most interesting, but he was not prepared to speak definitely about its significance. This group consisted of cases in which there was an enlargement of the thyroid gland in association with progressive enlargement of masses in the neck separate from the thyroid. These masses appeared to be deposits in lymphatic glands and consisted of a cystic papilliferous growth of thyroid

tissue. He had seen four of these cases and three of them were under observation at that time. There appeared to be very little interference with the general health. One patient had married and had a family since material had been removed from her neck for pathological examination by three surgeons on different occasions. She had lost neither weight nor strength and yet she had masses in her neck apparently containing a deposit of thyroid material in lymphatic glands. Some of these masses had occurred as high as the mastoid process, and in one patient the left recurrent laryngeal nerve had been paralysed. This problem was being worked at and would be reported upon later.

(A further series of lantern slides was shown to illustrate the cystic, papilliferous, thyroid tissue occurring in these nodules.)

Section of Surgery.

President—Mr. HERBERT J. PATERSON, C.B.E., F.R.C.S.

Carcinoma of the Œsophagus.

By H. S. SOUTTAR, C.B.E., F.R.C.S.

IN carcinoma of the œsophagus the surgeon is presented with a problem of vital importance to the individual. It is true that he can probably do nothing to effect a permanent cure, but by dexterous management he can do a great deal to relieve a condition as miserable as any that he will meet with in his work, and he can at least render the remainder of life tolerable. The disease is undoubtedly a common one, although it is exceedingly difficult to obtain accurate figures. According to the report of the Registrar-General for the decade 1911 to 1920, there were in these ten years 12,000 male deaths and 4,000 female from carcinoma of the œsophagus, or in all 4 per cent. of the total deaths from cancer. These figures are open to the objection that in the great majority of the cases the diagnosis rested on opinion and was not verified by any scientific method, but they correspond fairly with the statistics of other countries and of large hospital groups.

The disease is more common in men by about three to one or four to one, but this statement, as we shall see later, requires modification when the site of the disease and the age of the patient are considered.

The disease has three sites of election—the upper end, the middle, and the lower end; and at these points there is a definite narrowing in the tube at which a bolus of food is slightly checked, and at which, perhaps from this very circumstance, traumatic strictures, from the swallowing of corrosive fluids or hot water, are most likely to occur. The central narrowing is at the point where the œsophagus is crossed by the left bronchus, but it has been suggested by some observers that this is a mere accident and that a momentary spasm of the central part of the œsophagus is the true explanation of its liability to trauma.

Sex plays a large part in the incidence at each site. In men, carcinoma occurs in these three situations roughly in the proportions of 1-2-3. In women, however, the proportions are 3-1-1, more than 60 per cent. occurring at the upper end.

The age incidence, again, is entirely different in the two sexes. In men carcinoma of the œsophagus is a disease of later life, 96 per cent. of the cases occurring after 45 and 88 per cent. after 50. In women, on the other hand, it is by no means uncommon even at the age of 30, and after 40 the distribution is fairly even. This extraordinary difference in the incidence of carcinoma in the two sexes both as regards site and age is due to the remarkable fact that post-cricoid carcinoma is almost limited to women and occurs at a comparatively early age. The only explanation that has been offered is that a nervous spasm of the œsophagus, sometimes accompanied by severe dysphagia, occurs as a definite neurosis in young women. In some cases such a spasm has been definitely followed by the development of a carcinoma at the same site, and indeed this should always be suspected where a verified carcinoma in the post-cricoid region gives a history of dysphagia extending over several years.

PATHOLOGY.

The disease is usually of the squamous epithelial type. There are numerous cell nests and keratinization is marked. A form with rounded polygonal cells, presumably arising from the glands, is less usual. At the lower end a cylindrical type is sometimes seen, but it has usually invaded the œsophagus from the stomach,

although it is occasionally found in other situations, perhaps arising from isolated gastric cells. Just behind the cricoid, indeed, it is quite common to find in the normal œsophagus a small nodule on the anterior wall identical in structure with the gastric mucosa.

These growths show a strong tendency to surround the œsophagus by a lymphatic spread and to produce stenosis, but they occasionally remain localized to one portion of the wall, and in such cases the condition is likely to give rise to no symptoms and to be found accidentally at a post-mortem examination. As in the case of carcinoma occurring elsewhere the actual type will depend largely on the reaction of the surrounding tissues. We may find a dense fibrous scirrhus with severe stenosis, a deep ulcer possibly destroying the whole of the œsophageal wall, or a soft fungating mass with a profuse discharge, bleeding readily on contact. Usually single, the growths are occasionally multiple, having spread along the deep lymphatics from a single source, and it is important to recognize that more than one stricture may possibly be present. The extent of these growths and the degree of destruction they can produce without actually causing death are astonishing. The œsophageal wall may, over an extent of several inches, be replaced by a sloughing mass of growth which infiltrates all the surrounding structures, protecting them indeed by its infiltration. Perforation into the mediastinum may give rise to an abscess tracking in that region, but perhaps perforation of the respiratory tract with the development of an acute septic broncho-pneumonia is the most common termination. Occasionally the aorta itself is opened and an acute hæmorrhage terminates the scene. The neighbouring lymph glands are involved and it is not uncommon for infection to spread by the thoracic duct to the glands in the root of the neck on the left side. Occasionally these glands may form such a mass as to obstruct the œsophagus at its upper end, and the existence of a primary lesion lower down may not have been suspected. Secondary growths in different parts of the body are unusual, but this is due rather to the rapid course of the disease than to any inherent lack of malignancy. Considering the advanced age of most of these patients we must regard carcinoma of the œsophagus as a highly malignant growth, destroying the vital structures by which it is surrounded with such great rapidity that no opportunity is presented for the development of distant secondary deposits.

Of the structures which may become involved one group remains to be mentioned, namely, the nerves. Involvement of the recurrent laryngeal may occur at a very early stage. Paralysis of the corresponding vocal cord results, and the hoarseness which follows may actually be the first sign of the disease. Involvement of the dorsal roots more often occurs late, but in any severe intercostal pain of unexplained origin the œsophagus should always be remembered as a possible source.

SYMPTOMS.

In the large majority of instances the first complaint of a patient with carcinoma of the œsophagus will be a steadily increasing dysphagia. Occasionally the onset is quite abrupt, and the patient may be under the impression that some foreign body, such as a fish bone, has become impacted. The cause of his difficulty, however, is generally a reflex spasm at the upper end of the œsophagus, and not the actual mechanical obstruction. As we have already seen, such a spasm is a not uncommon neurosis in young women, but occurring in a man past middle life it ought to rouse suspicion and to lead to careful examination. The patient will often complain that food is brought back almost immediately in an unaltered condition, but the vomiting of large amounts does not occur, since carcinoma is rarely associated with any great degree of dilatation, contrasting in this respect with cardiospasm. In some cases the irritation of the growth causes a secretion of stringy mucus which may collect in considerable quantities during the night; in the morning a quantity of this material may be regurgitated, and the patient may be so far relieved that he can swallow

liquids with comparative facility for the rest of the day. Where much ulceration has occurred the mucus may be blood-stained, and so foul as to produce marked fœtor of the breath.

Although dysphagia is his first complaint, it is really a symptom of advanced disease, since it necessarily indicates stenosis. A careful investigation will often show that for some time before it occurred the patient was in poor health, had lost weight, and had an indifferent appetite, probably the result of septic absorption from an ulcerated growth not sufficiently advanced to produce obstruction. In any man past middle life with such symptoms the possibility of carcinoma of the œsophagus ought not to be forgotten. Occasionally it may be an early symptom, and in carcinoma arising in the post-cricoid region, pain in the ear is almost pathognomonic. Most usually it occurs late, and may be referred to the sternum or epigastrium; it is dull discomfort rather than an acute pain, and is due to the reflex spasm, which results from attempts to swallow. It is to be distinguished from the acute pain which occurs late in the disease, when the growth has extended so far from the œsophagus as to involve the dorsal roots. Occasionally hoarseness and aphonia occur early from involvement of the recurrent laryngeal nerves and paralysis of one or perhaps of both vocal cords. Of the later symptoms, perhaps those which depend upon the involvement of the respiratory passages are of the most importance. To the profuse secretion of tenacious mucus which follows involvement of the upper passages we have already referred. It is often accompanied by a constant, irritable cough which is very distressing. If the tracheal wall itself is invaded, actual mechanical respiratory embarrassment will occur, at first from swelling of the mucosa, and later from actual perforation of the wall by the growth. An œsophago-tracheal fistula may form, and the swallowing of liquids may give rise to violent attacks of coughing. It is astonishing how long such a fistula may exist before a septic pneumonia puts an end to the patient's misery. Wasting is invariable and the loss of weight may be very rapid. This is due to a combination of starvation with septic absorption, for it sometimes occurs before dysphagia has become a marked feature. That it is largely the result of starvation is, however, shown by the rapidity with which the patient may regain weight after intubation. On the other hand, the general misery of the condition, the inability to swallow, the constant pain, and the persistent cough which so often occurs, make sleep almost impossible, and lead to rapid deterioration in the health of the patient.

EXAMINATION.

In a general examination there are certain points which should be specially noted, such as the degree of wasting, a marked anæmia, enlargement of glands in the neck or abdomen, any change in the voice or difficulty in respiration. Apart from these, the examination will centre itself upon the œsophagus, and the degree, the site and the nature of the stricture. For this, three methods are at our disposal: bougies, X-rays, and direct examination with the œsophagoscope.

The question of the passage of bougies is one about which opinions are greatly divided, but in our opinion they should only be used apart from an œsophagoscope when no other method is available. Many of the carcinomata are exceedingly friable, and it is very easy to perforate them. In addition, the orifice of a stricture is very often at one side, and by no means in the centre of the œsophageal lumen, so that the injudicious passage of a bougie may easily lead to perforation of a pouch of the œsophageal wall, the bougie never having engaged in the stricture at all.

X-ray examination, on the other hand, is entirely safe, and when skilfully carried out gives direct evidence not only of the site of the lesion but of its nature. It is best carried out by giving the patient a thick emulsion of barium sulphate, when it will be

seen to trickle through the tortuous lumen of the stricture in a way which is characteristic of malignant disease.

The œsophagoscope should be a final court of appeal. In skilled hands it is perfectly safe, and gives evidence which can be obtained in no other way, but it should only be used by those who have devoted some attention to the subject, as in unskilled hands it is exceedingly dangerous and misleading. The normal œsophagus appears as a transverse slit, opening and closing with each respiratory movement. In carcinoma this movement is abolished, and the appearances may vary between that of a puckered scar, a fungating mass of growth, and a deep ulcer. The great object of œsophagoscopy, however, is for treatment rather than diagnosis.

TREATMENT.

During the past few years a great deal of attention has been paid to the possibility of the removal of tumours of the thoracic œsophagus and the reconstruction of that tube. The operation is surrounded by extraordinary difficulties and no completely successful case has yet been recorded. It is true that Torek resected one tumour but he did not reconstruct the œsophagus, whilst Lilienthal reconstructed the œsophagus but left his patient with a fistula. In our opinion these attempts are founded on an entire lack of appreciation of the nature of carcinoma of the œsophagus. We regard it as a highly malignant growth, rapidly invading surrounding structures and producing an extraordinary degree of destruction with great rapidity. The fundamental symptom of dysphagia occurs only at a late stage when stenosis has already occurred, and hence it is improbable that the surgeon will be called upon to deal with these tumours before they have reached a stage when removal is totally impossible. Added to this the patients are usually elderly and utterly unsuited for an extensive plastic procedure, so that in our opinion the question of radical removal is one that should not even be raised.

Radium and X-rays have both had strenuous advocates, but opinions on the subject are very divergent, and one cannot regard as hopeful in the treatment of these inaccessible tumours methods which have so signally failed in the treatment of carcinoma in more accessible regions. There remain for consideration the methods by which we can give relief to the dysphagia, namely, dilatation, intubation and gastrostomy.

Dilatation is certainly carried out with the greatest safety by means of the œsophagoscope, provided that the operator has acquired sufficient skill in the use of this instrument. Unless the orifice of the stricture can be actually seen, the blind passage of the bougie is only too likely to result in perforation of the œsophageal wall. In my own method of treatment I pass in full view a fine bougie, the terminal portion of which is flexible, and over it are slipped the larger bougies which it guides through the stricture. Sometimes a stricture which proved to be impassable to a bougie can be surmounted by the ingenious method of Plummer. In this a patient gradually swallows several yards of stout silk; it is gradually washed through the stricture and passes so far down the intestinal canal that the upper part can be drawn tight; over this can be slipped a series of perforated bougies as in the method above described.

The immediate result of dilatation is usually a considerable relief which may last for several weeks, when the obstruction may recur and require further dilatation. Provided too long an interval is not left this can generally be done quite easily without the passage of an œsophagoscope, but it is by no means free from risk, and the risk of actually perforating the growth necessarily increases with its progress. To avoid this risk several plans have been suggested. Hill passes a rubber-covered wire alongside which the patient can swallow; Symonds inserts a gum elastic tube which is anchored by a silk thread to facilitate withdrawal when it becomes blocked or foul. Both methods have given good results, but each has some inconvenience for

the patient. I have recently devised a tube the mechanism of which gets over some of these difficulties; I have been using it with considerable success.

This new tube consists of a spiral of gilded german silver wire with the upper end expanded in the form of a funnel. The tube is oval in section and is given a spiral twist to prevent upward movement in the event of vomiting. The tubes are very flexible and allow of a very large lumen so that they never become blocked, whilst the patient is quite unconscious of their presence. They are introduced through a large œsophagoscope after dilatation, and they are retained without difficulty, although on a few occasions they have ultimately passed right through the growth and have been passed by rectum. On no occasion have I seen any sign that they might work their way through into the neighbouring spaces. The tube lies in the lumen of the growth itself, which supports it, and it should exert no pressure on the œsophageal mucosa either above or below. A remarkable feature of this tube is the facility with which the patient can swallow, not only fluids but any solids which he can masticate satisfactorily, so that the increase in comfort and in nutrition of the patients is often very marked. In some cases the tube has remained in place for as much as a year, the patient swallowing with facility during the whole of that period, whilst in every case in which introduction of the tube has been possible, relief has been immediate.

If dilatation is impossible, gastrostomy is the only resort, and it is not an operation to be lightly undertaken. As already stated, these patients are usually elderly and in a bad condition for a plastic procedure, and the mortality from gastrostomy is out of all proportion to the dimensions of the operation. In spite of an occasional, brilliant success, the patient and his friends are usually very dissatisfied with the result, and at the best it is a miserable existence. On the other hand it is undeniable that occasionally a patient on whom a gastrostomy has been performed may regain his power of swallowing. If, therefore, the operation is to be performed it should be at the earliest possible moment when the patient is in fair condition and the risks are not so great; but under even the best circumstances it is a poor substitute for intubation, which I certainly hold to be the method of choice.

Examination of the Œsophagus by X-rays.

By GEORGE VILVANDRÉ, L.R.C.P., M.R.C.S.

THE examination of the œsophagus by means of X-rays and barium meal is of definite value. It entails no pain to the patient and is fraught with no danger. In the great majority of cases a correct diagnosis can be made if the examination is conducted by an expert. Not only the œsophagus but also the upper and posterior mediastinum are examined at the same time, and the thorax as a whole.

The presence or absence of an aneurysm is detected, and if this be present, no risk whatever is encountered. Cases that come described as gastric, are sometimes found in the X-ray room to be œsophageal. Most œsophageal cases come late for diagnosis, the symptoms are insidious, and often the patient does not seek medical aid until some sign of obstruction is present.

The X-ray evidence is dependent upon some irregularity of the œsophagus, some delay or hesitation in the progress of the barium meal or pellet. I am certain that the least degree of abnormality in the swallowing of an opaque meal or even slight delay of an opaque pellet should never be lightly dismissed, and that the least suspicious case should be further followed by the passing of the œsophagoscope and direct vision of the mucous membrane of the gullet. The use of a small, soft rubber bag filled by a tube with barium so as to show the least alteration in the contour of the œsophagus is a valuable help. Some hold that pellets are liable to stick

without disease being present. There is usually delay in the progress of a pellet just above the cardia. When the sphincter relaxes the pellet passes. Fortunately, in most people the lower end of the oesophagus is well seen in the semi-lateral position. One cannot here give a full description of the signs encountered in the different diseases of the oesophagus, but some of the pitfalls to be avoided in the differential diagnosis of several conditions may be indicated as follows. Much dilatation usually means a chronic stricture. In carcinoma little or no dilatation is present. In this also irregularity of the stricture is the rule. A dilated oesophagus with delay or obstruction at the cardia, accompanied by irregularity of the barium outline in its lowest portion, may be due to a cardiospasm, or spasm of the oesophagus, with food present in the lowest segment. Morsels of meat previously swallowed and held up above the cardia will cause irregularity. In such cases washing out the oesophagus will clear the diagnosis. The aortic impression upon the anterior oesophageal wall should not be a source of error in familiar hands, and experience will enable one to note its size.

Extra-oesophageal pressure should be readily detected, and will be seen to cause oesophageal deflection to one side or the other. It is encountered in aneurysm and in new growth of the mediastinum, either malignant, or simple, or tubercular, and in the presence of a cyst or lymphadenoma. "Pull" upon the oesophagus by extensive fibrosis and other conditions will be at once recognized by the presence of adventitious shadows in the thorax.

In my experience the most common disease met in the oesophagus has been carcinoma, next spasm of the oesophagus, so-called cardiospasm. Simple strictures, such as are due to swallowing of corrosive fluids, have been most rare.

Oesophageal pouches are met with now and then. Here again irregularity at the bottom of an oesophageal pouch is due to food. The patient can often empty the diverticulum by pressure with the finger, and the examination when repeated will show a smooth outline. As Finzi has pointed out, pouches empty from the top. They fill first, and then empty the following stream into the lower oesophageal channel. In other words, they overflow.

In carcinoma, in my experience, the most frequent site has been the middle third of the oesophagus, or, more accurately, the place where the left bronchus crosses.

By means of X-rays the length of the stricture is observable; in oesophagoscopy alone, only the upper surface. Also, with X-rays the direction of the channel is seen, whether straight or tortuous. These facts are of importance when the insertion of a tube is contemplated. When once this has been done X-ray examination will show whether the tube fits well or loosely, for if it fits loosely barium will be seen passing between the inserted tube and the diseased mucous membrane or growth, this causing further irritation. The presence of a tracheo-oesophageal fistula may also be observed, and in the few cases I have seen the barium which passed into the bronchi caused no appreciable ill effect.

Section of Surgery. SUB-SECTION OF PROCTOLOGY.

President—Mr. F. SWINFORD EDWARDS, F.R.C.S.

DISCUSSION ON FISTULA.

Mr. SWINFORD EDWARDS (President), introducing the subject of fistula in ano, said he considered this lesion to be a specialty within a specialty. There was no doubt that more mistakes were made by the ordinary surgeon when operating for fistula than in any other rectal lesion.

The genesis and pathology of fistula were then described, with special reference to the horseshoe and semi-horseshoe varieties, and he (the President) pointed out that the latter was much more frequently met with than was generally supposed.

The usual site of an internal opening was in the mid-line dorsally, low down between the two sphincters. When such was the case it always indicated some variety of horseshoe fistula, in other words a track more or less curved running to a point between the anus and coccyx, and from thence in a direction forwards to the internal opening. In laying open this track the external sphincter would be divided in the mid-line dorsally, i.e., at right angles to its fibres, and so facilitate healing without loss of power.

He (Mr. Edwards) considered that the following were the chief reasons for the failure of operative interference.

(1) Lack of topographical knowledge, the operator thereby failing to lay open the whole of the track or tracks.

(2) Non-recognition of fistulae due to caries of the coccyx or tuber ischii, or due to the lodgment of a foreign body in a sinus which the surgeon had failed to lay open.

(3) Tubercle: Operative interference in tuberculous subjects was often quite successful, though one did meet with cases in which the healing process was delayed or even abortive.

(4) The omission to remove all overhanging margins of skin and mucous membrane, and the neglect to employ what was known as Salmon's "back cut," by which two manœuvres an endeavour was made to render the wound more superficial, broader and less deep.

(5) Injudicious after-treatment, such as painful and too frequent dressings, and over-long confinement to bed.

He (Mr. Edwards) further said that in very extensive cases of horseshoe fistulae it was well to divide the operation into two parts. At the first, all lateral tracks should be laid open into the main posterior one—but this should not be laid open into the bowel. The wounds were thus extra-rectal, and need not be contaminated with feculent material. After three or four weeks, when these wounds were almost healed, a slight secondary operation would complete the cure by dividing the main dorsal sinus, which, of course, included the external sphincter. This second stage was really only division of a small dorsal fistula.

He (the President) asked for opinions on treatment by ionization, of which he personally had no experience.

As to excision with immediate suture he saw no reason for substituting this for the old method of healing by granulation. In order to save a week or two in convalescence, risk was run of the wound breaking down or of the formation of one or more stitch abscesses, with further surgical interference.

Sir C. GORDON-WATSON said he agreed as to the misleading impression usually conveyed by text-books in the matter of tuberculous fistulae. Such cases were by no means hopeless. Many fistulae might be tuberculous without the patient being tuberculous. In the latter case the fistula might do badly, but not in the former. Tuberculosis was no obstacle to a cure if the surgery and the after-treatment were correct. Referring to the President's statement that anterior "horseshoe" fistulae were unknown, he said he thought that this indicated a difference in nomenclature, as the "branched" fistulae, approximating very closely sometimes to a horseshoe shape, were undoubtedly found anteriorly. The high second internal opening was usually that of a submucous track only, but when not submucous the granulation was very slow. He had at times in very obstinate cases ventured to open them up into the bowel, and although incontinence had resulted it had not been permanent.

With regard to primary suture, he said he thought that this question had been influenced by experience in the war, when a great amount of work of a very important character was done. At one period in the war a certain number of beds at one of the casualty clearing stations were set apart solely for the purpose of investigation in connexion with primary suture of wounds, and he said he believed that something had been learnt about primary suture in consequence, and that better results were now forthcoming than could have been obtained without that experience. In the case of a direct fistula an excellent result could be obtained by primary suture, and the time of convalescence considerably shortened. A little while ago he said he had brought forward a series of seventeen cases of primary suture; the results were all exceedingly good, with a high average of primary union. Some were done by excision and primary suture treatment with flavine, and some with excision and primary suture treatment with bismuth and iodoform paste. Nevertheless, the indiscriminate use of this method was very dangerous. If it became a routine it would lead to disasters, and its proper use required a considerable experience of cases and an ability carefully to select them. He had found that Finsen light stimulated sluggish lesions to heal.

Mr. W. ERNEST MILES said that it seemed to him that the reason why many surgeons did not always obtain successful results after operations upon ano-rectal fistulae was because they did not appreciate the fact that these fistulae differed widely in type. He said he considered that the usual classification of fistulae into complete, blind external, blind internal and bilateral (horseshoe) varieties was misleading because it conveyed the impression that they were varieties of a single type, differing only in degree and therefore suitable for the same kind of operative procedure. The usual method employed for operating upon fistulae was that by incision, the whole of the tissues overlying the fistulous track being completely divided from one end of the track to the other, that is to say, from the external to the internal openings when both existed. Therefore, if the varieties of fistulae met with were merely varying degrees of the same type, this simple operative procedure should be followed by uniform results. It was a matter of common knowledge, however, that when fistulae were laid open in this way, from the external to the internal openings, the surgical end-results, although entirely satisfactory in some cases, were in others very bad indeed. His explanation of this was that fistulae varied considerably as to the relationship of the main track of the fistula to the muscular structures concerned in the anatomy of the outlet of the rectum. He drew attention to the fact that the precursor of every fistula was an abscess, and that the main track of a fistula was the contracted but unobliterated cavity of the pre-existing abscess. Ano-rectal abscesses varied as to their anatomical position. They developed in the subcutaneous tissue, in the submucous tissue of the anal canal and rectum, in the ischio-rectal fat, and in the loose cellular tissue of the pelvi-rectal space. When such abscesses discharged their contents through an opening, either on the skin surface or within the rectum or in both of these situations, a fistula resulted; and the type of the fistula was determined by the anatomical position of the original abscess cavity which then persisted as its main track. There were, therefore, four types of fistula, namely, the subcutaneous, the submucous, the ischio-rectal and the pelvi-rectal, each of them presenting blind external, blind internal, complete and bilateral varieties.

The most serious and, therefore, the most important consequence of an operation upon a fistula was its effect upon the sphincteric control of the rectal outlet, the degree of impairment being commensurate with the extent of the damage inflicted upon the muscular apparatus. The laying open, from end to end, of a fistulous track, of either the subcutaneous or the submucous types, was not followed by loss of control, because in them muscular tissue was not divided: and in the ischio-rectal type the only muscle damaged was the external sphincter, the power of contraction of which was not affected unless its fibres were severed obliquely or in more than one place. But, were a similar procedure to be carried out upon a fistula of the pelvi-rectal type absolute loss of control would result, because, not only the external sphincter but in addition the internal sphincter, the levator ani and both muscular coats of the rectum, up to the level of the internal opening, were divided. He found that among writers upon the operative treatment of fistula a much contested point was the expediency of completely laying open a fistulous track into the bowel the internal opening of which was situated at a high level in the rectum. Some said that they laid open such fistulae without any ill-effects whatever supervening, whilst others asserted that such a procedure was invariably followed by irremediable incontinence. It would seem difficult to reconcile two such divergent expressions of opinion unless it were conceded that both were right and yet both were wrong. It was obvious that when two experienced surgeons held opposite views upon a point such as this, the only possible explanation was that, whereas the experience of one must have been limited

to fistulae of the submucous type, the other had been unfortunate enough to encounter only pelvi-rectal fistulae.

He (Mr. Miles) agreed with the opinion expressed by the President that whenever a fistulous track was situated externally to the muscular coat of the rectum, and communicated with the bowel through an internal opening placed above the level of the internal sphincter as found in the pelvi-rectal type, it should never be laid open into the rectum. His experience was that these extramural tracks, even when two or more inches in length, could be induced to heal progressively and completely by dividing the extramural tissues freely in such a manner that a pyramidal-shaped wound was created, the base of which was on the skin surface. He also emphasized the fact that one of the chief causes of failure to obtain satisfactory healing of the wound, after an operation upon all types of fistula, was that the surface wound was not made sufficiently large. He pointed out that an extensive surface wound was not so necessary in the superficial types, but that when the main track was situated at a considerable depth from the surface, as in the ischio-rectal and the pelvi-rectal forms, then the area of the surface wound should be made to exceed in extent the combined areas of the walls of the cavity which had been created by the laying open of the fistulous track in its entirety. He found that when the surface wound was not made large enough the superficial portion of the wound cicatrized before the deeper part had had time to fill in completely, with the result that either an intractable sinus remained or the surface wound finally healed with a depressed scar; this was a condition always to be avoided, because such a scar, when extending into the anal orifice, was a common cause of rectal incontinence even when the muscular apparatus of the rectal outlet had escaped injury.

MR. ASLETT BALDWIN.—In regard to the deep sinuses running up outside the rectum, I have caused a number of them to heal by ionization. A bare, pure zinc electrode, attached to the positive pole of a continuous current, is introduced along the whole length of the track, generally without anaesthesia. Other slowly healing parts can be greatly assisted by ionization with zinc sulphate. Some time back I had a case of apparently anterior horseshoe fistula; it healed by first intention after having been sewn up completely. Some years ago a man came to me with an extensive posterior horseshoe fistula. A large area of the skin around was fiery red from septic inflammation. A previous President of this Section had operated on him without benefit, and had then told him that no further operation would be of any use. It could easily have been so cured, but I had to try something else. I ionized the whole of the tracks with a zinc electrode. To do so I had to make various additional small openings under local anaesthesia. Healing along the fistula apparently took place and the whole of the dermatitis disappeared. Occasionally a spot of pus appeared afterwards at one or another opening, when ionization was repeated. In this way the patient was kept comfortable to the end of his life, which came several years afterwards. Ionization is of great use to the rectal surgeon.

There is one useful practical point I learned from Mr. Edwards years ago, which I will mention for the benefit of those who are not used to operating on horseshoe fistulae. In opening them up the director should be passed as far it will go easily along the track, then the point is made to elevate the skin and is cut down upon and pushed through. The director is then withdrawn, entered at the new opening and passed further along the track, and so on till the whole is laid open.

MR. IVOR BACK referred to the fact that the internal opening of a fistula was nearly always at the level between the two sphincters, and asked for any explanation. He himself thought that it might be due to the tearing down of an anal papilla. In cases in which a high internal opening was found, there was always a low one also at the usual level. The high opening represented the upper end of a submucous abscess. In pelvi-rectal fistula the track led through the levator ani, and there was no internal opening. Such cases were primarily pelvic in origin, and were not due to disease of the rectum. The treatment differed in each class of case, and success depended on a realization of the exact state of affairs present.

Excision and primary suture of a straight fistula (not the horseshoe type) succeeded in a percentage of cases, but it was essential to realize that the sutures must include the base of the cut area so that no vacuum was left.

He had been much impressed by the value of helio-therapy in fistulae which were sluggish in healing up; in his experience natural sunshine was more efficient than artificial light, but patients had to be sent abroad for this treatment on account of our climate.

Mr. LIONEL NORBURY said that he agreed with the statement made by Mr. Ivor Back that in cases of direct fistula the track usually passed below the level of the external sphincter, although the thickness of tissue divided at the operation often suggested that the external sphincter must have been included in the incision. In his experience the fistulous track very often passed through the fibres of the external sphincter, which would then be partly divided at the operation.

He (Mr. Norbury) did not agree with the rather pessimistic accounts given in certain text-books that tuberculous fistulae would not heal even if treated in a radical manner. He had had several patients suffering from active phthisis in whom extensive fistulae had healed in a comparatively short time after radical operative measures. In such cases, spinal anaesthesia was often indicated. It was important for such patients to continue sanatorium treatment, if possible, during the healing process.

Mr. Norbury cited a case of malignant disease supervening upon an extensive fistula, and he asked the President for his experience with regard to this complication.

Section of Surgery.

President—Mr. HERBERT J. PATERSON, C.B.E., M.C.

DISCUSSION ON THE MORTALITY OF APPENDICITIS.

Mr. JOSEPH E. ADAMS.

THE incidence of a disease and its mortality are the two criteria by which the capacity of the medical profession should be judged. To prevent we must eliminate cause. The cause or causes of appendicitis have been a subject for both speculation and research for more than thirty years, but little has been established beyond the fact that the appendix is a dangerous possession. Its removal is therefore the surest method of preventing appendicitis, and the practice of removing an appendix which is to all appearances healthy, when the abdomen is opened for some other operative procedure, is quite sound provided it does not delay matters and add to the difficulty of the operation in hand. There has been talk of a function for the appendix, but as far as I am aware no one really knows what it is, and, surgically speaking, it is not an organ—it is a nuisance. Unfortunately the risk of an abdominal operation can never be reduced to that of having one's hair cut, and therefore preventive appendicectomy is customarily limited to those cases where there is evidence of a present or past attack.

Unless an epidemic occurs it is difficult to be dogmatic concerning the incidence of any particular disease. The increase or decrease can be gauged by personal impressions on the part of practitioners, by the collected statistics of institutions, and, lastly, by the returns of the Registrar-General. The last mentioned are certainly the most reliable source of information, but unfortunately they deal only with fatal cases. Mortality is not a fair criterion of incidence. If it be accepted that where mortality figures show an increase this must mean a rise in incidence; then much of what I wish to say becomes pointless. The chief value of the Registrar-General's statistics is that they cover the whole of England and Wales, bearing relation to some forty million inhabitants. Institutional figures are not easy to obtain, so that they can all be massed in one total, and the impressions of practitioners, so far as I have been able to benefit by them, do not support the view that appendicitis is noticeably on the increase. The question has been carefully studied by Rendle Short [1]. He states that "appendicitis did not become a separately notifiable and recorded cause of death until 1901." This was about the time of King Edward's historic operation for appendicular abscess on the eve of his coronation. The figures Rendle Short quotes end with 1918. His conclusions are important, and those which bear most directly on our subject are as follows:—

(1) Appendicitis was present, but was relatively rare in this and other countries until the end of the nineteenth century. Since then it has become very common in most highly civilized countries.

(2) The rise in Bristol, and probably throughout England, was beginning in 1895, and was pronounced between 1895 and 1905; since then it has been fairly stationary.

(3) This rise was most marked at first in towns, in the male sex, and probably amongst the better-off class.

(4) The privations of war did not reduce it.

Six years have passed since 1918 and the mortality is certainly higher. In 1918 the number of deaths shown by the figures published by the Registrar-General was 2,416; in 1923 it was 2,826. If the last part of Rendle Short's second paragraph is correct we have ample justification for inquiring why the returns of the Registrar-General show a steady rise in the death-rate. I show below the complete figures

from 1913 to 1923 inclusive, with their relation to the population (irrespective of sex). I take 1913 because it is a pre-war year, and 1923 because it is the last year available. The figures for the war years refer only to the civil population, and as this meant the withdrawal of a large number of males from the statistics they are no doubt vitiated.

Deaths from Appendicitis and Typhlitis.
(Registrar-General's Returns.)

Year	Male	Female	Total	Persons per million
1913 ...	1,387	1,141	2,528	69
1914 ..	1,573	1,111	2,684	71
1915 ...	1,441	1,062	2,503	67
1916 ...	1,474	1,129	2,603	70
1917 ...	1,390	1,053	2,443	67
1918 ...	1,298	1,118	2,416	66
1919 ...	1,278	1,151	2,429	66
1920 ...	1,389	1,142	2,531	67
1921 ...	1,474	1,237	2,711	72
1922 ...	1,524	1,211	2,735	72
1923 ...	1,608	1,218	2,826	74

It may be urged that deaths from peritonitis of unstated origin will show a decrease *pari passu* with the rise in appendicitis deaths. Such, indeed, is the fact, but the total decrease from about 490 to 420 is far too small a figure to afford an adequate explanation for an addition of nearly three hundred deaths from appendicitis. Nor do I think that any satisfaction can be extracted from the variation of the total number of inhabitants of England and Wales. A sufficiently striking contrast is afforded by the difference between the year 1920, when demobilization of the Army was completed, and 1923, the respective figures being sixty-seven and seventy-four deaths per million. These are post-war years, and my own view is that we should be justified in looking for a decrease. Instead there is a rise in these deaths, which most surgeons would be inclined to regard as preventable.

In order to gain further information as to the incidence of the disease I have obtained figures from the leading hospitals and Poor Law institutions in the London district. These should reflect the incidence in the London population, and I have endeavoured to get figures down to the year 1922. The hospital cases show slight diminution since 1912, and the infirmary figures a definite increase. The explanation of this turnover from hospital to infirmary is that the general practitioner, realizing the urgency of cases of appendicitis and the lack of empty beds in the hospital, prefers to send his cases where he knows they will be admitted without delay. This attitude is, no doubt, correct, and it remains only to demand that the treatment of the infirmary shall be as good as that of the hospital. The mere change of name from "infirmary" to "hospital," which has become the fashion since the war, will not suffice. There must have been a change of spirit and an increased keenness and capacity on the part of the Poor Law medical officers. Such I believe to have been the case, but I regret that these institutions collectively have been unable to supply detailed figures which could be instructively tabulated. The surgical equipment of many of them may well be the envy of the voluntary hospitals, but down to ten years ago this was not the case, and I believe the merits of surgery were largely overlooked by the guardians until the war enlightened them.

Both hospital and infirmary authorities have kindly replied to my inquiry, but the figures obtained are not sufficiently uniform to be massed together. Such figures have little or no bearing on the occurrence of the disease amongst the rural population, as it is recognized that long journeys are forbidden for acute cases, and most country practitioners do not trouble the London hospitals with their appendix cases. So far as the evidence of general practitioners has been available I have not found that it points to any particular increase in either the incidence or the severity of attacks of appendicitis.

MORTALITY.

At this stage of my argument I propose to leave the statistical method and consider the pathological side of the question. The death-rate is certainly proportional to the degree of sepsis present before the case comes under treatment. If an inflamed appendix can drain into the cæcum the attack will subside favourably, and this is the state of affairs in catarrhal appendicitis, the mortality of which is *nil*. If a diseased appendix drains by perforation into the peritoneal cavity the prognosis is grave. A localized abscess may form, and this may be successfully dealt with by nature or the surgeon. There is little doubt that appendicular pus may be absorbed and disappear without anything more serious than temporary impairment of the patient's health. Appendix abscesses also do burst into the bowel, and if pus is discharged with the faeces the patient may escape the consequences of perforative appendicitis. The two conditions which may precede perforation are ulceration of the wall of the appendix or obstruction of its lumen. In the latter case the tension inside the appendix ultimately becomes so great that it bursts like a distended paper bag, and most surgeons have experienced this during a difficult operation for appendicitis in the obstructed stage. In the former condition local necrosis of the wall, proceeding from mucosa to serosa, occurs without distension. There is no doubt as to which is the more serious event. The ruptured appendix pours a quantity of infected material into an unprepared peritoneal cavity. The ulcerated appendix is rather comparable to the leaking gastric ulcer. Protective steps on the part of that very useful ally, the omentum, are likely to have been taken, and a perforated appendix is not infrequently rendered almost harmless by being wrapped up in a covering of omentum. The analogy with the large and small gastric perforation is striking. It is rare to find evidence that the omentum has made any attempt to lessen the size of a large perforation of the stomach. Small perforations, on the other hand, are often obscured by reason of the reaction of the omentum, that energetic "plumber" of the peritoneum.

If, then, the mortality of appendicitis is to be abolished by treatment of the disease, sepsis must be eliminated, and the problem becomes one of early diagnosis. This is an ideal which is not likely to be attained, certainly not until it is recognized by the public in general, and mothers in particular, that stomach-ache is not an indication for a dose of castor oil. The mortality of appendicitis ever since it was first classified by the Registrar-General as a cause of death has always been highest between the ages of 5 and 15. This corresponds to the period when the mother is most apt to dose her offspring with aperients, and also to that at which the doctor finds the interpretation of symptoms extremely difficult. The liability of the doctor only begins with the first consultation, and the treatment of appendicitis is appendicectomy. Medical experience all goes to show that there are two safe periods for operation—one within twenty-four to thirty-six hours of the onset of disease, the other when the inflammation has subsided. With regard to operation in the acute stage, the capacity of the peritoneum is such that although sepsis may be encountered the convalescence of the patient is rarely affected by it. This means that the mortality is little higher than that of the interval operation, which is usually about 0.5 per cent.

TREATMENT.

If the incidence has not gone up, why is it that the number of deaths has increased? Can there be anything wrong with our treatment of appendicitis? In his presidential address to the Section of Surgery of the Royal Society of Medicine two years ago Sir James Berry [2] raised this question. He called attention to the fact that both the public and the medical profession were under the impression that the custom of operating freely for acute appendicitis had resulted in great saving of life. Would that the Registrar-General could support this optimistic view. Sir James Berry hinted that all was not well with the surgery of the appendix, and that greater

care was needed in the selection of cases for operation as regards both time and manner. It may therefore not be out of place to review briefly some of the important steps in the history of the treatment.

We start with the beginning of this century. It then required a physician to diagnose the disease and a surgeon to operate on it when required. This plan gave the physician a deal of anxiety and caused the surgeon considerable vexation. Frequently the operator complained that he was not called in soon enough. The physician's attitude gradually underwent a change, and Sir William Osler crystallized the position when he wrote in his famous text-book, "There is no medicinal treatment for appendicitis."

There being no scope for drugs, and the diagnosis becoming simple, the physician tended to withdraw from the case. This was the surgeon's opportunity, and the late Edmund Owen, rather more than twelve years ago, was perhaps the most insistent on immediate operation, and immediate appendicectomy, as soon as the diagnosis was made. Postulating that no one could tell the actual condition of the appendix by abdominal examination he made out a strong case, and I think I am right when I say that he carried the bulk of surgical opinion with him. Discussion there certainly was, but the main effect was to enlighten the public and to place the treatment of appendicitis on a firmer surgical footing. The notable achievement was that the practitioner learnt to rush his appendix cases into a surgical home, or hospital, as early in the disease as he could. The Hippocratic facies disappeared, and it did not take long to prove beyond dispute that the mortality for appendicectomy in acute appendicitis, when the disease is so limited, is very little higher than that for the interval operation.

Sir George Beatson was perhaps Owen's most strenuous opponent, and he advocated a policy of delay. This is in accord with the experience of the American surgeon Ochsner, who since 1892 has practised a policy of delay in appendicular cases. Ochsner maintained that absolute rest in the Fowler position, with only water by the mouth, and the withholding of aperients and morphine, would cause a subsidence of symptoms and an improvement in physical signs in a very short time, even in cases with general peritonitis. That resolution of an inflamed appendix could be confidently expected under this treatment gave the surgeon a new opportunity to practise the art of the physician. Appendicectomy was, and still is, the goal to be aimed at, but the time for the operation is best selected by the operator. It has long been known that the majority of deaths amongst cases operated upon occur from the third to the sixth day of the disease. Therefore if cases can be successfully watched until these days are passed the mortality should be lowered. Many of the cases first seen at this period have an inflammatory mass in the right iliac fossa with little rigidity elsewhere. All such should be classified as having local peritonitis. Some others have general peritonitis. If all these cases are treated by appendicectomy at sight a high mortality must be faced. The patient has already absorbed toxins, and his resistance is an uncertain factor. The disturbance of appendicectomy may just turn the balance of toxic absorption against him before he has had time to develop antibodies and neutralize the toxins present. If all such cases are treated by the Ochsner method, the most favourable result obtainable will be resolution in some and local abscess formation in others. Experience in this country seems to show that there will still be a certain proportion in which the symptoms will progress; and because of increasing pain, rising temperature and pulse, and worsened facial aspect, the surgeon will feel impelled to operate. This aspect of treatment has recently been studied by Mr. McNeill Love [3], and two very interesting papers have been published by him based on the statistics of the London Hospital. He agrees with every other observer that cases operated upon during the first twenty-four hours show a low mortality—namely, 0.9 per cent. The same death-rate holds for all cases in which the disease is limited to the appendix.

The recognized mortality for the so-called interval operation, where inflammation is presumably in abeyance, is rather less than this—namely, 0·5 per cent. Love presents two tables which give the mortality of acute appendicitis treated by immediate operation and by postponed operation. Operation in both instances means appendectomy, and immediate operation shows a percentage of deaths as high as 5·8, whereas cases treated on delayed lines are only fatal in 3·5 per cent. These figures are important because the immediate operation cases include a high proportion with general peritonitis—one-seventh of the total, and these have a mortality at the London Hospital of 20 per cent.

Statistical proofs are often looked upon with scorn, but nevertheless a surgeon is influenced by his own statistical results, and it behoves us all to know, and to watch, our personal figures. For this reason I have prepared, with the aid of Mr. A. C. Halliwell, until recently surgical registrar at St. Thomas's Hospital, and now resident assistant surgeon, figures for the years 1919 to 1923, which may be usefully compared with those published by Mr. Love in the *British Journal of Surgery*. These latter are from the London Hospital, and refer to the years 1920 to 1923.

TABLE I.—Cases Operated upon within Twenty-four Hours of Onset.

	London	St. Thomas's
Number of cases	221	303
Recovered	219	296
Died	2	7
Mortality per cent.	0·9	2·3

TABLE II.—Cases Treated on Delayed Lines.

Inflammation Subsided :	London	St. Thomas's
Number of cases	232	143
Recovered	227	143
Died	5	0
Mortality per cent.	2·1	0·0
Unsuccessfully Delayed :		
Number of cases	109	71
Recovered	102	62
Died	7	9
Mortality per cent.	6·4	14·0
Total :		
Number of cases	341	214
Recovered	329	205
Died	12	9
Mortality per cent.	3·5	4·2

TABLE III.—Cases in which Immediate Operation was Performed.

Inflammation Limited to the Appendix :	London	St. Thomas's
Number of cases	633	348
Recovered	627	315
Died	6	3
Mortality per cent.	0·5	0·8
Local Peritonitis :		
Number of cases	467	271
Recovered	438	257
Died	29	14
Mortality per cent.	6·2	5·2
Local Abscess.		
Number of cases	347	95
Recovered	331	91
Died	16	4
Mortality per cent.	4·6	4·2
General Peritonitis :		
Number of cases	230	187
Recovered	183	132
Died	47	55
Mortality per cent.	20·5	29·4
Total :		
Number of cases	1677	901
Recovered	1579	825
Died	98	76
Mortality per cent.	5·8	8·4

TABLE IV.—*General Peritonitis Cases.*
(All treated by immediate operation.)

Day of Attack	London (230 Cases) Per cent.	St. Thomas's (187 Cases) Per cent.
First	6.6	10.6
Second	29.8	46.5
Third	35.6	21.3
Fourth	13.3	9.0
Fifth	6.6	5.8
Sixth	4.7	1.0
Seventh	1.1	3.7

With regard to Table I the higher mortality of the St. Thomas's column may perhaps be excused by the fact that the notes record a rather high proportion of what are called fulminating attacks. Anyhow the figure is higher than it ought to be for such early operation, where the presumption is that the disease is limited to the appendix itself. The last table will show that 10 per cent. of our cases of general peritonitis gave a history of only twenty-four hours, and I shall comment on this later.

Table II seems to show that St. Thomas's has gained in the first section the patients it lost in Table I, and I rather think in reading Mr. Love's paper that the operation has been undertaken unduly early, and thus the higher mortality at the London Hospital is accounted for. I think no great clinical acumen is required to recognize that a large number of cases come to hospital with more than twenty-four hours' history, when the attack is obviously subsiding, and these, if sufficient patience is exhibited, will go far to improve the statistics under this heading. They are cases which, without a twenty-four hour rule, could be dealt with at once and do no harm to the mortality table. The heading "Unsuccessfully Delayed" is more important, and here I freely admit that our high figure of 14 per cent. is attributable largely to the pernicious system of dual control.

There are one or two points of difference in the practice of the two hospitals to which I should like to draw attention, and I have to thank Mr. Love for information so far as the London Hospital is concerned. In the first place, he tells me that all appendix cases are treated in the surgical wards at that hospital; that many of the surgeons adopt the expectant line of treatment in late cases more or less as a routine. At St. Thomas's Hospital, on the other hand, very little expectant treatment is indulged in on the surgical side, and a large number of appendix cases are still admitted to medical wards. Cases there are seen in the first instance by the resident assistant physician and surgeon, and where the disease appears limited to the appendix or the patient appears to have general peritonitis, immediate operation is arranged for. Where an appendicular mass is diagnosed the case is commonly admitted on the medical side, and here expectant treatment is practised. Dual control I have no hesitation in condemning, and I am convinced that it does not serve the interest of the patient; appendicitis is a surgical disease, and the man who sees and handles the interior of the abdomen is surely the one to make the most accurate estimate of the physical signs. A study of the nine fatal cases in our series of seventy-one shows that the mortality was attributable to imperfect appreciation of the degree of infection, and not to the fact that operation was done in the dangerous period of the third to the sixth days. Three of them had general peritonitis when the delayed operation was performed, and an equal number a subphrenic abscess, while the remaining three had obstruction of the small bowel.

If we turn now to Table III we shall see that at both hospitals the mortality is weighed down by the cases of general peritonitis. We all know that if our ideals of diagnostic accuracy could be reached the number of these widespread infections would shrink very considerably. General peritonitis is rare with only one day's history, and if we cut out these cases altogether and perform immediate operation on all cases, as shown in Table III, the St. Thomas's mortality comes down to 2.9 per cent. and that of the London to 3.5. These are very respectable figures, and

seem to me to justify the gospel of immediate surgical treatment. To strengthen this view I have prepared a fourth table, which deals only with cases of general peritonitis, and shows the day of the disease when they were operated upon in accordance with the principle of immediate attack. It will be obvious that it is the first twenty-four hours which give the diagnostician his chance. It matters not whether he be general practitioner, physician, or surgeon—he must make up his mind early or the risk to the patient will be increased. It may not matter greatly who does the surgery, but it does matter when the patient first comes under medical observation, and who sees the case. Diagnosis will always be the highest art both in surgery and in medicine, and let us recognize that herein lies the chance of practising preventive surgery, which alone can bring down the mortality of this disease to a level consistent with the pride of our profession.

I think I shall find general agreement when I say that there are three main types of acute appendicitis viewed from the clinical aspect. There is one in which the disease is limited to the appendix, characterized by local pain, local tenderness, and usually hyperæsthesia of the skin above Poupart's ligament. To the second variety the old-fashioned name of "perityphlitis" might still be applied, for the infection has passed beyond the confines of the appendix, which itself is often gangrenous and usually perforated. The infected peritoneal zone is that of the right iliac fossa or the pelvis. A localized abscess, an inflammatory mass without evidence of pus, or infection ill limited by adhesions, may be in existence at the time the patient is seen. The third main class is that in which there is widespread inflammation, diffuse rigidity of the abdominal wall, very little respiratory excursion, and an anxious expression on the face of the patient. In this last type general peritonitis may be inferred.

If it is true that immediate operation in the first class carries with it a death-rate of 1, or less than 1, per cent., I think we shall all agree that we have learnt the correct line of treatment. Personally, I do not feel disposed to pay very strict attention to the alleged day of the disease. Appendicitis seldom exhibits the dramatic onset of a gastric perforation, and frequently there is a day or two of ill-health before actual pain is felt. I think there may be marked pathological changes in the appendix which cause no pain at all, and they may certainly give rise to no vomiting. If so, the patient may easily be forty-eight hours out in his length of history. If the clinical signs point to appendicitis proper, appendicectomy is the quickest route to restored health. This is in accordance with the view that one can never tell what the diseased appendix will do next.

In the case of general peritonitis I doubt whether a policy of "wait and see" has much to recommend it. When it was always thought necessary for a physician to diagnose and a surgeon to operate, some fifteen to twenty years ago, the mortality was 70 per cent. at St. Thomas's. Now it is 29 per cent., and at the London Hospital 20 per cent. These cases still bulk far too largely in our tables, for in the London Hospital cases 13 per cent. of the total treated by immediate operation had general peritonitis, and at St. Thomas's 20 per cent. In spite of this our hospital figures are better than they were fifteen years ago, and I think the credit of this should be given to the improved capacity for diagnosis on the part of the general practitioner, and also to the doctrine of immediate operation for appendicitis. The actual fall in mortality since 1910 for all types of the disease is from 8 to 5 per cent. at St. Thomas's. Another factor, which has perhaps been overlooked, is the abolition of chloroform from operations on septic patients. I say this factor, rather than the introduction of gas and oxygen anaesthesia, because I think that ether is still the anaesthetic most generally used, and when given by the open method it is most satisfactory for the surgeon, and almost anyone can administer it. It is true that cases of general peritonitis under treatment by starvation, the withholding of aperients, and the Fowler position, undergo resolution, or the infection may

settle down to a localized abscess, but at present I think we have very few data which will guide us in the selection of such cases if we meet them for the first time with generalized peritoneal involvement.

If, as Mr. Love states, delayed treatment can only be carried out satisfactorily under hospital conditions, are we not in danger of reverting to the old state of affairs? For what the hospitals do to-day the general practitioner will do to-morrow. Let it be admitted that each case presents an individual clinical problem. Is it not so with every disease? But let it be recognized by the public that immediate operation yields the best results in appendicitis and the doctor's duty will be simplified. If we change this point of view the public will get the idea that appendicitis should be treated on medical and not surgical lines, and I think there is a very real danger of the mortality continuing to rise. Public opinion is first trained by the medical profession. Later, the same public opinion tends to call the tune for medical treatment. As a proof of this I need only refer to the recent trial of a doctor who failed to take a throat swab in a case of diphtheria and objected to the use of antitoxin. He was prosecuted, not by his medical enemies, but by the Crown for manslaughter. The Crown prosecution was at the instigation of a coroner's jury. Such a power over us does public opinion acquire as the result of education by ourselves. For our peace of mind I think it was a blessing he was acquitted; but the fact that the trial took place shows how careful we must be in formulating rules for public guidance. Patients are less exiguous than before the war in demanding the immediate attention of their doctors. Perhaps they are more prone to treat their abdominal pains before summoning assistance. I hope this does not mean that the precious surgical opportunity afforded by the first twenty-four hours of an attack of appendicitis is often allowed to slip. As long as the ownership of children is vested in the parents I am afraid mothers will continue to misuse aperients, and I am heartily in agreement with Mr. Love when he writes that delayed treatment is hardly to be recommended in the case of young children. Nearly every surgeon, I think, has had bitter experience in this connexion. I would add also that at the other extreme of life, in advanced age, watching and waiting lead to greater dangers.

It has been advised by some, particularly Sherren, as long ago as 1905 [4], that when cases are seen after the first thirty-six hours of the disease—that is, when the infection is likely to have spread beyond the appendix itself—the tendency should always be towards a policy of delay. Nevertheless he admitted then, and other surgeons have admitted since, that we may regret leaving a case, but we are not likely to regret operating when there is a doubt as to the chance of temporary improvement.

Sherren based his doctrine on the state of cutaneous tenderness present. If superficial tenderness is present in the "appendix triangle," bounded below by Poupart's ligament, above by a line drawn out from the umbilicus, and to the inner side by a vertical line just to the right of the mid-line, it usually means that the appendix is distended and intact. In cases that are subsiding the area becomes smaller and smaller. Disappearance of tenderness without signs of improvement of the patient means serious mischief in the appendix. This latter is a call for immediate operation irrespective of the day of the disease. The presence of cutaneous hyperæsthesia, however, is not a proof of safety, since the infection may spread outside the appendix without leading to any loss of tension within its lumen.

The important discussion, I think, centres around the proper treatment of those cases which I have included in the second clinical type. The disease is no longer limited to the appendix, but as far as the clinical signs are capable of interpretation the name of general peritonitis is not justified. There may be an inflammatory mass, there may be a visible tumour in the right iliac fossa, there may be pus in the pelvis. The day of the disease may be anything from the third to the fourteenth. Cutaneous hyperæsthesia may be present, though usually it has disappeared. What

is the proper treatment, viewed in the light of our knowledge of the mortality statistics? Love says that if you practise the delayed operation and choose your cases aright you may expect a mortality as low as 2.1 per cent. If, however, you plan your operation in the future and are compelled to do it in the present, the mortality in unsuccessfully delayed cases will be 6.4 per cent. If you perform immediate operation on cases with local peritonitis or local abscess, the mortality you may expect is 5.52 per cent. He puts together the first two figures and gets the more favourable result of 3.5 per cent. mortality for all cases treated on delayed lines, but I suggest that this includes a large proportion of cases where the condition is obviously subsiding. I do not feel disposed to quarrel with these figures, but I would suggest that it is still good surgery to drain local abscesses rather than to trust to nature to absorb or discharge the pus.

With regard to the absorption of pus in the peritoneal cavity, I think a word of caution is necessary. It is true that it can be absorbed, and actual pus may thus disappear, and in due time render the operation of appendicectomy easy; but are we sure that the patient will not suffer in the process? A recent unpleasant experience makes me hesitate to subscribe to this as an ideal course of action.

A previously healthy man was admitted to a medical ward with what was diagnosed as an appendix abscess. His fever lasted only a few days, and in about a fortnight the mass had subsided. He convalesced satisfactorily, and was discharged from the medical ward with instructions to return on the surgical side for appendicectomy. This he did, and the operation was done without any undue difficulty, no pus being encountered. All went well until the tenth or twelfth day, when his heart suddenly began to fail and he died within twenty-four hours. All that the post-mortem examination revealed was a fatty and degenerate heart muscle.

Personally I am inclined to attribute this to the toxins absorbed from his abscess. It is one thing for the peritoneum to absorb pus; it is quite another matter for the system to absorb toxins. Relief of tension is the best method of prevention, and I believe that this unfortunate event would not have occurred if the abscess in the first instance had been surgically drained.

In doubtful cases, where the presence of pus is uncertain but where there is an inflammatory mass, there is much to be said for the operation of coeliostomy. Drainage, preferably with corrugated rubber, may with advantage be made down to the appendicular lump, and discharge, or resolution, hastened in this way. Immediate operation should not always mean immediate appendicectomy, but rather the relief of intra-peritoneal tension. If there is scope for this the general principle for the urgency of surgery remains unaltered, and I do not think that the watching and waiting policy is likely to be revived in the case of general peritonitis, except in those cases in which the condition of the patient does not appear to warrant any operation. Anything short of appendicectomy here appears to be rarely successful, and if the patient will not stand this by all means give nature a chance to localize the infection.

In conclusion I would suggest that the treatment of acute appendicitis should now be standardized on the following lines irrespective of the length of the illness:

- (1) For acute appendicitis limited to the appendix, appendicectomy without drainage.
- (2) For appendicitis with general peritonitis, appendicectomy with cleansing of the peritoneum and perhaps temporary drainage.
- (3) For appendicitis with local peritonitis, relief of tension with subsequent appendicectomy.

The last suggestion is not intended to imply immediate operation in all cases, and treatment by physiological rest and the application of heat may be as successful in the treatment of acute inflammation in the right iliac fossa as in other parts of the body.

Treatment which is based on success gradually becomes standardized by the profession and recognized by the public. Experiment gradually gives place to routine, and it is our business to see that our practices are based on sound principles. Immutability is not to be expected in human affairs, but pathological laws do not change

greatly, and to lower the mortality of a disease we must either prevent it, or fashion our cure to meet the disease when its victim will best respond to the treatment.

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Mr. R. J. McNEILL LOVE

referred to the two schools of thought with regard to the treatment of certain types of appendicitis: the adherents of one school operated upon all cases as soon as diagnosed, whereas those of the other school, in certain selected cases, gave the acute inflammatory reaction an opportunity to subside before operating.

In two articles in the *British Journal of Surgery* he had analysed a number of statistics referring to a total of 2,018 cases of acute appendicitis, of which 110 died, giving a mortality of 5.45 per cent. These cases were divisible into two types: (1) immediate operation, 1,677 cases with 98 deaths, a mortality of 5.8 per cent.; (2) expectant treatment, 228 cases, of which eight died, a mortality of 3.5 per cent. All surgeons were agreed that in the early stages, when the appendix was not perforated, operation should be performed at once. The mortality in cases operated upon within forty-eight hours of the onset was 2.2 per cent. of 718 cases. When the cases in which operation was performed between the third and fifth days were considered, it was found that the mortality (of 612 cases) was no less than 10.2 per cent. During this period natural immunity had been exhausted and acquired immunity was not yet established; operation accelerated the absorption of toxins where the patient was least able to deal with them. For such cases the risks of expectant treatment were considerably less. The risk of rupture into the peritoneal cavity of a localized abscess was reduced to a minimum by permitting only water at first and later fluids by the mouth, and withholding all aperients. Fomentations were applied and Fowler's position adopted, and, if necessary, a glycerine enema was given to relieve discomfort. In his series there was only one case in which the calamity of intraperitoneal rupture of an abscess occurred, and that followed the sudden distension of the colon by a soap-and-water enema.

Of 228 cases in which expectant treatment was adopted 151 (nearly 70 per cent.) subsided and were subsequently submitted to appendicectomy, with a mortality of 1.9 per cent. In the remaining seventy-seven cases expectant treatment had to be abandoned and operation was performed, either because the general symptoms became more marked, or because of the formation of a local abscess which increased in size. The mortality of these seventy-seven cases was 5 or 6.5 per cent., only a fraction more than the mortality of cases submitted to immediate operation, so that a brief delay, even when the symptoms did not subside, did not appreciably increase the mortality. When expectant treatment had been successfully adopted there remained the question of the best time for operation. In this series 151 cases had been operated upon one week after the temperature and pulse were normal, but this period was probably too short, as it was found necessary to drain in 40 per cent. of the cases.

Sir George Beatson recommended operation when the temperature, pulse and blood-count were normal, the latter indicating resolution of the inflammation or the presence of merely sterile pus. A third school advocated operation after a delay of three months from the time when infection had subsided. In addition to the definitely lower mortality of expectant treatment there were also the following advantages:—(1) Immediate post-operative complications, such as intestinal obstruction, fæcal fistula, &c., were diminished; (2) late sequelæ, such as incisional hernia, were less frequent; and (3) the operation itself was much simplified. Expectant treatment imposed a greater tax on the nursing staff and necessitated continuous supervision by the surgeon, and for these reasons was only feasible in hospital practice.

Mr. C. A. R. NITCH

said he considered that the problem of the treatment of cases first seen about the fourth day was an exceedingly difficult one. In such cases where there was a mass to be felt in the abdomen he usually adopted the expectant plan, but felt the

responsibility of doing so to be very heavy, since the mortality of a leaking or ruptured abscess was very high. As regards the time for appendicectomy in delayed cases, he preferred to wait for three weeks after the infection had subsided, since he had known patients to die of general peritonitis when the period was reduced to one week after the temperature and pulse were normal. A still longer delay, on the other hand, meant denser adhesions and a more difficult operation.

Mr. CECIL ROWNTREE

said he always adopted the plan of immediate operation. In cases at about the fourth or fifth day, when the appendix was difficult to find, he did not persist in the search, but merely drained, and removed the appendix about three months later, when the inflammatory mass had disappeared, leaving singularly little in the way of adhesions, so that the operation was a simple one. His own impression was that the mortality of appendicitis was definitely less now than it was ten years ago, and he suggested that the Registrar-General's figures, which showed an increase in the mortality, were accounted for by the number of inexperienced surgeons now operating, and by their persistence in removing the appendix in cases where it would more safely be left.

Mr. A. E. ROCHE

related two cases which showed the extreme difficulty of deciding which cases were suitable for expectant treatment.

In one case, first seen on the ninth day with an abscess, operation was delayed until the thirteenth day, when, the lump having daily become palpably smaller, a perforated appendix, wrapped round with omentum, was removed without a drop of pus being seen, this evidently having been absorbed. It was possible to close the abdomen completely, and the patient made an excellent recovery. In the other case, first seen on the fourth day, operation was delayed on account of some doubt as to the diagnosis. On the eleventh day, after the temperature and pulse had been normal for some days, there was a sudden drop in the temperature with a rise in the pulse, and the patient rapidly succumbed. Post mortem there was found a perforated appendix, with pus from the pouch of Douglas to the diaphragm, an appendix abscess having ruptured intraperitoneally, although the patient was under skilled supervision.

Mr. H. S. SOUTTAR

said he invariably operated within an hour of making the diagnosis, although he agreed that operation was often very difficult about the third to fifth day, and required the highest degree of surgical skill and judgment. He used a small incision in order to limit the field, and did not persist in attempting to remove the appendix when it was not easily found. On the other hand, he knew of cases in which the expectant form of treatment was followed with complete success.

One country practitioner boasted that he never called upon a surgeon for cases of appendicitis, as abscesses, when they formed, invariably burst into the rectum. At the age of 65 this practitioner himself had appendicitis, and refused to be operated upon. In due course an abscess formed which burst into the rectum, and complete recovery ensued. But there is no accounting for miracles!

Mr. H. W. CARSON

said he thought that the time factor was not the only one to be considered. The age of the patient was also important. For children he considered the safest plan was to operate at once. But in the case of adults there was a period, generally about the fourth day, when an ill-timed operation had to be concluded by draining a bleeding, granulating mass left by the surgeon, although no pus might have been seen. He thought that one week's delay after the temperature and pulse had fallen to normal, in the cases treated expectantly, was insufficient, as the same state of affairs might exist then. There was one position of the appendix which called for operation

without delay, at any stage—namely, the five o'clock position, in which the appendix was hanging over the brim of the pelvis; in this event the diagnosis was made by rectal examination, for abdominal signs might be completely absent. He considered that the important question of pre-operative treatment had not received due attention from the teachers of surgery; the adoption of the Fowler position, for example, was just as necessary before operation as after, and yet it was rarely practised as a pre-operative measure.

Mr. HERBERT J. PATERSON (President)

said he considered that in addition to the five o'clock appendix, the retrocaecal appendix should be removed at once. His usual practice was to operate at once, and he nearly always removed the appendix; he thought that he only abandoned the search for the appendix in about 2 per cent. of cases. He asked Mr. Adams how, short of operation, it was possible to tell when the inflammation had spread beyond the appendix.

Mr. McNEILL LOVE (in reply)

agreed that a week was too short a period to allow after the temperature and pulse had fallen to normal. This was proved by the fact that 40 per cent. of cases operated upon after this interval needed drainage. Expectant treatment was not so successful in children.

Mr. ADAMS (in reply)

said that of nine cases in which death followed expectant treatment, three patients died of general peritonitis, three of small bowel obstruction, and three of subphrenic abscess. In reply to the President, he said it was impossible to tell accurately what was happening inside the abdomen except at operation. If expectant treatment were generally adopted by hospitals there was a danger that the practice would extend to those treated elsewhere, whereas expectant treatment should only be selected when the case was under the constant supervision of the surgeon who would operate if, and when, necessary.

Section of Surgery.
SUB-SECTION OF PROCTOLOGY.

President—Mr. F. SWINFORD EDWARDS, F.R.C.S.

**DISCUSSION ON THE VALUE OF X-RAYS IN THE
DIAGNOSIS OF DISEASES OF THE COLON.**

Dr. S. GILBERT SCOTT.

THE subject for discussion which I have been asked to open this evening is of equal importance to surgeons and radiologists. Although the radiological examination of the colon has been in practice for many years, it has not been developed, at any rate in this country, to the same extent as the opaque-meal examination of the stomach. What is the reason for this?

Speaking generally, the stunted development of the radiological examination of the colon is probably explained by the fact that full diagnostic value is not being



FIG. 1.—Colon after opaque meal,
uneven distribution.



FIG. 2.—Normal colon after opaque
enema.

obtained by most surgeons; consequently, as they and the physician get little or no assistance from any examination, they cease to supply material to the radiologist. Surgeons can help to increase the value of the examination in two ways; first, by supplying the material, and secondly, by keeping us informed of the operative findings in the cases examined. They can, at least, keep us informed of our failures. This is important. In hospital it is comparatively simple to arrange for the operative findings to reach the radiological department. In private practice, however, it is more difficult and entails considerable perseverance on the part of the radiologist.

No one need be disheartened or ashamed of their diagnostic errors, provided

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they take the trouble to investigate the causes. An error tracked to its lair is worth many a correct diagnosis, as it is less likely to be repeated.

Undoubtedly too much is expected of radiologists. If they overlook a lesion or make a diagnostic error the whole examination is discredited. But it should be remembered that we are working under difficulties. The science of radiology is young; we have no classics and only a few books of reference to assist us, and it must be realized also that patients' "interiors" vary as much as their exteriors. These are but a few of the difficulties we have to contend with.

It must be admitted that some of our colleagues display little enthusiasm for the opaque-enema examination. They declare it to be "a messy job." This is quite wrong. A mess, apart from accidents, is a sign of faulty technique and can easily be avoided. At the London Hospital, practically every abdominal case now undergoes an *opaque-meal* examination, simply because it has proved to be of very considerable diagnostic value in gastric cases. The large amount of material thus supplied enables the radiologist to discover the error of his ways, so that he has a better chance of improving his results.



FIG. 3.—Opaque enema, effect of feces in colon.



FIG. 4.—Appendix visualized twenty-four hours after opaque enema.

Now the value of radiology, broadly speaking, is dependent on two factors: *correct technique and correct interpretation*. Let us briefly discuss these in their relation to the examination of the colon.

(1) *Technique*.—As you know, the contents of the intestinal tract can be rendered opaque to the rays by the use of some innocuous substance such as barium. This may be given by the mouth as a meal or injected *per rectum* as an enema. The opaque enema is as valuable for the examination of the colon as the opaque meal is for the examination of the stomach. It is now many years since I gave up employing the opaque meal as a routine for the examination of the large bowel in detail. The opaque substance given by the mouth became so unevenly distributed by the time it reached the colon, haustration was so definite and unsymmetrical, and the examination at the same time was so tedious, that at the conclusion the question as to whether there was an organic lesion present often remained unanswered. This method had to be discarded in favour of the enema, to which, therefore, these remarks will be

limited. The whole of the large intestine is visualized by the opaque enema (figs. 1, 2). The opaque fluid is seen passing rapidly through the various twists and turns—filling defects and strictures are easily noted—the gut can be manipulated under the screen, and the examination is completed in a very short time without discomfort to the patient. In fact, the whole examination can be completed in under thirty minutes by one skilled in the method. Full technical details of the method will be found recorded in the *British Medical Journal*, January 24, 1925. It only remains to emphasize the importance of careful preparation of the patient; the presence of faeces may lead to serious errors of diagnosis, also the use of a suitable viscid fluid for the injection—such as will readily creep into cracks and crevices, if small lesions are not to be overlooked.

(2) *Interpretation.*—Let us now consider—taking it for granted that the technique is correct—the most difficult and the most important part of the examination, namely, correct interpretation of what is visualized. Lesions of the colon are based on direct evidence of obstruction or on abnormal changes in the lumen. Evidence



FIG. 5.—Diverticula twenty-four hours after opaque enema.

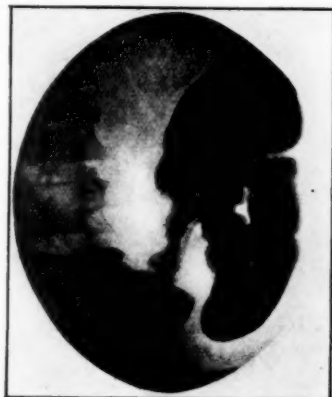


FIG. 6.—New growth of sigmoid.

of a lesion is there, but the value of the examination will always be dependent on the astuteness and experience of the examiner. This, consequently, will always be a variable quantity.

There are two classes of errors of interpretation. A lesion may pass undetected—error of *negation*; or a lesion is diagnosed but not found at operation—error of *affirmation*. Of the two, the error of affirmation is the more serious, as an unnecessary operation may result. There are many fallacies in this examination. Faeces in the colon (fig. 3), air-locks, pressure from without, spasms, a partially-filled gut, all give filling defects which may easily be diagnosed as organic in origin.

While there is but little difficulty in diagnosing the straightfaced stricture, such as is caused by a new growth or the multiple sacculations seen in colonic diverticulation, the atypical and small lesions are the ones that give rise to considerable difficulty in diagnosis, in fact, small lesions may be missed altogether. Again, a lesion may be detected, but to differentiate between, say, tubercle and new growth,

is often impossible. Nevertheless, experience obtained by handling a large amount of material will reduce diagnostic error.

A few examples will now be given illustrating the practical value of the method.

Take, for instance, a tumour that can be felt in the abdomen. The symptoms are vague—the surgeon wants to know whether the mass is of colonic origin. The enema is watched passing into the rectum, it reaches the neighbourhood of the tumour—a slight hesitation in the flow is noted, but not amounting to obstruction. The opaque stream flows *round* the mass, which is located by the hand on the abdomen. The lumen of the gut is seen to be flattened by pressure. Displace the tumour to one side and the lumen fills fully. The tumour is evidently not of colonic origin. In one such case the pressure on the colon was due to a hypernephroma in a movable kidney. This is valuable information to the surgeon. In another case the flow stops short on reaching the tumour. The obstruction is straightfaced, not tapering; it is not quite complete, for an increase in colonic pressure, cautiously applied, will demonstrate a tortuous channel tracking through a narrowed section of

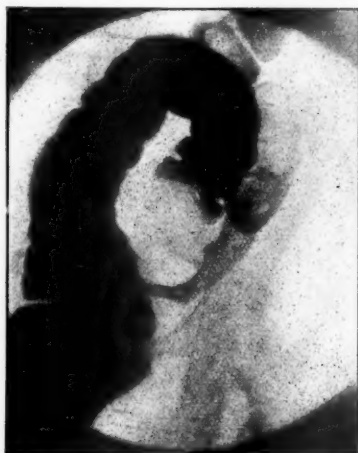


FIG. 7.—New growth of sigmoid.

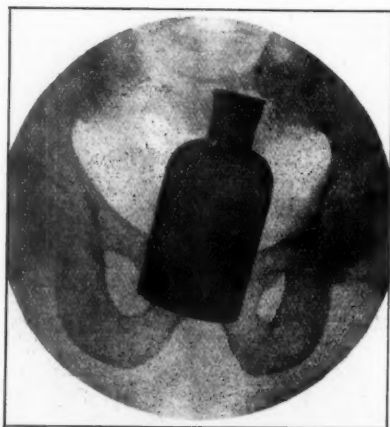


FIG. 8.—Bottle in rectum (6 inches).

the bowel. Once passed, the lumen fills fully beyond. An irregular stricture is now easily detected, corresponding to the tumour. Roll the mass under the fingers—the narrowed section moves with it. Obviously the tumour is of the colon—the type of stricture being that of a new growth. Again, the colon fills easily, but in the cæcal region an irregular filling defect is seen, this time localized to one side, extending along some three inches. This irregularity is permanent and a thickening may be felt per abdomen corresponding to it. This has not quite the appearance of a new growth just described, but is that of tuberculous disease—a somewhat rare condition, giving rise to considerable difficulty in differential diagnosis.

With regard to colonic diverticulation, this is by no means an uncommon condition after the age of fifty. If these sacculations are small and few in number, they may quite easily be overlooked unless the patient is examined *twenty-four hours after the enema* has been given (fig. 5). They are manifested as bud-like projections when seen at the actual time of the injection, or as round button-like shadows, often in large numbers, when the patient is examined again in twenty-four hours; the rest of the bowel will be

empty. They may remain filled two weeks or more after examination, in spite of lavages and injections. If inflammation is present in one of these saccules, the opaque fluid will not find its way into it, as the opening into the gut will be closed, but the presence of other pockets will indicate the probable nature of the mass felt. It is interesting to note that in a number of cases the opaque enema passes through the ileo-cæcal valve, also that the appendix is frequently visualized at the twenty-four hour examination, occasionally at the time of the injection.

At the London Hospital last year (1924) 167 colonic cases were investigated. In a large percentage the radiological findings were negative and only sixteen were operated upon. In one case a small carcinoma of the cæcum was missed. In the remaining fifteen cases the radiological findings were confirmed. In two cases an abdominal tumour was correctly located as unconnected with the gut.

In a series of colonic cases examined by Carman, of the Mayo Clinic, in which operation was performed, a diagnosis of a lesion was made in 90·87 per cent. of the cases; in 9·13 per cent. the presence of a lesion was not demonstrated. While diagnostic error was in part responsible for the undetected lesions, Carman is convinced that certain lesions of the colon are not revealed by radiology. With this I agree. In this country, at the same time, the most frequent error is that of affirmation and not of negation; in other words, there is a tendency to conclude that any deviation from the normal is of a pathological nature.

The diagnostic value has greatly increased in England during the last few years, but it is still below the standard attained in other countries. A serious effort must be made to rectify this.

The reasons may be summarized as follows: (1) Too much reliance on radiographs; (2) failure to detect some lesions unless deep palpation is resorted to during the screen examination; (3) faulty technique; (4) unacquaintance with the normal colonic variations; (5) want of collaboration between surgeons and radiologists; (6) failure to keep in touch with the operative findings.

Let us at least hope that the outcome of this meeting will be a closer collaboration between surgeons and radiologists.

Mr. HAMILTON DRUMMOND (Newcastle-on-Tyne)

said that his interest in the method under consideration, as an aid to diagnosis, dated back a good many years. In 1913 he read a short paper before the members of the Sub-section "On the use of X-ray Enemata in the Diagnosis of Stricture of the Large Bowel," in which he feared he made claims for the procedure as an aid to diagnosis that, in the light of further experience, had proved to be too optimistic, for he now felt that he had over-estimated the value of the use of X-rays in that connexion. Yet, as he hoped to show, he still attached great importance to radiology in the diagnosis of diseases of the colon, though riper experience had shown that the path of those who trusted to X-rays in diagnosis was beset by many pitfalls; and this observation did not apply alone to the recognition of the group of diseases they were then considering. In addition, there could be no doubt that very considerable experience was necessary when attempting to estimate the value of X-ray findings.

He said he did not claim to be an X-ray specialist in any sense, but he believed the best, indeed the only way to palpate the colon with entire satisfaction was to employ the fingers aided by sight; this secured the concentration of both senses.

At the outset, he urged closer co-operation between the clinician and radiologist in cases submitted to X-ray examination, and said he thought the surgeon should either furnish the radiologist with a history of the case or, better still, meet him in his department during the examination.

The question arose, at what stage should a case of disease of the large intestine be submitted for X-ray examination? At the present time many patients were sent

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to the X-ray department before any other examination had been made; and this occurred both in hospital cases and in cases referred to radiologists by practitioners. He considered that no colon case should be subjected to X-ray examination before a very careful clinical history had been taken, and a thorough general examination of the patient made, including a sigmoidoscopic examination. He recalled a case in which, on account of colon symptoms, diarrhoea, pain and discharge of blood, the patient had been referred to a radiologist for a barium enema. The opinion given by him (the radiologist) was that the patient was suffering from colitis, and that there was no growth, as the salt travelled to the caecum without any stoppage, whereas on digital examination an advanced palpable carcinoma, high up in the rectum, was found.

He thought it was unwise to submit patients suffering from acute intestinal obstruction to X-ray examination before relief had been obtained by operation or otherwise; little was gained and precious time was lost. Again, were they justified in basing the diagnosis of malignant disease of the colon upon the results of X-ray examination alone, for, in practice, was it not a fact that the history and symptoms usually furnished information of still greater diagnostic value? though, of course, X-rays might assist in locating the site of the disease.

He made further reference to the fact that barium administered in the form of an enema in cases of high rectal growths, and growths of the colon—more especially on the left side—might pass through the growth and reach the caecum, even in the presence of considerable stricture. He cited the case of a patient suffering from cancer of the ascending colon, with marked stricture, in which a barium meal passed through the colon beyond the growth without delay, and left no barium visible on the proximal side of the growth. He had known the same thing occur in a very tight stricture in the ileum, the result of tuberculous disease, when compensatory hypertrophy of the bowel above the stricture was satisfactory.

In illustration of the danger of relying upon the result of X-ray examination he related the following case:—

A man, aged 40, had several severe attacks of epigastric pain, with vomiting. The attacks came on suddenly and he complained of a definite, tender spot in the right side, just below the costal margin. As an aid to diagnosis, a barium meal was given. A few days later, in order to rid the patient of the barium, castor oil and several enemata were given, and a large portion of the barium salt remained in the hepatic flexure in the region of the tender area. It was thought from this that he was suffering from a growth in the region of the ascending colon, and that the barium was arrested on the proximal side of the stricture. In opposition to the radiologist's findings, a clinical diagnosis of gall-stones had been made, and was verified by operation, when an acutely inflamed, thick-walled gall-bladder was found, with a large calculus impacted in its neck. It was possible that the colon had become adherent to the inflamed gall-bladder and suffered from local ileus.

The barium enema was undoubtedly the best method of seeing the colon and demonstrating its relation to any tumour which might be present in the abdomen. He pointed out that the hepatic and splenic flexures of the colon were particularly difficult points at which to locate a growth, because at these positions the colon shadow overlapped, and the outline of the bowel became consequently obscured; it was in these two positions that barium was apt to be arrested, it might be temporarily or for a considerable time.

He had found the barium enema most useful after caecostomy, performed as an emergency operation on a patient admitted to hospital suffering from acute intestinal obstruction, in locating the position of the growth, for the solution remained distal to it. It was useful at times to introduce the barium solution through the caecostomy opening, when the shadow would be found to be arrested at the proximal side of the obstruction. The enema should be repeated later to verify the observation.

He had no doubt it would be admitted that X-rays were of great value in the

diagnosis of such conditions as the pelvic cæcum, V-shaped transverse colon and various kinks in the lower ileum and pelvic colon, though he was of opinion that operations for these conditions would pass into oblivion, as had already happened in the case of kidney fixation and fixation of other parts of the gastro-intestinal tract.

Dr. BERTRAM SHIRES

said that for many years he had carried out the two methods of examination in colon cases, and in the majority the enema was of the greatest value. Unless, however, the radiologist and surgeon worked hand in hand there were many traps into which they were liable to fall. In 30 per cent. of cases in which the barium enema was given there was a filling defect in the pelvic colon in the early stages. In one case in which there was a filling defect in the pelvic colon a study of the clinical aspect and the skiagram led to the diagnosis of carcinoma being made, but abdominal section revealed nothing more than a slight spasm at the lower end of the descending colon. One case appeared to be diverticulitis, but at the operation it was found to be generalized tuberculosis of the abdomen, with a mass in the pelvic colon.

Dr. ARTHUR F. HURST.

In estimating the value of radiology in the diagnosis of organic diseases of the colon, it is necessary to compare it with the other available methods of confirming, or otherwise, suspicions aroused by a consideration of the patient's history. Apart from the ordinary physical examination of the abdomen and digital examination of the rectum, the only direct means of investigating intestinal diseases are the X-rays, investigation of the stools, and sigmoidoscopy.

For the purpose of this communication my colleague, Dr. J. F. Venables, has analysed all the cases of the kind which have come under our observation at New Lodge Clinic during the last four years. In each an X-ray examination has been carried out by Dr. P. J. Briggs, the stools have been investigated by Dr. J. H. Ryffel or Dr. Venables, and I have inspected the rectum and pelvic colon with the aid of the sigmoidoscope. The relative value of these different methods can thus be estimated, but it is at once clear that all are valuable and should be systematically used, as it is generally impossible to tell beforehand which will give the most important information in a given case. In many the final diagnosis is only reached by a consideration of the symptoms and the results of the physical examination in the light of the information obtained, whether of a positive or negative character, with the X-rays, the investigation of the stools, and sigmoidoscopy.

This series of cases includes thirteen of carcinoma of the colon, excluding growths of the rectum which could be felt on digital examination, five localized adhesions of the colon, five cases of diverticulitis, three strictures following ulcerative colitis, one case of tuberculosis of the cæcum, and one of tuberculosis of the last three inches of the ileum.

(1) *The X-rays.*—(a) An X-ray examination after an opaque meal never reveals an obstruction of the colon which cannot be recognized with an opaque enema, but it is useful in the diagnosis of obstruction of the small intestines, especially near the end of the ileum. In only two out of thirteen cases of cancer of the colon was anything abnormal seen after an opaque meal.

(b) As I first demonstrated in April, 1908, an opaque enema shows the presence of obstruction at a comparatively early stage, often long before localizing symptoms develop. But a negative result does not exclude a growth of the colon; in one of the eleven cases in which an opaque enema was given, and in three earlier cases, a growth was found, though an opaque enema had failed to reveal the slightest abnormality.

An opaque enema is the only certain clinical means of recognizing diverticula of the colon. This is of great importance, as it may be impossible, even during an exploratory operation, to distinguish between a growth and an inflammatory mass secondary to diverticulitis. In one case in which we had found that symptoms of obstruction were due to diverticulitis, a recurrence of symptoms two years after the mass had been removed was shown by an opaque enema to be due to carcinoma developing in connexion with a diverticulum of the iliac colon, which had apparently escaped observation at the original observation.

Until recently I believed that ulcerative colitis never led to stricture on healing. I have, however, now seen four cases out of a series of seventeen, in which delayed recovery was due to narrowing of the colon, which was discovered with the X-rays; in two of these the whole pelvic colon was shortened and narrowed, but complete recovery ultimately occurred without operation. In the third case a long stricture was found in the transverse colon and a short one in the descending colon, and in the fourth a long stricture of the transverse colon and a kink of the pelvic colon; in neither were there present any symptoms pointing to obstruction. But the X-rays showed the necessity of short-circuiting operations, without which recovery would have been impossible. Delayed recovery in cases of ulcerative colitis, whether an appendicostomy has been performed or not, is thus an additional indication for X-ray examination.

(2) *The Stools*.—Microscopical examination may show the presence of blood-corpuseles and pus in stools which look normal to the naked eye. Much more frequently blood can only be recognized by chemical means and by the discovery of hæmatoporphyrin or acid hæmatin with the spectroscope. In four out of twelve cases of growth of the colon the blood was obvious; in all of the others occult blood was found in every stool. For some unknown reason it appears to be present with equal constancy when obstruction is caused by adhesions; thus, blood was manifestly present in one and occult blood in the remaining four of our cases of this kind; occult blood was also found in the cases of ileal and cæcal tuberculosis. In three cases suspicious symptoms and the presence of occult blood induced us to advise operation, though the X-rays had shown nothing abnormal; in two of these the symptoms were referred to the stomach and achlorhydria was present; and we diagnosed carcinoma of the stomach, so that an opaque enema, which might have led to a correct diagnosis before operation, was not given.

(3) *The Sigmoidoscope*.—The sigmoidoscope is so invaluable in diagnosis that every practitioner and physician should learn to use it himself in cases of supposed colitis or unexplained disturbance in the functions of the bowels. With its aid a growth of the upper part of the rectum or lower part of the pelvic colon can be recognized before it can be felt—often before a barium enema shows any definite deformity. By means of the sigmoidoscope I have twice been able to diagnose early malignant degeneration of a polypus before the wall of the bowel itself had become involved.

I hope that one result of to-day's discussion may be to induce surgeons to avail themselves more frequently of the assistance given by a thorough investigation before embarking on an exploratory laparotomy. I have had the disappointment of learning that a patient, in whom we had diagnosed a growth of the splenic flexure, had had his appendix removed by a distinguished surgeon, whom he consulted without telling his own doctor, to whom we had sent our report; eighteen months later he was operated upon a second time by the same surgeon for acute obstruction caused by an inoperable growth of the splenic flexure.

Dr. ULYSSES WILLIAMS

agreed that the barium meal was practically useless to show the position of malignant disease of the colon, but he found that the opaque enema was very useful. For revealing the appendix the meal should be made with buttermilk. He thought the operation

of colopexy should be allowed to fall into oblivion. It did not matter much where the colon was, as its position had practically no influence on the question of stasis. With regard to the normal level of the hepatic flexure, most books stated that it was at the tip of the eleventh costal cartilage, but in over 75 per cent. of normal healthy young adults he had found it to be, with the persons standing up, just at the level of the iliac crest.

Mr. J. P. LOCKHART-MUMMERY

said that the fact had to be borne in mind that a great improvement had taken place in the diagnosis of conditions of the colon during the last twelve years. Formerly tumours were diagnosed only when they were large and palpable, or when there was acute obstruction. Surgeons had recognized that unless some marked improvement on that state of affairs was forthcoming there were not likely to be satisfactory results in colon carcinoma. The real problem was to diagnose carcinoma when there was no obstruction and no palpable tumour. A great step had been taken in this direction. Of the various methods available in diagnosis he personally attached much significance to the sigmoidoscope, as did Dr. Hurst. If a patient with growth in the colon were examined with the sigmoidoscope when he had not been prepared in any way, there would be found, in the majority of cases, slight traces of blood up in the sigmoid, and it might be fairly certain that one had to deal with a growth high up in the colon (it was taken for granted, of course, that such a condition as ulcerative colitis was excluded). He thought that X-rays had helped most markedly in regard to the position of the growth, and that great improvements had still to take place in the diagnosis of position by X-rays. The radiographic experts at first were rather too enthusiastic about their method, and they made much too elaborate diagnoses from the X-ray findings. A more reasonable view was now taken. He thought that negative X-ray evidence was useless. It was not possible to declare that there was a normal colon simply because the X-rays showed nothing abnormal. X-ray evidence could only be taken as confirmatory evidence. One method of avoiding error in X-ray diagnosis of the colon was by repeated examinations. At least two or three examinations with the barium meal should be made. Perhaps the greatest value of X-rays was seen in the case of diverticulitis; X-rays furnished a method of distinguishing this from other conditions of the colon, and a proper X-ray examination would confirm or exclude diverticulitis.

Dr. W. H. COLDWELL

said he considered that there was still much to be said in favour of the barium meal as a diagnostic aid in conjunction with the opaque enema in colon conditions, though the enema was the more valuable. He had a word to say about the inefficiency of some lavages before making an X-ray examination, and the errors to which the shadow of faeces left behind might give rise. For the detection of diverticulitis, he thought the opaque meal was as useful as the enema, and most patients preferred the meal, owing to its greater convenience.

Dr. S. C. SHANKS

speaking of the barium enema method, pointed out that there might be certain "silent areas." The first of these was in the sigmoid, where the shadow of one loop might be superimposed on that of another, and thus obscure the signs of a pathological lesion. Another occasional "silent area" was that of the splenic flexure. Both of these areas were commonly inaccessible to radiosopic palpation. Again, in the middle of the transverse colon there was often an area which, in the supine position, the enema did not fill. This area, where it looped over the ridge formed by the structures on the front of the spinal column, was the highest portion of the transverse colon, and the enema tended to gravitate to the more dependent portions on either side. He

had seen such cases in which the skiagraphic picture resembled that of a carcinoma. The barium meal method was of value in investigating cases of chronic constipation. For example, in cases of dyschezia the large mass in the rectum settled the question.

Regarding the question of diverticulitis, in 196 examinations he had made by barium enemata he had found 14 definite cases of this condition (7 per cent.) and 5 doubtful ones. On separating hospital from private cases he found that in the former, out of 150 examinations there were 3 cases of diverticulitis (2 per cent.), while in 46 examinations of private patients there were 11 of diverticulitis (24 per cent.). That seemed to suggest that well-to-do people were more prone to this condition than was the hospital type of patient.

MR. ASLETT BALDWIN

described a case in which a man said he felt some inconvenience in the left side of the abdomen and had some trouble with defæcation. Examination with the sigmoidoscope did not disclose blood or mucus, and the X-ray report was that there was no filling defect nor obstruction. The man was put upon paraffin, with hopes for the best. Six weeks later, the patient said he felt certain there was something abnormal present. He (Mr. Baldwin) thought he could feel a small lump, and could feel some distension of the large intestine. He operated, and found a carcinoma in that situation. Another case was that of a man, aged about 72, who had constipation for a week and had been treated with enemas. When seen, he complained of pain in the upper part of the abdomen on the left side, which was tender. A large faecal mass could be felt on the right side of the abdomen. Washing out was done with ox-gall for a week, and this brought away flakes of mucus the size of a shilling and blood-clots the size of a threepenny piece, also much faeces. The sigmoidoscope showed streaks of blood. X-ray examination showed the transverse colon down in the pelvis, forming a very acute kink on the left side, and another on the right. There was no sign of any filling defect. The man was told to take paraffin and wear a Curtis support and though that was fourteen years ago he has since remained in good health.

Section of Surgery.

President—Mr. HERBERT J. PATERSON, C.B.E., F.R.C.S.

A Consideration of Gastric and Duodenal Ulcer:

An Address by CHARLES H. MAYO, M.D.

(Rochester, U.S.A.).

YEARS ago contractions of the pylorus through inflammatory conditions surrounding ulcers caused obstructions, starvation and slow death. The patients were ill such a long time that the obstructions were probably not due to cancer. It was before the era of aseptic surgery that the surgical relief of these cases was first put into practice. Billroth carried out resection of the obstructed area at the pylorus, then a partial closure of the end of the stomach, and union of the duodenum to the small opening left. The contemporary workers associated with him in this surgical practice were notably Hartmann in France, Mikulicz in Austria, and Kocher in Switzerland. The mortality was about 70 per cent., but the results were excellent in the successful cases. When we remember, however, that this work was carried on in the pre-aseptic period, we can only reflect on the strength of character exhibited by these great men who did so much to develop surgery in the world. They went deliberately ahead to conquer disease. Nicolodina wrote the first description of gastro-enterostomy—not that he did it; he advised it.

The high mortality stopped the work of Billroth's No. 1 operation, because those who practised it did not make any unusual effort at protection of the suture line, and trouble occurred at the fatal suture angle, where three lines of suture came together; and you know that the duodenum is an unsatisfactory portion of the intestine to suture. If there is leakage, there is a most distressing and destructive digestion in those areas.

We next have the Billroth No. 2 operation, in which Billroth closed the end of the stomach, and did a posterior gastro-enterostomy, as suggested by Nicolodina. Kocher made a separate opening posteriorly, drew over the end of the duodenum and sutured it in. In some cases this was a little short, and the operation was not taken up to any great extent. I am discussing the gradual advance of surgery, because in all the developments of our work it has been, first relief, and then working backward on aetiology; then prevention, and finally new methods.

A different operation was devised by Loreta and Hahn, who forced the stomach wall into and through the pylorus. They sought to divulse the pylorus so as to permit the stomach to empty. But the contracture was worse than ever, and the operation had no following.

Then we have the Rammstedt operation, that of dividing the pyloric muscle without opening the mucous membrane. That has proved sound and is very successful in the treatment of children with congenital contractions of the pylorus. In order to prevent a feared reunion of muscle, Straus supplemented this procedure by suturing in some fat, but this addition is not necessary in the case of Rammstedt's operation.

The next procedure was established by Dr. John Finney, of Baltimore, who, in 1892, started a pyloric operation by suturing, at the pylorus, the upper duodenum to the greater curvature of the stomach, and then, with a little horseshoe incision, forcing down the suture line and closing the outer edge of the incision over it. Finney has, in the last three years, developed a new operation—the Haberer procedure—that is,

resection and division at the pylorus, a new opening in the anterior wall of the duodenum, and suturing the pyloric end of the stomach into this new opening. It was done a number of times during the period when surgeons were deciding that the excision of the ulcer was the essential point; with a large ulcer of the anterior wall of the duodenum excised, it was found easier to divide the pylorus, close in a few cases the end of the duodenum, and unite the pyloric end in the opening. The operation has been described by a number of people for emergency, but I do not think that it will be of any very wide application.

In the early period, following Finney's work, there were put into practice the methods of using mechanical adjuncts, namely, the Murphy procedures of union, such as the Murphy button and the Mayo Robson bone bobbin, over which it was possible to suture if it was thought that the opening would not be patent during the first few days following the operation, or that the size of the opening would not be known after it had been made. Many used the Robson bobbin for a few operations and then decided that they did not require such an apparatus to work with.

Another method consisted in the adoption of the bone-plate, but this was put forward by Senn, of Chicago, for use in an operation which had been planned by Connell. For a certain period these decalcified ox-bone plates were used throughout the world, but were superseded by the gradually developing methods of suture; so that now practically all mechanical adjuncts are discarded, excepting the occasional use of the Murphy button for special cases.

The Polya operation came next: in this operation large areas of the distal part of the stomach are excised, the duodenum being closed and the open end of the stomach being drawn through the colonic mesentery, like a posterior gastro-enterostomy, and attached to the jejunum. It is a method that has found a wide sphere of usefulness throughout the surgical world of to-day. And yet there are times when a very short mesentery of the transverse colon is found, and there is no room through which to make a gastro-enterostomy or to draw the end of the stomach. In such a case it is necessary to do the anterior operation of carrying the jejunum over the loop of the transverse colon, and attaching it to the divided end of the stomach (Polya-Balfour operation).

Those operations were mostly devised for the treatment of cancer by excision of large areas of the stomach, leaving a small upper part only. It is remarkable how small a portion of stomach left will become distended and grow larger with use, showing more capacity than the surgeon might have thought possible.

Dr. Balfour advised this change in the Polya operation first for the cancer cases. In a case of cancer, the anterior Polya-Balfour operation is a better procedure than the original operation of Polya.

Gastro-enterostomy was from the first successful; it went through various periods of development like other surgical procedures; it was safe in the hands of many, and the extent of its application became wider and wider as time went on. There has been, indeed, an unnecessary application of it for the relief of symptoms, without care being taken to observe whether there was an ulcer present in the particular patient or not. Many people who have had symptoms of ulcer have undergone gastro-enterostomy when something else was causing reflex symptoms. The origin of the trouble may have been the gall-bladder, for instance, or the appendix. The stomach is the great organ of complaint in the abdomen, and is subject to many irritants, from the brain to the lower abdomen, in a reflex way.

We have, moreover, failures from bad technique, such as occasionally occur in the excitement of an operation; possibly the patient is under serious stress from anæsthetic troubles. Three times I have found patients so much worse after a gastro-enterostomy had been done in another clinic, that I re-operated on them; in one patient the

anastomosis to the stomach was made 9 in. from the end of the ileum, in another case I found the distance to be 10 in., in another case 18 in. Here were three cases with an entire short-circuiting of all the small intestine, with only a little food going out through the pylorus, the greater part passing through the gastro-enterostomy opening. I think the accident was due to excitement on the part of the unaccustomed operator, because in two of those cases I could not find any sign of ulcer. They were patients who have that habit of complaint which, with an energetic surgeon, will lead to operation on something!

There are troubles which are due to kinks, twists and adhesions. There was: the 7-in. anterior gastro-jejunosomy loop, which gave trouble because of the accumulation of food in it; a twist of the gut at the point of attachment, because when we passed from anterior to posterior gastro-enterostomy we kept to the same twist of the gut as when we brought the jejunum over the transverse colon. Then a too short jejunal loop tends to kink. There are only a few people who have a jejunum which goes straight down from its origin; the first loop is nearly always the splenic loop.

The next trouble, which has been admirably described this morning by Mr. Walton, is the gastro-jejunal ulcer.¹ Four years after operation we found the silk sutures we had used for gastro-enterostomy in festoons, and always hanging back into the stomach, instead of falling into the loop of bowel, as we should expect from the passage of food. Not all of these cases developed ulcer, and the sutures eventually separated. But silk sutures continued to be found, not only in cases at other clinics which did not recover after operation, but also in our own patients, some of whom developed gastro-jejunal ulcer. The point was made this morning that patients who do well from the start rarely have trouble later; but patients who within the first few weeks or months do not do as well as one would like, and after they go home have occasional suffering, and need to be watchful about what they eat, are those who become afflicted with this kind of ulcer.

The apprehension that has arisen during the last two or three years with regard to the defects of gastro-enterostomy has led, on the Continent, to the development of resection of the stomach for duodenal as well as for gastric ulcer. With only a little ulcer of the duodenum present, and with only occasional symptoms, coming on in the spring and autumn, some surgeons have cut away two-thirds of the stomach merely to prevent the onset of those symptoms. Personally we, as surgeons, would not have that done to our stomach in order to get rid of something which was below the stomach. It is so rare for such troubles to occur, that they had better be allowed to happen rather than for the patients to undergo an operation of such magnitude, with a higher mortality. It is too much of an operation primarily, except for ulcers in the stomach, and for a few special ulcers of the duodenum.

With regard to the danger that occasionally a large ulcer develops carcinoma on one marginal area, as ulcers do everywhere when they are irritated chronically, this rarely happens, however, in the case of small ulcers. Usually the growths form only in the areas of highest acidity. One-third of cancers in men occur in the stomach; very few are to be found in the course of the small gut, in comparison with the number in the large gut.

Balfour has surveyed the work done at the Clinic, and of 1,000 consecutive cases of ulcer of stomach and duodenum operated upon more than ten years ago, he found that 90 per cent. had been successful, surgically. Defects of all kinds complicated the remaining 10 per cent. of the cases, e.g., recurring hæmorrhage, partial obstruction, and gastro-jejunal ulcers. When we were using silk for the sutures, about 4 per cent. of the cases developed gastro-jejunal ulcer. Now, using catgut, the figure is 2 per cent. Gastro-enterostomy in the majority of cases, unless in the presence of some unusual condition, is a good operation.

¹ See *Lancet*, 1925, i, p. 1215.

Now to come to a discussion of ulcer itself. We speak of ulcer as being due to the action of acids and pepsin, hence they are called peptic ulcers. It is difficult sometimes to explain the occurrence of ulcer in an acid-free stomach. We used to be content with a single stomach test at the end of an hour, but a patient who has a non-acid stomach must be tested a second, even a third time, because in a few the acid develops strongly at the end of one hour and twenty minutes, lasts twenty minutes, then disappears. In people who have had no acid in the stomach for a long time you can bring back acid temporarily by injecting histamine. That is not a treatment, but it proves that something has gone wrong with some hormone.

People with achylia are subject to troubles of the nervous system, for there occur trophic ulcers on the toe, and ulcers on the heel. These people are not likely to be greatly relieved, except by exploration of the condition, with medical care. They have no gastric digestion, they have no acid or pepsin, but their gastric ulcers do not perforate. I had a patient—a case of achylia—last January who had undergone two gastro-enterostomies. At the first operation a loop was attached to the stomach, and there was seen to be nothing wrong with the emptying of the stomach. A second vigorous surgeon made another loop—gastro-enterostomy. So then it was open for the food to select any one of three passages! Patients with achylia suffer from headache and other troublesome symptoms, because of the alkaline condition which has caused the irritation of the nervous system.

Ulcers can be produced by large doses of drugs, and they can be caused by bacteria. You can cultivate bacteria from an ulcer and inject quantities of the culture into animals. A chill and a reaction follow the injection; only in 75 per cent. of the animals will an ulcer develop, but if it does it will not last. But if you can afterwards create in the animals a chronic focus of infection with the same bacteria, like human tooth infection, which will keep feeding out bacteria, the ulcers will continue in many animals.

Then there are the mechanically or chemically produced ulcers. In some animals duodenal ulcer can be produced if you separate and re-attach the common and the pancreatic ducts so that they deliver down into the lower ileum; or if you divide the lower duodenum and unite it to the ileum, thus short-circuiting the jejunum, a large percentage of duodenal ulcers will occur. Gastro-jejunal ulcers are due to the failure of the material which is formed in the pancreas, liver, and duodenum to pass through the upper part of the intestine.

And there is another possible cause, which is most important. One of the most brilliant men, in regard to the promise for his future, died here in London last autumn, Dr. John Hunter. I think he was one of the greatest young anatomists in the world. The parasympathetic and the sympathetic nerves are involved in many diseases, and Dr. John Hunter, through the influence of Royle, made investigations concerning the proportion of muscles in the body which had the power only of fixation. Sometimes the circular intestinal muscles go into spasm, and then they are associated with these disturbances in the intestine and stomach (Devine). They have to do with diverticula in the large and small bowel. The cardio-spasm, which lasts for years, is cured by over-stretching of these particular muscles. It is, perhaps, every second or third fibre which goes into spasm. We see such a condition in the pylorus of people who exhibit an idiosyncrasy for food. The pylorus closes up, and by stimulus on the sympathetic nerve there may be produced a spasm which is obstructive and adds to the difficulties of emptying.

People with ulcers are relieved by eating food. They say: "I have hunger pains, and I am relieved if I can get something to neutralize or dilute my acids or give work for them," and they take soda or food. The pains do not occur while there is food in the stomach. There is spasm of the pylorus, and retention of gastric secretion after the food has left the stomach, and there is a gnawing pain; the gastric juice is ready for action, but nothing is there for it to work upon, and then the hunger pain occurs.

There is the very important fact that under normal conditions the pylorus is closed, that it opens under the influence of alkalies below it, or above it, and that a little intermittent contraction occurs just below the common duct for this final emptying of gastric secretions. After the food has left, and some acid remains, there should be a partial regurgitation of waves from the common duct area with Brunner's gland secretion, which should flow back into the stomach to help it to empty. On numerous occasions, after a too radical removal of the gall-bladder for jaundice by a vigorous operator, I have united the hepatic duct above the pylorus, and afterwards the pylorus is always open. I have had such cases in which fourteen years have elapsed since operation, but the pylorus is open because you are making use of the patient's own alkali. If you could increase pyloric spasm without overdoing it, you could prevent this regurgitation of the alkalies, increase the retention of this highly digesting fluid, and there might then develop a chemical ulcer.

There may be infarctions of bacteria, which select the stomach because they come from the stomach, because part of the gastric wall and the gastric mucous membrane, so far as the hormone is concerned, is associated with the production of that bacterial culture. In the case of higher acidity we should expect emptying of the stomach to take place more quickly; but burning and trouble come on earlier after eating.

There is much in the selection of types of operation. It is a question whether they have to do with hæmorrhage. In 24 per cent. of ulcers of the duodenum there is hæmorrhage, but in only 8 per cent. is there serious hæmorrhage. You must elicit from the patients whether they have tarry stools. People with bleeding ulcer make the best spontaneous recovery; they go on well for a year, or even several years, and then suddenly there is another terrible hæmorrhage. The ulcers do not often perforate, and the question is whether there is too much or too frequent bleeding which cannot be controlled. There is also the question as to whether the patients suffer just as if they had an active ulcer without hæmorrhage; such cases do well after gastro-enterostomy. In the case of operation for ulcer of the duodenum where the patient has suffered from hæmorrhage, there may still be hæmorrhage three, four, or even seven years after an operation which has been successful in giving relief.

We find in our studies that a little more effort on the part of the sympathetic nervous system produces these partial spasms of the pylorus that prevent the normal alkalization, by which the acids are lowered and the pylorus opened for final emptying. After gastro-enterostomy, the patients can use their own soda apparatus, and we can treat them along the same lines. This is also the reason why we have hour-glass stomach—a ring contraction which forms on the line of the lower end of the acid-producing upper part of the stomach. The pyloric hormone has not much to do with acid formation. I have taken out that hormone area many times and its removal does not stop the formation of the acid.

I have taken some time to bring out these points. We have been working for forty years on the surgery of the stomach with much yet to learn.

I think those who practise the medical profession have more the habit of acquiring the closed mind than most other people, and they get a fixed "set" in their ways. It is said that "reading maketh a full man"; but I think that travel makes us lose our respect for our own opinions to a large extent, for I have witnessed most beautiful operations which were done wholly differently from the way I do them, and were yet just as successful. And I have seen the same type of operation as that which we do at the Clinic done the world over. So as we travel about and see these various plans for accomplishing the same result, we appreciate the fact that there are "many ways of going to the post-office," and that the essential aim of surgery is to accomplish your desired result with the least possible danger to the patient.

[March 4, 1925.]

An Advanced Case of Spindle-celled Sarcoma of the Breast.

By PINTHU SAI, M.B., B.S.Lond.

(Director and Surgeon, Siriraj Hospital, Bangkok, Siam.)

WITH A

Pathological Report on the Specimen by Professor A. G. ELLIS, M.D.

(Pathologist to the Hospital and Rockefeller Representative.)

History.—Patient, a married lady, aged 52, of Siamese nationality, complained of a lump in her right breast, of two years' insidious onset. She had been married for twenty years without any offspring. Her period, starting at the age of 15, had been of the irregular type all her life, often disappearing at times for three or four months and then coming on again; and she said it was her present complaint.

The tumour had been growing rapidly during the last three months and had been troubling her a good deal on account of its weight and of its pain, felt more during her period. It was now so big that she had had to devise a sling for its support and she was unable, on account of its size and weight, to lie on her back, for the tension on it caused pain.



FIG. 1.

On examination, a large, more or less round tumour of two years' duration was seen to occupy the whole breast; it measured 11 in. vertically, 8 in. in its transverse diameter and 9 in. antero-posteriorly. Deep pressure upon the tumour did not cause pain; the tumour was extremely heavy to lift up. Its actively growing into projections made it appear somewhat irregular in outline and suggested progressive degenerations taking place within its substance. The shining appearance of the surface in parts, in the upper and inner quadrant, indicated the signs of starting ulceration. A

large number of veins coursed towards the nipple from the periphery. There was no discharge from the nipple on pressure. The deeper parts of the tumour were fixed to deep structures, and it was impossible to move it about in either direction on the patient lifting up her arm above her shoulder.

A close attachment of the tumour to the sternal origin of the pectoralis major muscle could easily be made out. Its great weight and its rapidity in growth suggested its nature as being one of actively growing sarcoma of the mammary gland in a nulliparous subject.

A photograph of the tumour reproduced (fig. 1) shows its enormous dimensions. A photograph of the patient after the operation was not taken on account of my being absent from the hospital on the day she left the ward.

It is a matter of speculation as to why a nulliparous female with irregular menstruation should be the subject of a mammary sarcoma.

Operation.—I performed Halsted's operation and removed the patient's breast successfully. She made an uninterrupted recovery and left the hospital after fourteen days' stay in the ward, relieved of the great burden she used to carry. This case is remarkable for the enormous size and rapidity in growth of the tumour, and also for the ignorance of the patient in having left it to grow to such a size.

A somewhat similar case was successfully operated on by me at the same hospital two weeks later. The pathological sections showed it to be of the same nature, and this patient also made a good recovery and left the hospital similarly relieved of her burden.

A microscopical drawing of a section of the tumour (reproduced, see fig. 2) has been made by Dr. R. W. Mendelson, Medical Officer of Health, Bangkok.

PATHOLOGICAL AND MICROSCOPICAL REPORT ON THE SPECIMEN BY
PROFESSOR A. G. ELLIS, M.D.

The specimen is, in general, quadrilateral in shape, the upper end being slightly broader. It measures 11 in. (27 cm.) vertically, 8 in. (20 cm.) transversely, and 9 in. (22 cm.) antero-posteriorly. It weighs 17 lb. (7.7 kg.).

The posterior surface has a nearly circular, flat, freshly incised area 8 in. (20 cm.) in diameter, the site of amputation. The remainder of the specimen is covered by skin and in general is smooth, but has a number of slightly raised, broadly rounded elevations, especially at the lower end and upper internal angle. These elevated areas are quite soft, the remainder of the specimen being fairly firm. The skin is very smooth over some of the elevated areas but has nowhere become extremely thinned.

The incised surface has small areas of what appears to be muscle that is infiltrated by tumour, these having been divided by the incision made for removal; this tissue is greyish-red in colour and firm.

For examining the tumour, a median incision was made into the cut surface towards the centre, but not cutting the skin. The incised surfaces at the base are of grey, slightly lobulated, rather soft and very moist tissue. Towards the centre are darker, degenerated or hæmorrhagic areas with small spaces, from which reddish fluid escapes; no definite cysts are present. From these incised surfaces blocks of tissue were taken for microscopic study.

Sections from these blocks are of a new growth made up largely of small spindle-cells in interlacing bands. There is very little fibrous stroma. Among these cells are numerous thin-walled blood-vessels or channels with no demonstrable wall (see fig. 2). Areas of slight or extensive hæmorrhage are numerous.

In many parts of the sections myxomatous degeneration has occurred, leaving

only a few scattered cells. This change includes just noticeable points to extensive areas of the tumour. Breast tissue is not present in any of the sections studied.

Diagnosis.—Spindle-cell myxosarcoma of breast.

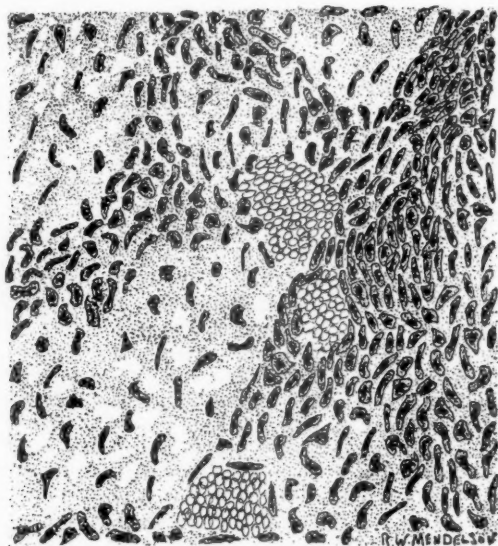


FIG. 2.—Spindle-cell sarcoma of breast. Part of the drawing shows the effect of degenerative change in the intercellular substance and cells of the tumour, though this field was not taken from an advanced portion. Blood-channels among the cells are shown. (Professor A. G. Ellis, M.D.)

Section of Therapeutics and Pharmacology.

President—Professor A. J. CLARK, M.C.

Recent Progress in the Study of Experimental Scurvy.¹

By S. S. ZILVA, D.Sc.

THE subject with which I am about to deal to-day will be treated mainly from the chemical point of view. Experimental scurvy falls within the province of several branches of science and obviously one cannot avoid touching its pathological, physiological and even medical sides. My knowledge of these branches of science is indeed very limited, but so long as one is fully aware of the limitation the danger is not as great as it may seem.

Nutrition and diseases arising from nutritional disorders have greatly attracted the attention of researchers in medical science during the last decade or so, in much the same way as problems connected with infection and immunity received the main share of their attention in the previous generation. The reason for this is not far to seek. The advance made in the chemistry of the constituents of foods, mainly proteins, the partial elucidation of the part played by inorganic constituents in nutrition, and finally the discovery of the vital part played by minute quantities of accessory factors of unknown chemical constitution, brought into greater evidence the important bearing of nutrition on the causation of disease. The exigencies of war conditions were a further stimulus to scientific observers to follow up this line of work in the light of the latest knowledge. Although the existence of accessory food factors became plain from various researches in nutrition during the last fifty years or even previously, this subject was only definitely formulated by Hopkins a few years before the war. The name "vitamin" was at first coined by Funk for the factor concerned in the prevention of polyneuritis in birds, and beri-beri in man, and it was soon applied generically to a number of these unknown principles. It would not be amiss at this juncture to point out that the term as it is now used does not imply any particular chemical characteristic, but rightly or wrongly is utilized as a convenient expression for substances of unknown chemical nature which are required in extremely minute quantities, and which cannot be synthesized by the organism. We do not even know whether they are of an organic or inorganic nature. To-day we are concerned with one of these factors, namely, the antiscorbutic factor, frequently referred to as vitamin C, the absence of which from the diet is responsible for the onset of scurvy.

Scurvy, I need hardly mention, is not a disease prevalent to any extent in this country in a well-declared form. I say "well-declared" deliberately. Adult scurvy may even be considered as non-existent in Britain. Both adult and infantile scurvy could be treated successfully prior to the experimental days. The treatment, as is well known, was dietetic, and with the exception of the very small school which ascribed the causation of scurvy primarily to infection, it was generally acknowledged that the aetiology was in some way connected with faulty feeding. This could be rectified by the addition of fresh foods, especially fresh vegetables, and British seafaring and medical men were mainly responsible for the introduction of this treatment. On the identification of infantile scurvy with the adult form of the disease the method of treating the former at once became obvious.

The experimental period of scurvy began with the discovery of Holst and Frölich

¹For the loan of the blocks illustrating this paper the author is indebted to the courtesy of the University Press, Cambridge.

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that the disease could be regularly induced in guinea-pigs by dietetic means. In their classical publication of 1912, recording the result of several years of careful labour, they demonstrated beyond any doubt that a condition of scurvy, similar to that of the human being, could be produced in these animals if they were fed solely on a diet of oats and bran. The disease becomes manifest after about fifteen to eighteen days. If this diet is continued without being changed the animals decline in weight and succumb with the manifestation of the characteristic post-mortem changes, namely, intramuscular and subcutaneous hæmorrhages, osteoporosis, displaced costo-chondral junctions, &c. If, on the other hand, the scorbutic diet is supplemented with an antiscorbutic food or preparation the onset of the disease is prevented, or if the antiscorbutic substance is administered before the disease is too far advanced, it is mostly arrested and cured. This method of estimating the antiscorbutic potency of substances has since been employed in all experimental work on scurvy. The basal diet has been modified by various investigators but the principle of the method remains unchanged. All the results to which I am going to refer this afternoon were obtained by feeding the substance to be tested *per os* daily to guinea-pigs subsisting on a diet of oats, bran and a daily ration of 40 c.c. of autoclaved milk. The substance was administered after the animals had been on the diet for about ten to fourteen days, and in the case of active substances the animals were killed by chloroform after two months, and the degree of scurvy, if any, was ascertained at the post-mortem examination. In the case of totally inactive substances the animals behave as they do on the basal scorbutic diet alone. On insufficiently active doses the onset of, and the fatal termination of, the disease are delayed.

By utilizing the guinea-pig as a test animal, quantitative results can be obtained with an error of about 20 to 25 per cent.

Another animal suitable for experimental scurvy is the monkey. This animal is also very susceptible to the disease when kept on a scorbutic diet. A scorbutic diet, well balanced in other respects, suitable for this purpose is rice, caseinogen, supplemented by salts, marmite and an occasional dose of cod-liver oil. The time taken for this animal to develop the disease varies with the individual but is usually about two months. The advantage of utilizing the monkey in experimental scurvy is that it is more suitable for curative work. The administration of one or two doses of a potent substance will make the disease clear up with astonishing rapidity. More dilute solutions may also be employed owing to the larger capacity of the monkey's stomach. On the other hand, the longer the time taken in inducing the disease, the greater variation in the individual disposition to it, and the higher cost of the animal and of its upkeep makes the monkey less suitable for experimental work than the guinea-pig, especially for quantitative work.

These biological methods are still the only ones available. Various attempts to introduce chemical methods for the detection and estimation of the antiscorbutic factor which would be less laborious, less time-consuming and more exact, have failed. Recently Bezssonof made the observation that a number of antiscorbutic substances gave a blue coloration with phosphomolybdotungstic acid in an acid medium (Folin's reagent for phenol), and he suggested the use of this reagent for the detection of the antiscorbutic factor. In view of its importance Mr. Kay and I investigated the problem in detail and we arrived at the conclusion that the test is unreliable. We found that although a great number of antiscorbutic substances give the reaction, some do not. On the other hand some substances which do not possess any antiscorbutic activity at all give an intense coloration with the reagent. Further, no quantitative relation exists between the intensity of coloration and the antiscorbutic activity in substances which react in both ways. It is plain that this reaction is of little value and may even be dangerously misleading. I have since found another reaction, namely, the reduction of ammoniacal silver nitrate in the

cold, to coincide so far in every case tested with the presence of the antiscorbutic factor, but, as I shall show later, this reaction also cannot be relied on.

Since Holst and Frölich's discovery much work has been done, including some experiments I am going to mention to-day, which definitely prove that results obtained in the laboratory are applicable to human scurvy. Holst and Frölich, in their original work, ascertained some of the properties of the antiscorbutic principles and established its presence and absence in various foods. The quantitative distribution of the antiscorbutic factor in nature has since been worked out in great detail, and extended by many careful and efficient workers, so that we are now in possession of many facts which may be considered as a great asset to the dietetician. Our debt is specially due in this respect to Miss Chick, Miss Hume, Miss Delf and other workers of the Lister Institute. Some facts, as I have said before, have actually been applied and confirmed in practice. A striking instance is the observation made by Fürst, a pupil of Holst and Frölich, that cereals and pulses which do not possess any antiscorbutic activity become potent on germination. Wiltshire in 1918, making use of this observation, succeeded in curing twenty-seven cases of mild scurvy by including in the dietary 4 oz. of germinated haricot beans. Another set of thirty of his patients who received instead 4 oz. of lemon-juice, did not do much better. He thus confirmed, clinically, Fürst's experimental observation. Recently Dr. Harden and I have shown that this activity is acquired by barley even before germination is actually observed. This was demonstrated by a malting experiment, in which the grain was tested in the various stages of the process. In the preparation of malt, the barley is first steeped. It is then allowed to germinate at a suitable temperature, after which it is kilned to arrest the germination. After removing the sprouted germ, malt is obtained. We found that after steeping, and before any visible signs of germination appear, the barley which was previously inactive showed marked antiscorbutic potency. So also, of course, did the germinated barley. This activity was, however, destroyed in the kilning. It would be of great interest to test the antiscorbutic value of steeped grain in practice, as the procedure is simpler than germination, and would be of great use in cases of emergency.

The investigation of the chemistry of the antiscorbutic factor is fraught with many difficulties. It is present in such minute quantities in all the active natural products that in attempts to isolate it by employing the usual chemical technique, active fractions are obtained in which the impurity, paradoxically, is the main bulk of such preparations. It is associated with great labour, since their activity can only be established with certainty by the daily administration of such preparations for six weeks. In addition, one is greatly restricted in the use of reagents, since toxic and disturbing effects on the animals must be borne in mind in view of the prolonged period of feeding. An inquiry of such a nature is associated with great expense, and it was only possible to carry out much of this work because of the generous financial support received from the Medical Research Council.

Before any serious attempts at the isolation of the antiscorbutic principle could be made its properties had to be ascertained in order to ensure stability in its manipulation. Of these, information on its relation to heat, its behaviour towards oxygen and change of reaction, afforded valuable help.

Antiscorbutic solutions on storage, unless certain precautions are taken, lose their activity. This loss we know now can be mainly attributed to oxidative changes. Miss Delf, in studying the effect of heat on the antiscorbutic potency of juices, observed that a marked activity was retained by orange- and swede-juice heated above 100° C. As the juices were heated in an autoclave and consequently in absence of air, she suggested that the destruction might have been affected by retarded oxidation or by the formation of stabilizing bodies in the process of heating. That the former was the case was proved independently by Hess and myself as an indirect outcome of different investigations. Hess found that the

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addition of 4 c.c. of a normal solution of hydrogen peroxide to a litre of raw milk, which was left in the incubator overnight, prevented the development of bacteria, and at the same time affected the potency of the milk to such an extent that 80 c.c. of this milk per day did not prevent or even delay the onset of scurvy in guinea-pigs; in other words, the destruction of the antiscorbutic factor was effected by the action of the hydrogen peroxide. My observations on the oxidation of the vitamin were arrived at from experiments carried out upon the action of ultra-violet rays on the inactivation of the vitamin. I observed that destruction of the antiscorbutic factor took place when active solutions were exposed to the light in open vessels. This I was soon able to prove was not due to the action of the light as such, but to the action of the ozone which the lamp generated. It was further found that ozone, even in the dark, was able to destroy the potency of active solutions. These observations led to more inquiries in order to ascertain whether less drastic oxidative means would have a similar deleterious effect and this I found was actually the case.

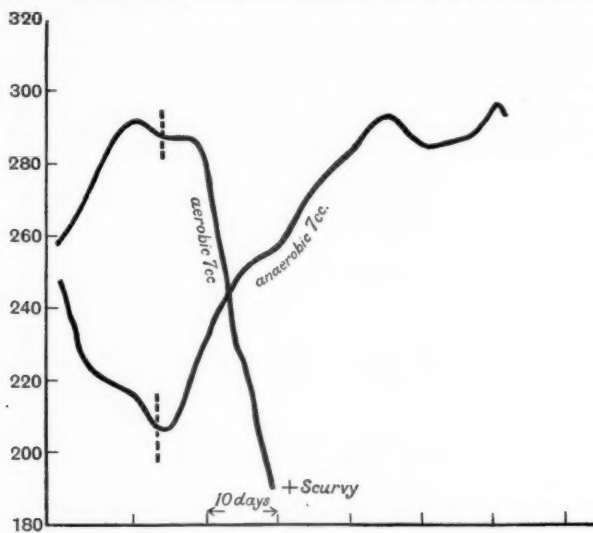


CHART I

When air was aspirated through an antiscorbutic solution for twelve hours a loss of about 70 per cent. to 80 per cent. in the potency was recorded, a loss which would have taken much longer (many weeks) to occur if kept under ordinary conditions. A greater loss was observed when air was aspirated through a boiling solution for one hour. On the other hand boiling for two hours in an atmosphere of CO_2 made no impression on the activity. In extending this inquiry, I was able to correlate the change in reaction with the inactivation brought about by oxidation. In their early work, Holst and Frölich had observed that antiscorbutic solutions were more stable in acid solution. Dr. Harden and I have shown that alkalinity has a deleterious effect on the vitamin. From indications of another observation I concluded that a connexion exists between reaction and oxidation in the destruction of the activity. I was able to demonstrate it definitely by a series of experiments. When an antiscorbutic solution is made about $\frac{N}{20}$ alkaline (about $\text{pH } 12$) and allowed to stand for twenty-four hours in the air it is totally inactivated. If this change of reaction is brought about whilst taking great precautions in excluding air,

and the liquid kept for twenty-four hours under anaërobic conditions, no loss takes place at all. This is shown by (Chart I) giving the weight curves of two animals fed on the aërobic and anaërobic solutions.

No loss was observed even in the case of the minimum dose when the alkaline preparation was kept anaërobically.

This oxidative change takes place fairly rapidly since it can be demonstrated that 80 per cent. of the activity is thus destroyed in half an hour, whilst after an hour no activity is left in the solution at all, as will be seen from the weight curves of animals receiving daily dose of 5 c.c., the minimum dose being about 1.5 c.c. (Chart II). A daily dose of 3 c.c., the sample being exposed for half an hour, failed to prevent or even delay the onset of scurvy.

Another set of experiments has shown that by making the solution of the same acidity as the original lemon-juice, i.e., by lowering the PH to 2.2 and aspirating air through a boiling solution, less destruction was brought about than when the

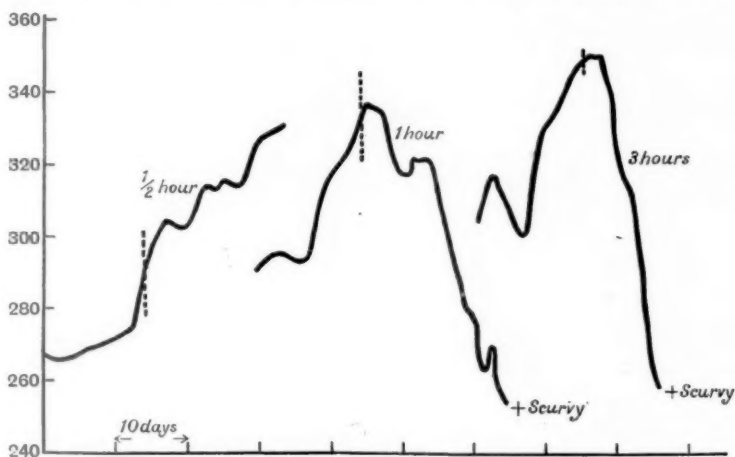


CHART II

solution was neutral, i.e., PH 6.8-7. This is shown by the weight curves of animals which received 5 c.c. per day of the various preparations (Chart III).

The experiments I have just discussed give us information which is of importance in the manipulation of antiscorbutic substances, whether it be in the kitchen, in the factory, or in the laboratory. It throws light on one of the chief causes responsible for the loss of the antiscorbutic activity on storage and on heating. By observing the necessary conditions it is now possible to prevent such loss to a very great extent. This was demonstrated by some experiments on the conservation of the potency of concentrated antiscorbutic preparations. These were kept without any loss for five months in acid solution in evacuated flasks. Not only was this stability demonstrated by the ordinary laboratory experiments on guinea-pigs, but such stored preparations were found effective by Dr. Still in the treatment of infantile scurvy. There is no reason why longer periods of storage under such conditions should have a more damaging effect. Experiments on these lines are still in progress, and it is hoped they will disclose whether or not there is a limitation in the period of storage.

We will now turn our attention to the progress made in the isolation of the anti-

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scorbutic principle. This was mostly carried out on lemon-juice, which was found to be a good source for that purpose. As you are aware, the main bulk of the solid matter of lemon-juice consists almost entirely of citric acid. The total solids of the juice vary from 8 to 9 per cent., and the citric acid from 7 to 8 per cent. The first thing to ascertain was whether the activity was associated with the citric acid fraction, or with the neutral residue of the juice. This was a comparatively easy task, since the calcium salts of citric acid can easily be removed by neutralizing the juice with calcium carbonate, and treating the neutralized solution with excess of absolute alcohol. In this way not only is the acid removed, but also other matters which are precipitated by alcohol, such as proteins. From quantitative experiments, and allowing for the error inherent to the method of estimating the antiscorbutic factor, it was found that the entire activity remained in the neutral residue. By this very simple manipulation it is therefore possible to remove about 85 per cent. of the solids of the juice, leaving a neutral residue which, owing to its low solid

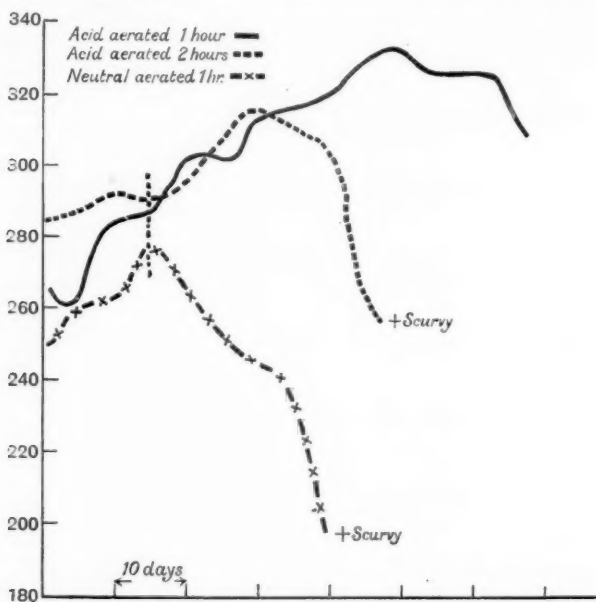


CHART III

matter content, can be concentrated many times, yielding a very palatable and highly active preparation. Such preparations have been tried therapeutically by Dr. Still on several occasions during the last few years with good results, especially on children, in whose case the scorbutic condition is complicated by digestive disturbances. As I have already mentioned, by utilizing our knowledge of the influence of oxidation and reaction on the stability of the antiscorbutic factor, it is now possible to prepare such concentrated antiscorbutic solutions which will retain their activity for some time. Although by this first step in the isolation of the vitamin it was possible to remove the major part of the non-active solids of lemon, the real difficulties, from a chemical point of view, were found only to begin at this stage. The neutral active residue of the lemon juice was ascertained to consist mainly

of invert sugar. To remove sugar by ordinary chemical means is associated with drastic treatment not suitable for the fractionation of an unstable principle such as the antiscorbutic factor. Recourse was therefore had to a biochemical method. I have shown that by fermenting this neutral fraction with yeast under certain conditions the sugar is almost entirely removed, leaving behind the total activity in the residual fraction. This definitely disposed of the possibility that the activity was in any way associated with the bulk of the sugar of the juice. Moreover, this new fraction was more suitable for further manipulation. Numerous precipitating agents were tried next. Some were proved to be totally unsuitable or only partially suitable. One of these reagents, namely, basic lead acetate, was however found to precipitate the entire vitamin content. After removing the lead by suitable means a very active fraction was obtained. The table (Chart IV) illustrates my scheme of fractionation.

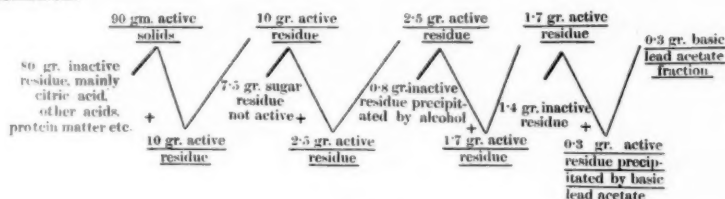


CHART IV

The above figures are an approximate average. They vary to some extent with the juice. It will be seen that the solids of such active solutions form only about 0.03 per cent. of the solution; the activity is that of the original lemon-juice which contained 9 per cent. of solid matter.

A very active fraction can also be obtained by precipitating deacidified lemon-juice with basic lead acetate without previously fermenting the juice. More inactive matter, including some sugar, is brought down in this fraction with the vitamin, and the fraction is therefore not as pure as the one previously discussed. The chief chemical characteristics of the most potent fraction are that it contains very little nitrogen, merely traces of phosphorus, and that it decolorizes potassium permanganate and reduces ammoniacal silver nitrate in the cold. This last reaction, as was mentioned before, was found so far to be given by every active fraction. These reducing properties of active substances have been studied by Mr. Connell and myself in relation to their activity. The action of oxygen in the presence of heat and in alkaline solution was investigated. It was found that although this treatment, as in the case of the antiscorbutic activity, does destroy these reducing properties of active solutions, the rates of the destruction of the three properties—namely, the antiscorbutic activity, the property of decolorizing potassium permanganate, and the property of reducing silver nitrate—were decidedly different. The reducing properties of active solutions cannot therefore be taken as criteria for their activity. This will be seen from the following figures:—

TABLE I.—INACTIVATION BY HEAT.

Solution	Percentage of silver-reducing substance destroyed	Percentage of permanganate-reducing substance destroyed
Original	0	0
Heated in CO ₂ for 3 hours	0	0
Heated in air for 1 hour	25 ± 15	5
Heated in air for 3 hours	40 ± 20	5

TABLE II.—INACTIVATION BY ALKALINITY.

Solution	Percentage of silver-reducing substance destroyed	Percentage of permanganate-reducing substance destroyed
Original	0	0
Standing 24 hours in vacuum	0	0
Standing 1 hour in air	30 ± 12	6
Standing 3 hours in air	45 ± 25	6

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Besides the purely chemical investigations, the antiscorbutic factor was also studied from the physico-chemical point of view. This was mainly restricted to work on adsorption and diffusion through collodion membranes. My own work on adsorption has yielded a number of suggestive but in many instances ambiguous results: so much so, that I am not in a position to give you much definite information on this subject. One point I am satisfied about, namely, that adsorption in neutral solution will inactivate such solutions. Whether this is due to the removal of the principle in active form by the adsorbent, or to catalytic surface oxidation on the adsorbent, I am not yet in a position to say. More definite results were obtained by diffusion experiments. Some years ago Brown introduced a suitable method of differential dialysis through collodion thimbles. He has shown that by soaking collodion thimbles in alcohol of various strengths, different degrees of permeability could be obtained. Thus a thimble soaked, say, in 30 per cent. alcohol will allow substances of lower molecular weights and possibly of lower molecular dimensions to pass through, whilst impeding the passage of substances of higher molecular weight which would pass through a thimble soaked in stronger alcohol, say, 90 per cent. In other words, one obtains a sort of molecular sieve, the size of the meshes of which can be regulated by the strength of the alcohol in which the collodion is soaked. Dr. Miura and I have utilized this technique in order to obtain some information as to the approximate size of the active molecule. The results revealed that membranes which permitted free passage to substances of small molecular dimensions such as sodium chloride retained the antiscorbutic factor during a period of three to four days of dialysis. Only thimbles through which high molecular dyes known as semicolloids, such as neutral red, methylene blue, &c., pass, allowed the antiscorbutic factor to dialyse.

A 90 per cent. thimble only just allows the passage of semicolloids, whilst a 95 per cent. thimble is freely permeable to them. Chart V shows that the antiscorbutic vitamin behaves similarly. From this observation it can be concluded that the size of the active molecule must lie between the molecular dimension of sodium chloride and that of a semicollloid. This investigation was extended more recently by Mr. Connell and myself. It was possible to demonstrate that the antiscorbutic factor diffuses through membranes of somewhat lower permeability than those

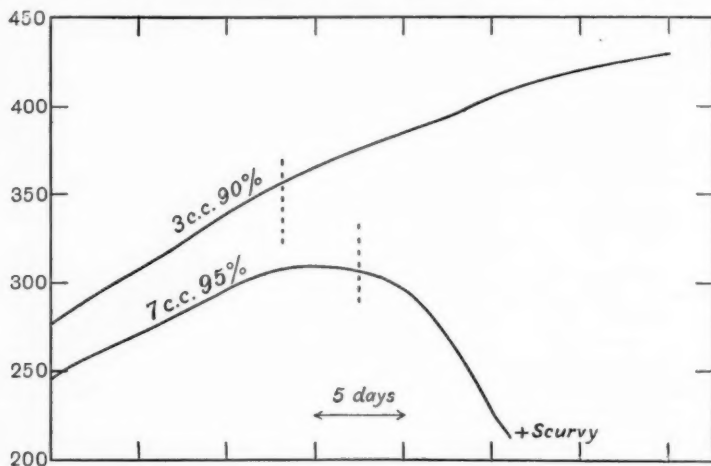


CHART V

which permit the passage of dyes—the size of the active molecule appears to be not far removed from that of a hexose. The rates of diffusion of the antiscorbutic factor, of the nitrogenous fraction and of the reducing sugars of decitrated lemon-juice, were studied in great detail in this investigation and definite evidence was obtained proving that the rates are different. This will be seen from Chart VI.

This confirmed the view that the antiscorbutic factor is not associated with the sugar, which as you have seen was already demonstrated by chemical means to be

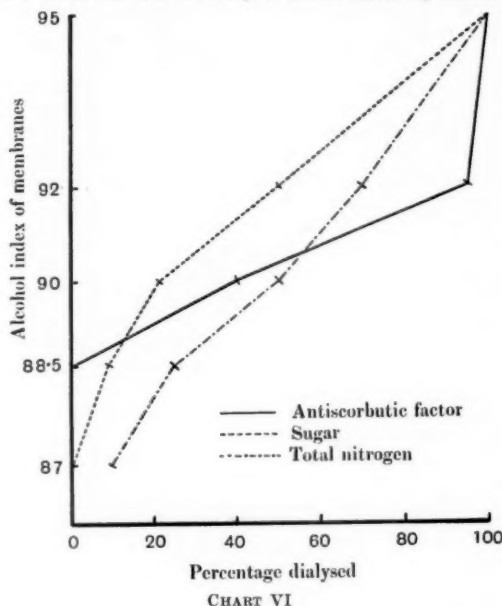


CHART VI

the case, and it further suggested that the bulk of the nitrogenous fraction of decitrated lemon-juice cannot be the source of the active principle.

The facts I have brought before you this afternoon form a short summary of the main work done in experimental scurvy during the last few years. The investigation is still in progress and more facts are gradually emerging. How far we are still from the ultimate goal, namely, the identification of the vitamin, is difficult to say. The main properties have been established. The connecting link in scurvy between the laboratory and the ward has also been forged; and finally it has been definitely demonstrated that very minute quantities of the substance only are required to fulfil the antiscorbutic function, thus justifying its name of vitamin. The task before us is clear: the immense interest of the subject fully justifies further honest, patient, and intelligent efforts.

The Treatment of Infantile Scurvy.

By G. F. STILL, M.D.

THE treatment of infantile scurvy has been but little affected by recent research in connexion with vitamins; indeed it is difficult to see how it could be, for the fact that infantile scurvy is due to deficiency of some factor present in fresh foods, such as fruit and vegetables, raw milk and raw meat juice, was not only recognized by

Cheadle, as far back as 1878, but was already at that time applied to treatment. It was emphasized again by Barlow in 1889 when he gave explicit directions as to the use of orange-juice, potato, raw milk and raw meat juice in the treatment of this condition.

My own observations date back thirty-two years and I have notes of 134 cases of infantile scurvy seen during that time. Looking back on that series I venture to say that treatment was just as effective at the beginning of that period as it is to-day.

The researches of biochemists, however—and not least among them the researches of Dr. Zilva—upon scurvy in recent years, have provided for the clinician methods of administering antiscorbutics which may be of great practical value in particular cases. In any observations upon the relative value of antiscorbutics or of different methods of administering them, it is obviously desirable that the vitamin-deficient diet which produced the scurvy should be unaltered except for the addition of the particular antiscorbutic to be tested. The biochemist very rightly continues the basal diet upon which his patient has acquired scurvy, when he is estimating the value of an antiscorbutic; the clinician has responsibilities which prevent this. I mention this to forestall the very justifiable criticism that most of the clinical findings are not based on what the laboratory observer would call "a clean experiment"; there are inevitable fallacies in clinical observations, but not, I think, sufficient to deprive these observations of practical or even of scientific value.

In the series of cases mentioned, whatever treatment was adopted for the scurvy the infant was at the outset taken off the patent "food," or other scorbutic diet upon which the scurvy had occurred, and in almost every case was put on milk, or diluted milk, which in the large majority of cases was heated just to the boiling-point. This alteration in feeding does not, I think, invalidate the results obtained from various forms of treatment, for experience shows that when not heated at all cow's milk has comparatively feeble antiscorbutic value, so that although able to protect a healthy infant from scurvy it is only slowly curative of the infant who has already acquired scurvy, and therefore, *a fortiori*, the cow's milk will have still less curative influence when boiled.

In illustration of the feeble antiscorbutic value even of raw milk, I will mention the case of an infant who developed scurvy after being fed for several months upon a mixture of pasteurized milk with a completely malted carbohydrate patent "food." Three weeks before the scurvy symptoms appeared the feeding had been changed to unpasteurized fresh milk; nevertheless the scorbutic effect of the previous feeding was not counteracted, and the infant developed severe scurvy.

The majority of cases in my own series were treated with "potato-cream." The biochemist as a result of experiments on animals seems inclined to rank potato as of lower antiscorbutic value than fruit-juice, particularly orange-juice or lemon-juice. Speaking purely as a clinician and admitting freely the practical difficulty of ensuring strictly comparable conditions of experiment in clinical work, I would suggest that potato is at least as potent an antiscorbutic as either orange-juice or lemon-juice. Apart from its potency potato has also the merit of being one of the most generally available of antiscorbutics.

The method of preparation and administration is very simple. The potato is baked in its skin, so as to become as mealy or floury as possible. The floury part of the potato is then scraped off into a cup and broken up finely, hot milk is next added gradually, and the mixture is at the same time rubbed up with a spoon, just as is done in mixing cocoa, so as to obtain a uniform and smooth mixture. Enough milk is added to make the consistency that of a medium cream, roughly a cream which will pour out of a jug rather heavily; in actual measurement, two heaped teaspoonfuls (with an average-sized teaspoon holding one and a half drachms of water) to one and a half ounces of milk, or one ounce by weight of the crumbled

floury potato to one and a half ounces of milk. Of this so-called "potato-cream," two domestic teaspoonfuls, that is, three to four drachms, are given three or four times daily, and each dose is given in *part* of a feed.

Attention to the details mentioned is necessary for practical reasons. If the potato-cream is not rubbed into a very smooth consistency some infants object to the rough sensation in the mouth. A smoother mixture can be obtained by using hot milk than by using cold milk in the preparation of the potato-cream. If the potato-cream is put into the whole of a feed and part of the feed is refused, the infant will fail to get the full dose of potato, and as one of the common symptoms of scurvy is loss of appetite, the infant at the beginning of treatment will often refuse to take the full-sized feed for his age. It is generally wise, therefore, to ascertain first what bulk of feed the child has recently been taking, and put the dose of potato into about two-thirds of this, after taking which the infant can have the remainder of the feed. On the other hand, the potato-cream should be put into as large a bulk of feed as is reasonably certain to be taken, for the more it is diluted the less the infant is likely to object to the roughness of the potato.

I have tried in several cases to determine the earliest time at which definite improvement was noticed under the particular treatment used. The symptom which usually begins to abate first is the tenderness in the limbs, and generally the next improvement noticed is some return of movement in the affected limb.

In the case of an infant aged nine months, with much tenderness of the legs and almost complete loss of movement in the left leg, very little movement in the right leg, and complete loss of movement in the left arm, with some hæmaturia, four drachms of potato-cream given three times daily caused definite diminution of the tenderness in less than twenty-four hours, and in eight days after this treatment began the child was moving all her limbs normally and was quite well in every respect, except that the microscope still showed a few blood-cells in the urine.

There is no need to multiply instances, as I might, of the rapid improvement which occurs with potato-cream used in this way. As a rule, three or four doses of about three drachms each are sufficient to produce definite improvement in the sense of diminished tenderness.

The time which must elapse before a case of infantile scurvy loses all its symptoms depends largely upon the lesions which the disease has already produced when treatment is begun. Where, for instance, there is a large effusion of blood under the periosteum, and still more where periosteal ossification has taken place enclosing the subperiosteal hæmorrhage in a bony sheath, it may be many weeks before the last trace of thickening will have disappeared, although the child may seem well otherwise. I have noticed also repeatedly that, as in the case mentioned above, blood-cells can often be found persisting in the urine for days, or even weeks, after all other symptoms of scurvy have entirely disappeared.

The potato-cream treatment at the age at which infantile scurvy is most frequent, namely, from six to ten months of age, is highly effective as a temporary and curative measure in the acute stage, but it is likely to produce digestive disturbance if continued after the acute symptoms of scurvy have all disappeared. In the majority of cases this will take place in about ten or fourteen days; it almost seems as if there were a peculiar tolerance of potato during the acute stage and that this tolerance passes off as the scurvy disappears. For this reason potato-cream is not suitable as a prophylactic, for which purpose fruit-juice should be used. I shall have more to say of prophylaxis later.

Next to potato-cream I find that orange-juice was the most frequent remedy employed in my series. I have used two drachms four-hourly. I am under the impression that this is less rapidly effective than a similar dose of the potato-cream; certainly it is an error to suppose that orange-juice is so potent that a small quantity

such as one or two drachms administered once or twice daily will effect a rapid cure of scurvy; such an amount is not even potent enough in some cases to prevent scurvy, much less to cure it. If orange-juice is given in doses sufficient to produce such rapid improvement as occurs with the use of potato-cream it is liable to cause diarrhoea. In the treatment of scurvy it is well to bear in mind the warning of Glisson, that the child with scurvy "doth impatiently endure purgations"; in other words, that diarrhoea is one of the chief dangers of the disease. In the series of 134 cases there were six deaths, and four of these were due to diarrhoea.

Grape-juice I have thought even less potent. In one case in which the juice of four grapes had been given twice daily, there had been no apparent improvement after fourteen days of this treatment, whereas with potato-cream two teaspoonfuls four times daily, and two teaspoonfuls of raw meat juice three times daily, the infant was definitely better in forty-eight hours and continued to improve rapidly.

Raw meat juice was given in this case, but probably it contributed very little to the improvement. I have used it less and less for this purpose as I have gained experience, for it is so feebly antiscorbutic that it has but little value in the treatment of scurvy, and even as a prophylactic it is ineffectual unless used in comparatively large doses.

I have used also lemon-whey, i.e., the whey strained off from milk curdled by the addition of fresh lemon-juice. Lemon-juice, like orange-juice, is very effective if used in sufficient quantity, but both these juices are apt to disturb digestion and excite diarrhoea.

Where this danger threatens valuable assistance has come from the researches of laboratory workers. Dr. Zilva has proved by animal experiment that lemon-juice can be decitrated and after decitration concentrated without losing its anti-scorbutic value; moreover, when preserved by special methods which he has described, this concentrated solution will retain its potency for months. The special virtue of the decitrated lemon-juice lies in the fact that it has much less tendency to excite diarrhoea than has ordinary lemon-juice or orange-juice, and when it is used concentrated, for instance to one-tenth of its bulk, the equivalent of several lemons can be given daily without producing any disturbance of the bowel.

In one case, a boy (Patrick M.), aged 10 months, after treatment for a few days with potato-cream and orange-juice but later with orange-juice alone, developed loose green stools and became so ill that although the antiscorbutic was stopped, it seemed likely that the child would die from the bowel complication. Concentrated decitrated lemon-juice prepared by Dr. Zilva was then given, the equivalent of nearly four lemons on the first day, and then the equivalent of about five lemons daily; under that treatment the child rapidly improved and recovered.

As showing what a large amount of antiscorbutic it is possible to give in this way I may mention an infant, aged 8 months, to whom I gave the equivalent of about fifteen lemons in the first twenty-four hours. This proved to be too much, for it made the stools loose, but with a reduction of the dose the bowel trouble quickly ceased, and the subsidence of the scurvy symptoms already begun continued. In this case improvement, i.e., diminution of tenderness, was observed thirteen and a half hours after the first dose had been given, during which time only two doses had been taken of $2\frac{1}{2}$ drachms each, the equivalent together of five lemons.

Clearly, when such a preparation is available it may be of great value for the infant whose bowels are already loose when treatment has been begun, or who shows a tendency to looseness under other treatment.

The concentration is, however, a process requiring care and skill, and the storage of the concentrated solution requires special technique in order that the solution may not gradually lose its antiscorbutic activity; such solutions therefore are not readily available for general use.

Decitration of lemon-juice is a simple matter, requiring only saturation of the juice with calcium carbonate, and then clearing by filtration.

PROPHYLAXIS.

Scurvy is pre-eminently a preventable disease. There is only one completely satisfactory prophylaxis against infantile scurvy, and that is breast-feeding.

In theory, no doubt, it is possible for a mother whose diet has been entirely devoid of such foods as supply vitamin C—potatoes and other vegetables, fruit and milk—to give her infant a breast milk which would allow of the occurrence of scurvy; but thanks, perhaps chiefly, to the almost universal use of potatoes as part of the daily food in this country, breast milk is practically never a scorbutic food. I have not myself seen a single instance of scurvy in an infant still on breast-feeding.

If breast milk is not available, and if on account of the risk of tuberculous infection, raw milk from the cow is not advisable, the next best food in my opinion is cow's milk heated rapidly until it just reaches boiling-point, and then allowed to cool quickly. In these days it is very necessary to be sure that the milk has not undergone some heating process before it is sold; in several cases in my series it was found that the milk which had given rise to scurvy had already been pasteurized or sterilized before it was sold, so that, when heated again, it became so deficient in vitamin as to cause scurvy. Another process which is mischievous from this point of view is the homogenizing of milk—a process which, followed by heating to boiling-point, has given rise to severe scurvy.

Dried milk is not protective against infantile scurvy but it is far less likely to cause scurvy than are any of the patent "foods."

In many cases in which scurvy occurs, breast-feeding has been used for the first few weeks or months of life; after that it was replaced by some scorbutic "food." It is interesting to note that the appearance of scurvy is generally speaking correspondingly delayed in such cases, so that instead of appearing at the sixth or eighth, month as is usual, it begins perhaps at ten or eleven months of age or even a little later.

A fact which becomes evident from the study of a large series of cases of infantile scurvy, and which has an important bearing on prophylaxis, is the difference in degree of scorbuticity shown by different foods. The reason of this is not always apparent, but it is a very definite clinical fact. For instance, there is a certain well-known dried "food" composed of dried milk mixed with completely malted cereals, and it is prepared for use by mixing with hot water only. This "food" is very highly scorbutic, it requires a relatively large quantity of vitamin-supplying antiscorbutic to prevent this "food" from producing scurvy, and also to cure scurvy occurring in the infant fed on this preparation; whereas a plain dried milk, by whatever method dried, though it can produce scurvy, is much less liable to do so even if no antiscorbutic is given. Again, there are certain well-known patent "foods"—consisting of partially malted cereal combined with an active digestive ferment which comes into action when the food is mixed with warm milk. They are in the course of preparation heated either up to, or nearly to, the boiling-point. Such foods are potent producers of scurvy, whereas milk heated just to the boiling-point, though it can produce scurvy, very rarely does so.

It is therefore not possible to lay down any rule applicable to all varieties of infant feeding as to the minimum quantity of any particular antiscorbutic which will protect from scurvy; much larger quantities are needful with such patent "foods" of either of the specially scorbutic types just mentioned, than when a plain dried milk, or a condensed milk or boiled milk is used.

But there is another element which complicates the problem of prophylaxis, namely, the infant itself. One of the most perplexing features of scurvy in the infant is the uncertainty of its occurrence. The laboratory worker can produce scurvy at will in his monkey or guinea-pig, but there is no certainty that two infants fed apparently in an exactly similar way on a scorbutic diet will both develop scurvy. To

take a recent instance from my own experience, two successive infants in a family were both breast-fed for the same period, namely three months; then each was put on the same diet, namely one of the well-known "foods" mentioned above, composed of dried milk combined with completely malted cereal. Both infants had this food in its different grades according to age and neither received an antiscorbutic of any kind, except that both on one or two isolated occasions were given a teaspoonful of fruit-juice. Both thrived apparently in equal degree; in both the same diet was continued till they were about twelve months of age. One developed scurvy at ten and a half months old, the other remained quite well. It is difficult to explain such differences, but they occur so often that an assumption of error in the mothers' statements hardly seems sufficient explanation for all cases. I have mentioned these points because they may have a bearing upon the failure of antiscorbutics in some instances to protect from scurvy, although the antiscorbutic had been given in doses which one might have expected to protect.

In many cases the failure has been due to the use of doses which experience has shown to be totally inadequate. Often a teaspoonful of orange-juice or grape-juice has been given in the twenty-four hours, an amount which will offer little or no protection against scurvy from those types of food which I have mentioned as most productive of scurvy—in other words, most deficient in vitamin C.

I have notes of several cases, however, in which scurvy occurred in spite of doses of the antiscorbutic which are commonly supposed to be more than adequate to protect. Most of these were seen in the course of private practice where the infant was under careful conditions of nursing and where the intelligence of an educated mother assisted inquiry.

One infant, with scurvy which began at ten months of age, had been given three teaspoonfuls of grape-juice daily since the age of two months (the food for several months before the scurvy appeared had been of the most scorbutic type mentioned above). I have notes of several cases in which two teaspoonfuls of grape-juice had been given daily for some months up to the time of onset of scurvy.

An infant that had been fed on cow's milk till three months of age, and then on one of the most scorbutic of patent "foods" (one consisting of partially malted carbohydrate associated with an active digestive ferment and prepared by mixing with warm milk), developed scurvy at the age of six and three-quarter months, that is, within the short period of three and three-quarter months from the time the patent "food" was begun, in spite of taking the juice of half an orange daily from the age of three months. It was noteworthy that although the juice of half an orange estimated at about an ounce daily (the father was a medical man, so careful details of the history were obtainable) had failed to prevent scurvy the administration of potato-cream one and a half teaspoonfuls, about three drachms, four times daily, caused the symptoms to disappear rapidly, and in four days the infant was moving his legs much more freely.

In another case the infant had been given regularly the juice of half an orange one day and half an ounce of raw meat juice, extracted in the usual way with cold water, on the next day, for some months, but, nevertheless, as a result of being fed from birth on a patent "food" of the most scorbutic type (dried milk associated with completely malted cereal, prepared by mixing with water) he developed scurvy at the age of nine and three-quarter months.

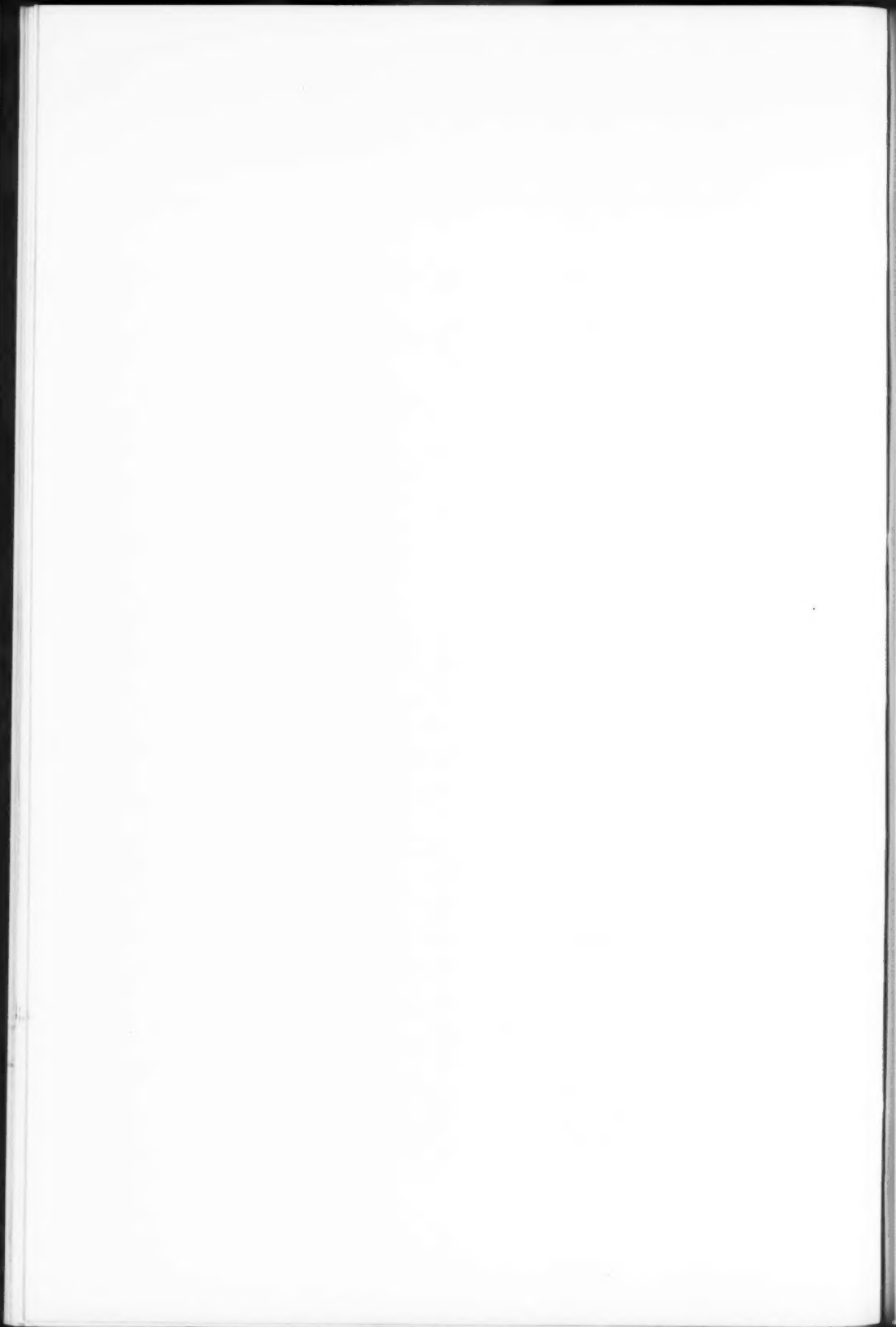
The value of raw meat juice as an antiscorbutic is very small. In several cases raw meat juice one to two teaspoonfuls, probably three to four drachms, had been given three times daily regularly as part of the diet without preventing scurvy. As might be supposed, the various "meat extracts" are quite worthless from this point of view, though I have several times known them to be ordered with the idea of preventing scurvy.

It may not be out of place also to mention that there are several preparations on the market which contain bone-marrow, egg, &c., in combination with malt, and are advertized as containing vitamins, and consequently are sometimes erroneously supposed to be valuable antiscorbutics. They have a real nutritive value, but have

not, so far as I have seen, any antiscorbutic value sufficient to prevent scurvy, which occurred in several cases where such a preparation had been given regularly.

Potato, for the reasons already stated, though so useful and potent for curative treatment, is not suitable for prophylactic use, except in the infant who has already reached the age of ten months at least, when it may be given once daily mashed in gravy.

The most generally suitable prophylactic is fruit-juice, and from the observations recorded here it is clear that when the diet is of a highly scorbutic type more than two drachms four times daily may be necessary to prevent scurvy, perhaps as much as three drachms four times daily. It is advisable to divide the daily quantity into several doses, for one big dose is more liable to disturb digestion and cause looseness of the bowels. Orange-juice and lemon-juice are perhaps a little more potent than grape-juice, but lemon-juice, unless decitrated, is certainly very liable to cause digestive trouble, and orange-juice to a less degree shows this tendency in some infants; but there is marked idiosyncrasy in this respect, and some infants take orange-juice with less risk of disturbance than grape-juice. Tomato-juice, particularly canned tomato-juice, has been recommended in America, and is given in a dose of about one ounce daily.



Section of Therapeutics and Pharmacology.

President—Professor A. J. CLARK, M.C., M.D.

The Unknown Factors of Gout.

By WILLIAM J. SMITH JEROME, M.A.Oxon., M.B.Lond.

I MUST preface what I have to say on the subject of gout with an apology—an apology for submitting for your consideration in place of a statement respecting newly acquired knowledge, a suggestion only, i.e., an unsubstantiated supposition which time, of course, may prove to be either fact or fiction.

I am led, however, to believe that the present suggestion will be found to be fact and not fiction, because, in addition to other considerations, in the first place, it not only gives a reasonable explanation of the *modus operandi* of uric acid in gout, but explains, at the same time, several associated puzzles; and in the second place it does this, so far as I am able to ascertain, without violating any canon of either chemistry or physics, these being the sciences concerned in the several processes I am venturing to suggest.

The puzzles in question are as follows: (1) The cause and mode of production of uratosis; (2) why uratosis occurs in gout, and not in leukæmia; (3) why hyperuricæmia and uratosis do not run parallel; (4) why in an acute attack of gout the renal excretion of uric acid is apt to be at first subnormal, and to become later, at the acme of the paroxysm, excessive in amount; (5) why Ebstein's necrosis is liable to occur.

The attempt to solve these puzzles has necessitated the postulation of some at present undiscovered factor or factors in gout, and I am of opinion that these are to be found (1) in the production of some abnormal gelatinous uric acid; and (2) in the inevitable sequel, viz., a breach of Ringer's fundamental law relating to saline equilibrium.

This law includes the following facts:

(1) For the efficient performance of the functions of animal tissues the kations of sodium, potassium and calcium must be present.

(2) They must bear a definite proportion to each other.

(3) Disturbance of the equilibrium so established makes the unbalanced kations toxic.

(4) When the toxic kations are in a sufficiently high degree of concentration disintegration of tissue occurs.

(5) In addition to such equilibrium in the lymph there must be a definite quanti-

tative relationship between the crystalloids and colloids within the cell; such relationship being absolutely necessary for the normal exercise of the interfacial and other forces concerned in the quickly changing activities of the protoplasm; and for the maintenance of the integrity of the cell itself.

(6) As the adjustments necessary for the maintenance of such equilibrium are both delicate and complex, occasional disturbance is inevitable. And of such disturbance there must be appropriate accompanying symptoms.

Nevertheless in only very few of our long list of maladies is disturbance of saline equilibrium recognized as exercising an important influence. It is probable, therefore, that the symptoms of such disturbance, though recognized, have at times been attributed to other than their rightful cause: and this error I believe to have happened in the case of gout.

My presumption in offering a solution of a problem of such long standing as that under consideration will be understood when I tell you that I have stumbled on a key which contains, I believe, the information necessary for an understanding of the subject.

For this key I am indebted to Sidney Ringer, Jacques Loeb, Sir William Bayliss, and McBain; to the first two for the law of saline equilibrium, and to Bayliss and McBain for their simultaneous discovery of the law of anionic-aggregation. To these four, therefore, if my suggestion proves correct, will pertain chiefly the merit of having solved a problem which has already defied solution for nearly eighty years, mine being the more modest part of interpreting and applying the results of the labours of others.

In the absence of such knowledge as is supplied by the laws in question, a solution of the problem was impossible. It was not, therefore, through want of mental acumen that Garrod ascribed the inflammatory as well as other symptoms of gout to the direct action of uric acid, which since then has been found to be non-toxic. Moreover, I am about to suggest that time will yet show that non-toxic uric acid, by spontaneous chemical action, indirectly and paradoxically, through disturbance of saline equilibrium, and by the agency of toxic ions, is responsible for some—not all—of the inflammatory phenomena of gout.

The manner in which I believe this to take place I will now describe.

Gout, as you are aware, is at present believed to be due to an infection, arising probably in the alimentary canal. During an infection some cells are liable to succumb to the action of the virus, whilst others are less seriously affected.

The nuclein derived from the first of these I suppose to yield abnormal (gelatinous) uric acid, and that from the other cells normal crystalline uric acid.

The crystalline is then gradually, and more or less continuously, excreted by the kidneys; while the gelatinous for a time (three or four days) is retained within the circulation.

This period of retention coincides with that which precedes or accompanies the onset of an acute attack of gout when the renal excretion of uric acid is apt to be subnormal.

On the other hand, the anions of the gelatinous acid, by virtue of their colloidal nature, form aggregates too large to pass through the kidney cells; such aggregates may even contain some undissociated molecules, and they have been named by McBain, "micelles ioniques." The bond, however, which unites these aggregates, not being

that of true molecular combination, but only a lax physical association, is easily loosened in the presence of a multiplicity of sodium kations such as exist in cartilage and other sodium-rich tissues.

As the capillaries of these tissues have a fairly high degree of permeability, they offer little resistance to the passage of the uric anions through them; the many sodium kations present serving to dissociate the aggregates and to attract their constituent ions.

In the lymph the uric and sodium ions combine chemically, forming sodium biurate. This, in consequence of the high sodium concentration, is more or less precipitated. But as the act of precipitation, by the removal of sodium, lowers the sodium concentration, the precipitate undergoes partial solution, the dissolved part, after absorption, being carried to the kidneys for excretion, and the undissolved portion remaining, possibly to form the nucleus of a tophus, or to be absorbed later, as a tophus sometimes is absorbed. It is the simultaneous elimination of this sodium biurate, coming from the gelatinous uric acid, with some of that of the crystalline acid, which makes the renal uric excretion abnormally high at the acme of a gouty paroxysm.

As the lymph of the sodium-rich tissues, though containing a high sodium percentage, is at the same time a balanced saline solution containing, in addition to its other constituents, sodium, potassium and calcium kations, in conformity with Ringer's law, the removal (by precipitation) of sodium makes all the kations, thereby unbalanced, toxic. The toxic ions, then, through alteration of the permeability of the cells, and entry of the ions, excite pathological reaction, in the form sometimes of Ebstein's necrosis, and at others of that of a milder type of inflammation.

The above scheme will be found to explain the puzzles already indicated, e.g. :—

(a) Uratosis occurs because some of the uric acid, being of the gelatinous type, and therefore incapable of passing through the kidneys, permeates the capillaries of the sodium-rich tissues and forms in their lymph a precipitate of sodium biurate, which, because of the high sodium concentration, admits of only partial solution.

(b) Uratosis takes place in gout and not in leukæmia, because some of the uric acid is of the gelatinous type in the first of these diseases and not in the second.

(c) There is no parallelism between hyperuricæmia and uratosis, because the first of these depends on the amount, and the second on the type of the uric acid.

(d) The renal excretion of uric acid during an acute attack of gout is at first below par, and then excessive in amount, because the gelatinous acid is first retained in the circulation through its inability to traverse the kidneys, and later in the form of sodium biurate is partially eliminated, simultaneously with some of the normal crystalline acid.

(e) Ebstein's necrosis occurs when there is a sufficiently high concentration of kations made toxic through an infraction of Ringer's law, in cartilage or other of the sodium-rich tissues.

It may now be asked whether gout is the only disease in which gelatinous uric acid is formed. Uratosis is common in chronic alcoholism, and in lead poisoning; and its mode of production in these cases may be presumed to be the same as in gout. There is, however, a great difference in the nature of a bacterial toxin in alcohol, and of lead. And I would draw attention to the possibility that an early effect of each of these poisons, by deranging the functions of the alimentary canal, may be to beget a gastro-intestinal poison similar, if not identical, in the three cases. It is also

possible that two of the poisons under consideration may simultaneously modify the nuclein-katabolism.

In connexion with the law of saline equilibrium I mentioned the names of Ringer and of Loeb, and I should like to emphasize the fact that, of these, Ringer was the acknowledged pioneer. I mention this because, though Ringer's expertness as a clinician was fully appreciated during his lifetime, it seems to me that his scientific reputation has come to be associated more with his physiological saline solution than with his discovery of a fundamental law of nature—this latter being a high distinction, and one which falls to the lot of relatively few.

Section of Tropical Diseases and Parasitology.

President—Professor LEONARD S. DUDGEON, C.M.G., C.B.E., F.R.C.P.

The Relation of Nephritis in Egypt to Intestinal Infection.

By H. B. DAY, M.C., M.D., F.R.C.P.

(ABSTRACT.)

MORE cases of nephritis with oedema are admitted to Kasr-el-Aini hospital, Cairo, than to a general hospital in London. In the last few years I have personally studied some 100 cases and can supplement observations previously published.

For practical purposes a rough division of these cases into three groups will suffice, viz., acute nephritis (thirty-four cases), syphilitic (five cases) and chronic (sixty-one cases).

The acute cases are of most interest from the ætiological standpoint. In 88 per cent. of this group there appeared a causal connexion between the renal inflammation and an antecedent infection. Twenty out of twenty-seven patients from whom a history was obtained had been incapacitated for some time by definite illness before the appearance of renal symptoms. The results of this inquiry, confirmed by observations in hospital, are tabulated below:—

Illness	History only	Persisted on entry	Found in others
Fever (usually prolonged) ...	2	6	6
Fever later accompanied by dysentery or diarrhœa ...	3	4	2
Dysentery ...	4	1	2
Total	20		10

While in a few cases antecedent fever was probably due to malaria or acute pyelitis, in the majority it appeared to be closely associated with some bowel disturbance which was apt to develop after a few weeks.

Examination of the patients showed that in 55 per cent. of these acute cases of nephritis there were gross signs of *Bilharzia mansoni* infection. The history of prolonged fever followed by dysentery, often slight or transient, or by enlargement of the spleen and liver, is quite characteristic. Nephritis seems specially liable to occur as a complication when ova begin making breaches in the intestinal mucosa.

Other forms of intestinal disease, especially when prolonged, account for some cases of nephritis. Ankylostomiasis, in spite of its wide prevalence, appears much less important a cause than does disease affecting the large intestine.

Urinary bilharziasis not infrequently implicates the ureters and so predisposes to renal disease, often of a more septic type.

BACTERIOLOGY.

The work, begun in collaboration with Dr. J. K. Clarke, was continued, and can be summarized as follows:—

Cultures of the catheter urine showed bacilli of intestinal types as the sole or predominant organism present. The bacilli were usually of the coli-typhoid group,

2 Day: *Nephritis in Egypt*; Castellani: *Diseases of Central America*

sometimes *Bacillus pyocyaneus*. Pure cultures were generally obtained from acute cases. The urine of chronic cases of nephritis, which only showed granular or hyaline casts on microscopical examination, was usually sterile, as also was the urine from cases of uncomplicated syphilitic nephritis.

When pus was present, as commonly happens in old cases of bilharzial disease of the bladder, staphylococci were usually found, with or without bacilli. Streptococci were uncommon.

Injection of these bacilli into guinea-pigs and rats set up a tubular nephritis and also induced parallel changes in the liver. Typical *Bacillus coli* failed to reproduce the lesions.

Agglutination tests on patients proved unsatisfactory, even after the injection of vaccine.

TREATMENT.

Charts were shown illustrating the effects of special treatment on the theory of an intestinal origin of most cases. Removal of an intestinal focus of infection had an immediate favourable effect on a complicating nephritis; but was only possible in a minority of cases.

The importance of intestinal infection was evident on comparing the effects of complicating disorders that developed in several patients. Whereas chest complications had little effect on an existing nephritis, the onset of an acute dysentery or enteritis was most serious. Out of nine patients so affected four died in consequence. Vaccine therapy proved the best means of treatment in most cases of bacillary nephritis that were unresponsive to usual measures. In non-resolving cases with persistent cedema, doses of 20 to 50 millions caused diuresis with improvement in all respects. An autogenous vaccine was preferred, but similar results were often obtained by using vaccine prepared from other patients. It was found that failure might be due to using too large doses in some cases. Such patients could be recognized, and the special features demanding caution were one or more of the following: Fever decided oliguria or high albuminuria.

In treating such patients it was discovered that small doses of vaccine given by the mouth on an empty stomach were of striking benefit. Five to ten millions were sufficient, and the dose should only be cautiously increased. The effect was so immediate that it appeared to act as a de-sensitizing agent. This of course raises the question of a possible protein action apart from any direct immunizing effect. But it is the results which are more important than the adherence to any view or theory.

Observations on Some Diseases of Central America.

By ALDO CASTELLANI, C.M.G., M.D., F.R.C.P.

DURING my recent visit to Central America, which I undertook, together with several colleagues, at the invitation of the United Fruit Company of America, I had the opportunity, thanks to the kindness of the company's officers, of seeing a very large amount of most interesting clinical material, and in the brief sojourns at the various places we visited I was also given every facility to study certain cases in the magnificent hospitals and laboratories of the company, as well as in the well-equipped local government hospitals of Guatemala, Costa Rica and Panama. There was, of course, no time to carry out any investigation, but I was able to collect a certain amount of material, and to make a few observations which I venture to think may perhaps be of some slight interest to the Fellows, as several of the diseases found in Central America are not well known to us in Europe, though most of them have been thoroughly investigated locally.

I propose touching briefly on the following subjects:—

- (1) Guatemala nodules (Guatemala onchocerciasis).
- (2) Central American myiasis (El Torsalo, Gusano).
- (3) Tropical rhinoscleroma.
- (4) Alastrim.
- (5) Granuloma inguinale.
- (6) Dermal leishmaniasis of Central America.
- (7) A new type of mycotic stomatitis.
- (8) Pineapple estates pyosis.
- (9) Dermatitis papulosa nigra.
- (10) Central American mossy foot.
- (11) The red mite of Central America.

(1) GUATEMALA NODULES.

(Onchocerciasis of Guatemala.)

Historical.—For some years local practitioners, planters, and farmers of certain districts of Guatemala, on the Pacific slopes of the country, had noticed that a large number of the Indian labourers on the coffee plantations presented some peculiar nodules on the scalp. They had also noticed that these carriers of nodules often suffered from eye trouble, and that another condition was common in the same district, characterized by swelling of the face and fever, usually known locally as "*erisipela de la Costa*." These conditions apparently did not attract the attention of any investigator, and nothing was published, even from the mere clinical point of view, until R. Robles, in 1915, began his researches and elucidated the aetiology of the nodules by finding in them adult filariæ of the *Onchocerca volvulus* type and enormous numbers of microfilariae. Robles came also to the conclusion that the same filaria was the cause of the eye condition so often found in carriers of the nodules, and that it was also the cause of the erysipelatosus affection of the face known as "*erisipela de la Costa*" (erysipelas of the Coast). Robles sent some of his material to Brumpt, who confirmed his findings and created a new species for the filaria: *Onchocerca caecutiens*, Brumpt, 1919, the blinding filaria.

Robles' work was confirmed and greatly enlarged by Calderon, who carried out a large number of researches on the experimental reproduction of the nodules and the transmission of the filaria, and for this purpose lived for three years on a distant coffee plantation in which the disease was common. The more recent work of Fülleborn is also of great interest. He doubts whether the new species of filaria created by Brumpt is valid, as he sees no morphological differences between *Onchocerca caecutiens*, Brumpt, 1919, and *Onchocerca volvulus*, Leuckart, 1893; he does not believe that "*erisipela de la Costa*" and the eye condition are in any way connected with the nodules and the filaria.

While at Guatemala, thanks to the kindness of Dr. Calderon, and the Superintendent of the General Hospital of Guatemala City, I had the opportunity of seeing and studying eleven cases of nodules, most of them also presenting eye trouble. Dr. Calderon was good enough to allow me to take some photographs, and also very kindly gave me some of his own photographs and drawings and excised nodules. Professor Fülleborn and Dr. Zupuka, too, kindly gave me some material. I was also present at the operation of the removal of the nodules from three cases.

Geographical Distribution.—The geographical distribution of Guatemala nodules is extremely limited. The condition has been found, so far, only in a narrow strip of land on the Pacific slopes of Guatemala (districts of Sacatepiquez, Escuintla, Chimaltenango, Solola), at an altitude of 600 to 1,200 metres, and no cases have been reported from any other Central American country. Personally, I am inclined to believe that further investigation will reveal that the condition is not so limited, and

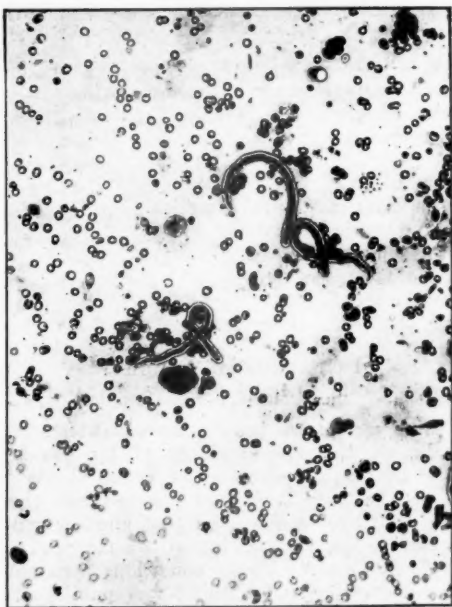


FIG. 1.—*Onchocerca caecutiens microfilaria*



FIG. 2.—Section of nodule showing *Onchocerca caecutiens microfilaria*.

is probably present in other Central American countries. Fulleborn states that a Mexican planter has described to him a nodular condition of the scalp which sounds very much like Guatemala nodules in certain valleys of Southern Mexico, especially in the valley de Custipeec, at an altitude between 800 and 1,400 metres.

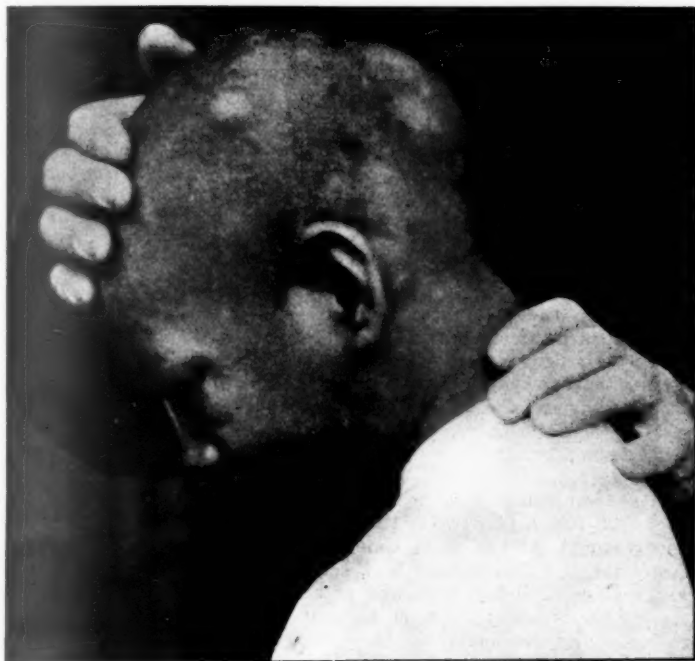


FIG. 3.—Guatemala nodules.

Etiology of Guatemala Nodules.—The examination of the nodules as found by Robles show adult filariæ of the onchocerca type and an enormous number of microfilariae. The morphological characters of the filariæ and of the microfilariae are identical with those of *Onchocerca volvulus*, Leuckart, of West Africa, but Brumpt has created a new species, *Onchocerca caecutiens*, basing it principally on the fact that the two filariæ give rise to two somewhat different clinical pictures: in African onchocerciasis the nodules are more frequently found on the body than on the scalp, whilst in Guatemala onchocerciasis the nodules are mostly found on the scalp. Moreover in the African onchocerciasis eye symptoms are rare or absent.

The average dimensions of the *Onchocerca caecutiens* are as follows: Male, length 24 to 42 mm., breadth 154 to 190 microns; female, length as much as 50 cm., breadth 300 microns; vulva situated about 800 microns from anterior end. Eggs oval. Brumpt has given a full description of the worm in the *Bull. Soc. Pathologie Exotique*, xii, 7, p. 464.

Clinical Symptoms.—In a well-marked case several nodules, each the size of a pea to a large nut, are seen on the scalp, though less frequently they may be found also on the body, especially on the sides of the chest close to the ribs. The nodules

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are deeply embedded in the subcutaneous tissues, and the skin over them is often movable. They have no tendency, as a rule, to spontaneous cure; very rarely they may become absorbed; in exceptional cases they become suppurated.

The Eye Symptoms.—A large number of carriers of nodules present severe eye-symptoms as observed by Robles and Calderon. These, however, according to Fülleborn and others, are not related in any way to the filarial infection. The eleven cases I saw at Guatemala City, nine of them under the care of Dr. Calderon, all showed in various degrees eye-symptoms which may be briefly described as follows: There is severe photophobia—the palpebral conjunctiva is very hyperæmic; there are often signs of keratitis, so-called keratitis punctata, and later diffuse keratitis. In a number of cases the pupils do not react either to light or accommodation, or only in a very sluggish manner. The fundi are generally normal.

Are the Eye Symptoms connected with the Filarial Infection?—As I have already stated, Fülleborn and others maintain that the eye-symptoms observed in so many carriers of nodules have nothing to do with the presence of the filarial nodules; they are observed, they state, in a number of natives not suffering from onchocerciasis. In my opinion they are connected with the nodules and the onchocerca. Dr. Calderon showed me a number of histories of cases from which it was clear that the eye-symptoms as a rule improved very quickly after removal of the nodules, and later on cleared up completely. It is on record that patients who were absolutely blind regained their sight perfectly ten days after the operation. Calderon is inclined to believe that the eye-symptoms are not caused by any mechanical action of the filaria, but by toxins secreted by it.

Erysipela de la Costa (Erysipelas of the Coast).—In the districts in which the nodules are common, a condition is very frequently observed characterized by the following symptoms: The patient, without practically any prodromata, suddenly develops a high temperature without rigors; the temperature very quickly rises and remains high (103-104° F.) for two or three days, when it rapidly falls to normal with little or no sweating. At the same time as the temperature rises, and while the temperature lasts, the skin on the face, scalp, and ears becomes œdematous, and rather hard, and occasionally, especially at the tip of the nose, a peculiar bluish-green discoloration is seen; there is no redness as in ordinary erysipelas. The œdema gradually disappears after the temperature has become normal, but the skin on the face, and especially on the forehead, often remains somewhat rough, occasionally slightly thickened, and there may be a very slight pityriasic desquamation.

Is Coast Erysipelas a Manifestation of Onchocerciasis?—Fülleborn and others consider that this condition is not connected with the filarial infection and the nodules. Personally, especially after seeing two of Dr. Calderon's patients who had just recovered from an attack of coastal erysipelas and had nodules on the scalp, I cannot help thinking that coastal erysipelas is a manifestation of the *Onchocerca caecutiens* infection. In fact, in my opinion, the nodules, the keratitis, and the coastal erysipelas are manifestations of the same condition—*Onchocerca caecutiens* infection.

Diagnosis.—In the districts in which the condition is endemic the diagnosis is extremely easy. In any doubtful case puncture of the nodules will clear up the diagnosis. It may be stated here that, as first observed by Calderon and Fülleborn, microfilariae are found in the subepidermal tissues of the skin in parts of the body which are situated far from the nodules; they are very rarely observed in the blood of the general circulation. This is, perhaps, the place to say a few words on Guatemala nodules in relation to African volvulus. *Are the two conditions the same? Are the two filariae identical?* Fülleborn considers the two filariae identical; Calderon in one of his papers, expresses the opinion that onchocerciasis may have been introduced into Guatemala by Jamaican blacks whose ancestors came from West Africa. It is to be noted, however, that the condition is absolutely unknown in

Jamaica. Personally I am inclined to believe that the two conditions, West African onchocerciasis and Guatemala onchocerciasis, although very similar are not identical. As regards the morphological identity or otherwise of *Onchocerca caecutiens* with *Onchocerca volvulus*, this is a subject to be investigated by helminthological experts; but I venture to remind you that many parasites which were considered to be identical even a few years ago have now been demonstrated to be different species.

Prognosis.—The nodules after reaching a certain size become quiescent, and the attacks of coastal erysipelas, too, after some time seem to cease, but the eye-symptoms become steadily worse, and in a number of cases total blindness ensues.

Transmission of Guatemala Nodules.—Robles suspected at first certain species of *Simulium*. Calderon's recent researches tend to incriminate a *Culex* mosquito.

Treatment.—This is surgical; enucleation of the nodules under local anaesthesia. Tartar emetic, salvarsan, and neosalvarsan have been given intravenously without any benefit whatever. Fülleborn has tried the intranodular injections of cocaine, but no definite results were achieved.

Prophylaxis.—Very little is known on this subject. If Dr. Calderon's researches are confirmed, anti-mosquito measures will be found useful.

In concluding this section of my paper, it is my pleasant duty to express my deep thanks to Dr. Calderon for his kindness in allowing me to examine a number of his patients and for giving me a large amount of material; my thanks are also due to Prof. Fülleborn for much valuable information given me, especially with regard to the morphology of the filaria and the literature on the subject.

(2) CENTRAL AMERICAN MYIASIS.

(El Torsalo, Cusano.)

Myiasis is extremely common in all Central American countries. Two types must be distinguished:

(1) *Primary Myiasis.* The fly-larva is the true cause of the condition.

(2) *Secondary Myiasis.* The fly-larva is a secondary invader.

The commonest form of myiasis met with is the "*furuncular*" or "boil-like" type. The condition is very painful, and at first looks like an ordinary large boil; later, at the apex, a small aperture is seen covered by a crust, on removal of which not rarely a moving body with two small brownish-yellowish spots (the posterior end of the larva) can be seen. If the condition is left to itself, in due course the larva dilates the opening by means of its posterior end and eventually escapes, and, falling to the ground, crawls away and becomes a pupa.

As regards the mode of infection, there is a consensus of opinion at the present time that the eggs of *Dermatobia hominis*, Linnaeus junior, 1781, better known as *Dermatobia cyaniventris*, Macquart, 1843, are carried by the female mosquito of the species *Janthinsoma lutzi*, Theobald. It would appear that the fly seizes the female mosquito and attaches her eggs to the ventral aspect of the abdomen; it has also been suggested that the fly deposits the eggs upon foliage, and these accidentally reach the mosquitoes.

The treatment is surgical: in the first stages opening of the boil and removal of the larva; in the later stages extraction of the larva by means of forceps.

For more details on Central American myiasis the reader is referred to the recent monographs and papers on the subject by Umana, Arias, Picato and others.

(3) TROPICAL RHINOSCLEROMA.

Rhinoscleroma is generally considered to be a disease of Eastern Europe, principally Austria, Poland and Russia. A few cases have been reported from other countries in the temperate zone and in the sub-tropics. It is extremely rare in the tropics, but there is one exception—Guatemala. For reasons unknown the condition

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is, comparatively speaking, quite common in that country, and at the General Hospital of Guatemala City I had the opportunity of seeing six cases.

Rhinoscleroma, as is well known, is a disease of the nose which begins insidiously with the formation of a painless pink or red nodule in the anterior nares or on the surface of the nose, or on the adjacent part of the upper lip. The characteristic feature of the nodule or nodules is the peculiar cartilaginous or stony hardness. The nodules after a time may coalesce into a large mass which often completely blocks up the anterior nares. The surface of the mass is smooth and tense, and tends to crack. The tumour has a tendency to recur after removal. The disease is believed to be caused by the bacillus of Frisch, which is a bacillus closely allied to Friedländer's pneumobacillus. With regard to the cases I saw in Guatemala City, in three of them an ulcerative process, clinically resembling leishmaniasis, was present, and in one verrucoid lesions were seen which, clinically, looked very much like blastomycosis. These ulcerative processes, however, were apparently secondary, and had developed on true rhinoscleroma, as certain portions of the masses were extremely hard, like bone, and such hardness is not a feature of simple blastomycosis or leishmaniasis. Unfortunately, there was no time to investigate the cases, and two slides I took from one of them were mislaid. The routine treatment of rhinoscleroma at the Guatemala City Hospital is by X-rays.

(4) ALASTRIM.

In Jamaica we saw a large number of cases of alastrim, and among the various colleagues who saw them the usual discussion took place: Is alastrim a synonym for smallpox, or is it a different disease? The majority, including myself, were inclined to come to the conclusion that alastrim and smallpox are two slightly different conditions. The eruptive elements can hardly be distinguished, but in practically all the cases we saw, even those with a most abundant eruption, the general condition of the patient was remarkably good, and we were told that secondary fever is generally absent. The mortality, in contrast to the high mortality of smallpox, was less than 1 per cent. It is known also that Jenner's vaccination may be successfully carried out shortly after an attack of alastrim. Remembering how many varieties of relapsing fevers there are, it seems to me hardly surprising that there should be at least two varieties of smallpox.

(5) GRANULOMA INGUINALE.

This condition is common all over Central America, especially in coloured people. Some very good cases were shown to us in the Porto Castillas Hospital. In most cases scrapings from the ulcers showed the presence of Donovan bodies. According to the experience of medical men practising in Central America, tartar emetic answers well in the treatment of this disease.

(6) DERMAL LEISHMANIASIS.

Three types of dermal leishmaniasis are seen in Central America in my experience:

- (1) The espundia type, affecting the nose, palate and throat.
- (2) The frambæial type, characterized by the presence of large nodules somewhat resembling yaws. These nodules may occasionally resemble blastomycosis. A subvariety of this is leishmaniasis of the lobe of the ear. The nodule ulcerates quickly, and the result is a large loss of substance.
- (3) The Oriental sore type. This is, clinically, absolutely identical with the condition so common in certain parts of Asia and Africa, which goes by the name of "Aleppo boil," "Delhi boil," "Biskra boil," &c.

It is interesting to note that the tartar emetic treatment answers much better in the espundia and frambæial types than in the Oriental sore type.

(7) A NEW TYPE OF MYCOTIC STOMATITIS.

In Honduras I saw two cases of a peculiar mycotic stomatitis, quite different clinically from ordinary thrush. The patches, which were of large dimensions, situated on the buccal mucosa and tongue, were much less raised than in thrush and not so creamy-white; they looked much more like a thin, translucent, whitish film covering parts of the mucosa. The first impression was that they were patches of incipient, diffuse leukoplakia of the smooth type. Definite ulcers were not present. I was able to make some scrapings and immediately inoculate a few glucose-agar tubes. The result of the mycological investigation has been rather interesting: it is apparently a true case of symbiosis of a bacillus with a rather delicate yeast-like fungus. So far I have not succeeded in separating the two by plating or otherwise. In contrast to the fungi-producing ordinary thrush, the colonies of this fungus + bacillus are extremely minute and delicate, somewhat resembling streptococcus colonies. The result of the investigation of these two organisms will be given in a further paper.

(8) PINEAPPLE ESTATE PYOSIS.

In the lower districts of Costa Rica in the pineapple plantations, a large number of labourers suffer from a type of pyosis affecting principally the hands and arms, and feet and legs. Numerous flattened pustules are seen, some of which break, and small, shallow ulcerations are formed. The hands and arms may be somewhat cedematous. There is little or no itching, and usually no fever. The condition heals under a simple antiseptic treatment within ten or twelve days.

As regards the aetiology, the usual pyogenic cocci are found in the pustules and ulcerated lesions; it is not scabies, as there are no burrows and the *Sarcoptes* has never been found. It does not seem to be primarily caused by some other mite, as is the case for instance with copra-itch, as pruritus is absent or only slight. But this point might be further investigated.

(9) DERMATITIS PAPULOSA NIGRA.

In Jamaica and Central America I have come across a peculiar dermatosis which does not seem to have been described previously. It is particularly common in natives of Jamaica. In a well marked case a large number of black or very dark brownish papules, somewhat cupoliform or flattened, are seen on the face, principally on both malar regions, while they are rare or absent on the lower parts of the face and chin; a few may be present on the forehead. They are not pruriginous and not painful. The maximum diameter of each papule varies between 1 mm. and 4 or 5 mm. At times two or three papules are very close together and seem to coalesce. The first papules appear in youth. I have seen them present in small numbers in boys and girls of 16 to 18 years of age. The condition gradually becomes more marked and the papules increase in number and size the older the patient becomes; and it is quite common to see middle-aged men and women with their faces studded with these black papules.

A full description of the lesions will be given later.

Diagnosis and Aetiology.—The papules are neither ordinary warts nor senile warts, lacking the verrucose surface of the former and the greasy appearance of the latter. The aetiology is unknown.

Treatment.—This is very unsatisfactory. The papules may be destroyed by using the micro-burner, or a strong solution of formalin, but small white marks, as a rule, remain permanently, and to this the coloured people object more than to the black papules.

(10) CENTRAL AMERICAN MOSSY FOOT.

In Costa Rica and Guatemala several medical men informed me that there exists there a peculiar granulomatous condition of the foot which was considered by some

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to be a type of Madura foot, by others a malady *sui generis*. At Port Lemon I had the opportunity of seeing a case; parts of the dorsum of the foot and toes were covered with granulomatous nodules and masses covered by yellowish dirty crusts. On removal of these the nodules looked very much like the nodules of yaws, but more diffuse, with a granulomatous or papulomatous red surface, exuding a thin secretion, which later on thickens into a crust. The Wassermann reaction was negative. I identified the case with the "mossy foot" described in Brazil by Thomas. The condition must be distinguished principally from Madura foot, yaws and verruga Peruviana. In Madura foot there are sinuses discharging a purulent secretion



FIG. 4.—Central American Mossy Foot.

containing sclerotia; both sinuses and sclerotia are absent in mossy foot. In contrast to yaws the condition is found only on the foot and occasionally the legs, but on no other part of the body. With regard to the aetiology nothing is known. The treatment is most unsatisfactory, tartar emetic, salvarsan and neosalvarsan having no effect whatever; the administration of potass. iodide and mercury is also ineffective. I suggested that X-rays might be tried.

Histology.—This has been investigated with the kind help of Dr. Mackenzie Douglas. The corium is oedematous at places, at other places there is infiltration with lymphocytes and epithelioid cells, closely resembling the appearance seen in granuloma. Here and there giant-cells are surrounded by epithelioid cells and small round cells, as seen in typical tubercular follicles. No *Bacillus tuberculosis* could be found however in sections stained by the Ziehl-Neelsen method.

(11) THE RED MITE OF CENTRAL AMERICA.

All over Tropical America and principally in certain countries of Central America, Honduras and Panama, red mites which attack man, small mammals and birds are extremely common. They are known by various names: in Mexico the red mite found there is called *Tlalsahuate*, in the Guianas a similar mite is called the *balata* or *pou d'Agoute*; in New Granada there is the *naibi*; in Cuba the *colorado*; in Martinique the *bête rouge*. In the Argentine and Uruguay the *Tetranychus molestissimus* Weyenberg 1886 is a great pest; it differs zoologically from the above-mentioned mites belonging to the family *Tetranychidæ*; it lives in a web on the inferior surface of the leaves of *Xanthum macrocarpum* and attacks man and animals

from December to February. Very little is really known of these various mites, and the whole subject in my opinion requires revision. I will limit myself to saying a few words on the red mite of Panama and Honduras. While there recently I collected a number of specimens of the red mite and sent them to Dr. Hirst for identification. He has kindly informed me that the mite is *Trombicula tlalzahuatl* figured under the name *Leptus* (*Trombicula*?) *similis* in his "Mites Injurious to Domestic Animals."

Climatology.—In Central America the red mite is met with principally in the low country, especially the districts near the coast, but it may also be found, though apparently not in such large numbers, in the hills at a fairly high altitude.

Red Mite Dermatitis.—The mites give rise to an extremely itchy, papuloid eruption, the papules being red and at first rather scattered. True pomphi are not present. In a later stage, owing to scratching and a secondary invasion by staphylococci, a form of pyosis or impetigo may develop.

Experimental Reproduction of the Dermatitis.—If a red mite is picked up, with a toothpick or similar device, and placed on the forearm of the volunteer it will be seen that the little creature moves quickly here and there and then stops and begins burying itself partially into the skin, and then the patient begins to experience a very severe local pruritus. It is said that the mite buries itself completely into the skin, but this is not my experience; the burying is apparently only partial and after a time the mite drops off and a papule appears at the place where the mite was.

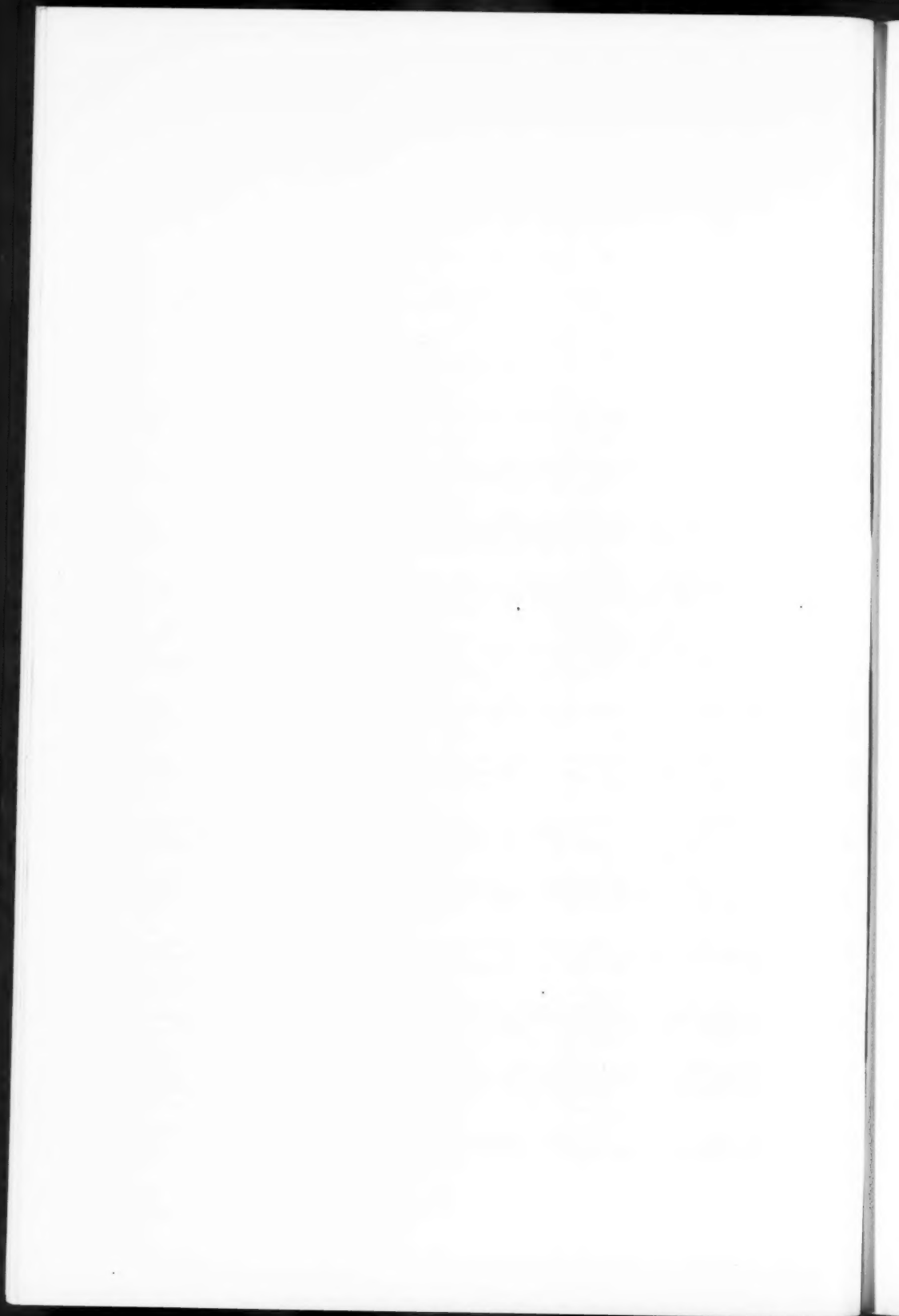
Lower Animals.—The red mite attacks many other mammals besides man—dogs, cats, and rabbits, for instance, but apparently it manifests a predilection for fowls and chicken. At Almarantes I have seen a number of chicken with portions of their bodies covered with red specks (the mites), and showing signs of severe sickness with loss of feathers over those areas where the mites were more abundant. Numbers of young chicken are actually killed by these pests.

Treatment.—The usual treatment in Central America is sulphur ointment and tincture of iodine. What I found most useful to allay the pruritus were carbolie and menthol lotions, especially my old prickly heat lotion, which consists of:

Ac. salicy.	gr. ij.
Menthol	gr. x.
Spir. rect.	ʒij.
Glycerin	℥xxx.
Zci. ox.	ʒiij.
Calam.	ʒiij.
Aq. rose ad	ʒvj.

Prevention.—This consists in wearing high boots, but at times this precaution is not sufficient. It is interesting to see how at Almarantes and other places in Central America, even after a short walk in the grass, the boots get covered with red mites, some of which go upwards and get inside from the top. Sulphur powder is considered to be a preventive.

In conclusion I should like to express my gratitude to the authorities of the United Fruit Company for their kind invitation to visit their stations in Central America. My thanks are specially due to Dr. E. Deeks, their medical manager. I desire to express my obligation to all the medical men connected with the United Fruit Company's hospital as well as to the local Government Hospital for their unfailing courtesy and for their generosity in placing at my disposal a large amount of clinical material. My thanks are also due to Dr. Mackenzie Douglas for much assistance rendered in the histological investigation of the pathological material.



Section of Tropical Diseases and Parasitology.

President—Professor LEONARD S. DUDGEON, C.M.G., C.B.E., F.R.C.P.

Edible and Poisonous Fungi.

By J. RAMSBOTTOM, O.B.E., M.A.

THE edible or poisonous qualities of fungi are not the subject of the same interest in this country as they were to the ancients or to our more immediate ancestors, and probably most of us never partake of any fungus but the common mushroom except on the Continent, when a wild gastronomic adventure might include cèpes or escargots.

In classical times there was considerable interest taken in the edibility of fungi. Many instructions were given how to distinguish those which were safe from those which were not; and minute descriptions of the manner of preparing them for the table and for treating the guests in case of mishap. An often-quoted epigram of Martial that gold and silver and dresses might be trusted to a messenger but not boleti, indicates the esteem with which certain esculents were regarded.

The general public considers the common field mushroom and the coarse horse mushroom as the only species fit to eat. That other forms can also be eaten is generally known, though it is usually imagined that most toadstools are extremely poisonous. In any event the non-poisonous species are not thought worthy of any attention. It is remarkable that such beliefs should be prevalent when the facts are that few toadstools are poisonous and many of the edible ones are of much more delicate flavour than the common mushroom, either wild or cultivated. How are such edible species to be recognized? The several species are distinguished by their botanical characters: whether they are edible or poisonous is known only by experience. Fortunately the list of well-known esculents is sufficiently extended to warrant even the most confirmed mycophagist refusing to embark upon preliminary experiments.

All rule-of-thumb methods for distinguishing between edible and poisonous fungi are utterly useless. It is a matter of surprise that newspapers, which in these days publish many well-informed articles on various branches of natural history, should litter their pages every autumn with worthless notes on how poisonous and edible fungi may be distinguished. It is true that we are seldom, if ever, now told to avoid those species which grow near serpent's holes or near rusty nails but much of the remaining advice is the same as is to be found in Pliny. Although such "rules" are current in every nation they are worthless.

(1) It is often stated that poisonous fungi display one or other of certain characters. Nothing whatever can be told from their appearance though to "look poisonous" is often sufficient to repel the potential mycophagist. Viscidity and colour of exterior, colour of flesh or change in colour on breaking, odour, taste, exudation of a milky fluid and so on, are of no special significance in this connexion. All may be of use in the identification of a given specimen but take one no further.

(2) Reliance is sometimes placed on the fact that a species is nibbled by rabbits or squirrels, or is slug-eaten. It is not uncommon to find *Amanita phalloides*, the most poisonous fungus known, nibbled by rodents, and slugs seem to have a great liking for it.

(3) The blackening of a silver coin or spoon is probably the most widely believed "tip" for recognizing a poisonous fungus. *Amanita phalloides* does not blacken silver and, moreover, the pellicle can be peeled from the cap—a sure sign of edibility

to the majority of people. The coagulation of milk or the turning brown or blue of onions placed in a stew with fungi are also assumed characters for poisonous fungi—of equal value.

(4) A common recommendation is that fungi growing on highly manured ground should be avoided and this in spite of the ordinary knowledge of mushroom culture.

Many other similar beliefs have a local following, but in this country, judging from results, it would appear that their efficacy is seldom tried. Whether "seeing them in black and white" in the newspapers will add courage to these beliefs remains doubtful.

The rules advanced by mycologists are often really little better than some of these. Species with a volva, or with pink spores, or which grow in woods are pilloried as those to be avoided. Such rules are to be deprecated not because they lead to the neglect of many well-known esculents, but on account of the false air of security they provide. As there are so few poisonous species the sane way would be to learn to recognize them—or to restrict oneself to the edible species one knows, even if such restriction should confine one to mushrooms from the markets. Covent Garden in our day offers nothing beyond *Psalliota campestris*, but in former times the parasol mushroom (*Lepiota procera*), blewits or bluelegs (*Tricholoma personatum*, *Tricholoma nudum*), chanterelle (*Cantharellus cibarius*) and Sussex and Wiltshire truffles (*Tuber aestivum*) were offered for sale. On the Continent certain markets restrict the sale to specified fungi but others allow any species to be sold after an inspector has certified them as edible.

The question of the food value of fungi was raised in many European countries both during and after the war. Fungi resemble green vegetables in the amount of water they contain (80 to 90 per cent.). There is also from 2 to 5 per cent. proteid nitrogen but a proportion of this is indigestible. Carbohydrates occur as fungus cellulose (about 3 per cent.), sugars (trehalose about 4 per cent., mannite about 1 per cent.), and glycogen. About 1 per cent. of lecithin is present and there is a similar amount of mineral matter. We know that fungi are a complete food from the fact that one or two experimenters have existed solely upon them; also in certain barren countries, or where the inhabitants are of low intelligence, fungi may form the only food, as for example in Fuegia, where the natives subsist on *Cyttaria*. To purchase fungi for their food value would hardly be economical: they are best regarded as appetizers.

The poisonous effects of fungi are of various kinds. First there is *natural idiosyncrasy*, many people being unable to eat fungi of any kind without discomfort. This is analogous to the well known inability to eat strawberries, eggs, &c.

Secondly there is *simple indigestion*. Fungi are somewhat indigestible and portions pass through a perfectly healthy digestive apparatus practically unchanged. Quite wholesome fungi sometimes cause indigestion in cases where there is no defect in the digestive powers. Over-indulgence and faulty cooking are, however, responsible for most of the trouble. A heavy meal of fungi should not be taken when one is fatigued after a long walk, and should not be eaten unaccompanied by other food.

Thirdly, *poisoning by old and altered fungi* is sometimes encountered. Fungi should be eaten fresh, though many of the tougher forms like the morel or the fairy ring champignon may be dried for future use. As in the case of all organic substances changes are soon brought about by the activities of putrefactive bacteria and moulds. The poisoning resulting from eating partly decayed fungi is of similar nature to ptomaine poisoning.

Finally, there is *specific poisoning*. Roch's classification of fungi which contain specific poisons is as follows:—

(1) Those containing substances which excite the muscular apparatus, especially smooth muscular fibres (uterus, vessels, &c.), e.g., *Claviceps purpurea* (ergot).

(2) Those containing hæmolytic substances: e.g., *Gyromitra esculenta*.

(3) Those containing irritant principles which bring about gastro-enteritis by direct action on the mucous lining of the intestines: e.g., acrid species of *Russula* and *Lactarius*, *Entoloma lividum*.

(4) Those containing a substance which paralyses the central nervous system: e.g., *Amanita muscaria*.

(5) Those which, after a long incubation period, bring about the degeneration of the cells of the organism, particularly those of the nervous system and glandular parenchymatous cells, especially of the liver: e.g., *Amanita phalloides*.

The poisons of the first three groups appear to be eliminated during cooking and sometimes even by drying.

From earliest times interest has been taken in neutralizing fungus poisons by special methods of cooking. Pliny suggests that they should be cooked with meat or with pear stalks, and that vinegar neutralizes their dangerous qualities. Gérard, in the fifties of the last century, found that by washing poisonous fungi, leaving them for two hours in water to which had been added a small amount of vinegar, then, after washing and boiling them for half an hour in fresh water, they were rendered perfectly innocuous—needless to say the water used is thrown away. A similar method is described by Fabre in "The Life of the Fly." The peasants of Sérignan and its neighbourhood regularly eat many poisonous and doubtful species, and no one seems to have heard of a case of mushroom poisoning. The method adopted is to "blanch" the mushrooms, that is to say, to bring them to the boil in water with a little salt in it. A few rinsings in cold water conclude the treatment. French mycologists do not favour the method.

The fungi of which I have been speaking, and of which lantern slides will be shown, are all of a size of which the common mushroom can be regarded as an average—a meal of microfungi being out of the question. These larger fungi belong to the great classes Basidiomycetes and Ascomycetes. So far as I am aware no Phycomycetes and no member of the Fungi Imperfecti reach any appreciable size.

Basidiomycetes, which include mushrooms, toadstools, bracket fungi, fairy clubs and so on, are characterized by the fact that their spores are borne on the exterior of a structure known as a basidium, which is usually clavate and has four projections, to which the spores are attached and from which they are violently shot off when they attain maturity.

Ascomycetes, which include cup-fungi, ergot, yeasts, &c., on the other hand, have their spores formed within somewhat similar clavate structures, asci, to the number of eight.

A few words should be said about the spores. These correspond to the seeds of flowering plants, but differ from them in not containing an embryo. In the forms we are considering they are thin-walled, globose or elliptical, with hyaline protoplasm and not infrequently oil as reserve food material. The spore on germination gives rise to the mycelium (spawn). The details of sexuality and development are botanically exceedingly interesting, but would carry us out of our way.

Probably truffles are the best known edible fungi amongst the Ascomycetes. All are subterranean, though some are not deeply buried. They occur principally in calcareous soils, usually associated with the roots of trees. Truffle-grounds are generally poor in undergrowth; the best collecting places are at the edge of woods. Truffles have been prized as esculents from classical times; at that period a widespread opinion was that they were formed during thunderstorms.

As truffles are usually hidden under leaf mould or earth, often to a foot underground, there is considerable difficulty in finding them. Certain animals are, therefore, used for their detection, which they do by scent. Where truffles are not very plentiful dogs are used for hunting them; where they are abundant a sow is more usually employed. The most valued species is the Périgord truffle (*Tuber melanospermum*), which, so far, has not been found in this country. Our

most frequent species is *Tuber aestivum*, which was formerly hunted extensively in Sussex and Wiltshire by means of mongrel terriers and sold in Covent Garden. There is still a small amount of hunting done.

It should be noted that there are also underground genera in the Basidiomycetes which somewhat resemble *Tuber*. The so-called "red truffle," formerly sold in Bath market, is a variety of *Melanogaster variegatus*. The two British species of *Rhizopogon*, *Rhizopogon rubescens* and *Rhizopogon luteolus*, are also edible.

Other well-known esculents among Ascomycetes are species of *Morchella* and



FIG. 1.—*Morchella esculenta* Linn.



FIG. 2.—*Helvella crispa* Fr.

Helvella. The best-known species of *Morchella* is *Morchella esculenta* (fig. 1), which occurs in spring in the clearings of woods, though all the species are edible. Most frequently morels are used for flavouring soups, sauces, gravies, and in ketchups: they are previously dried threaded on strings.

Most species of *Helvella* are known to be edible, *Helvella crispa* (fig. 2) and

Helvella elastica being most sought after. It is probably advisable to throw away the water in which they are cooked, as occasionally those having partaken of this have complained of its effects, which may be due to helvellic acid. When *Gyromitra esculenta* is cooked—*Gyromitra* is a genus closely allied to *Morchella* and *Helvella* and with its spore-bearing layer covering brain-like ribs or folds—the water should always be thrown away as, in spite of its specific name, the fungus has a bad reputation in Germany, though it would seem that the trouble has always been caused by children eating it raw.

The large cup-shaped fungi of our woods have a clean record so far as I know, but as even the largest species, such as *Peziza aurantia* and *Acetabula vulgaris*, have very little substance there would be more patience required in their collection than risk run in eating them.

It is amongst Basidiomycetes that we meet with the principal edible and poisonous forms. In order to appreciate the lantern slides it is necessary to understand a little about the classification of this group. As modern classification depends to a great extent on microscopic characters it is not so convenient for our present purpose as the one which was generally used in this country until a year or two ago. In this, Basidiomycetes are divided into two orders, Hymenomycetes and Gasteromycetes, according as to whether the spore-bearing surface (hymenium) is exposed either from its beginning or during its development, or enclosed until maturity by a covering of sterile hyphae; mushrooms are typical examples of Hymenomycetes, and puff-balls of Gasteromycetes. Different families of Hymenomycetes are distinguished by the arrangement of the spore-bearing surface. In the Agaricaceae the hymenium is spread over the surface of radiating gills, whereas in the Polyporaceae it lines the inside of tubular structures or of reticulations; it is spread over spines or protuberances in the Hydnaceae and over smooth erect club-shaped sporophores in Clavariaceae; the Tremellineae are gelatinous with a smooth hymenial surface; the Thelephoraceae have a smooth or slightly rugose hymenial surface and usually a leathery consistency.

The Agaricaceae comprise the mushrooms and toadstools. In order to ascertain the genus to which a fungus belongs it is necessary to know the colour of the spores. This can easily be obtained by laying the cap face downwards on a piece of paper or glass. Spore colour viewed in mass is white, pink, brown, purple or black. In addition to the spores the presence or absence of a ring and a volva has to be noted (fig. 1A). The mode of attachment of the gills is also important. When they do not touch the stem they are said to be *free* (fig. 2A); when they just reach it, *adnexed*; *adnate* when they are slightly attached; *sinuate* when they show a slight curve or sinus near the stem and *decurrent* when they run down the stem. Thus an agaric with white spores and gills either free or adnexed, and possessing both a ring and a volva belongs to the genus *Amanita*. This is a most important genus to recognize because it contains most of the poisonous fungi.

The most notorious of these is *Amanita phalloides* (fig. 3). The cap in this species has a range in colour from yellowish-olive to whitish and is satiny when dry. Its stem is whitish, often with a tinge of green; at its middle there is a reflexed ring and at its bulbous base a large membranous volva usually more or less buried in the soil. This species occurs in woods and adjoining pastures from spring to autumn and is very common. It is the most poisonous fungus known and it is responsible for more than 90 per cent. of the deaths caused by fungi: surprisingly small quantities often bring about fatal consequences. The recorded cases of poisoning by this fungus during the last half-century show between 50 and 60 per cent. fatalities. It may be said that in cases of fungus poisoning, if *Amanita phalloides* and its near allies can be ruled out of account, the chance of recovery is almost certain, for no other fungus causes the death of a healthy person. From the extreme danger of this species it is advisable that its distinguishing characters should be impressed upon everyone

undertaking gastronomic experiments with fungi. The clinical symptoms are practically always the same. "After ingestion there is an incubation period of about twelve hours (8-40), during which little or no discomfort is felt. This is followed by a sudden seizure of intense, increasing, abdominal cramp-like pains

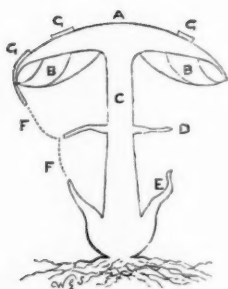


FIG. 1A.—Section of *Amanita phalloides* Secr.: (a) cap (pileus); (b) gills (lamellae); (c) stem (stipe); (d) ring (annulus); (e) volva.

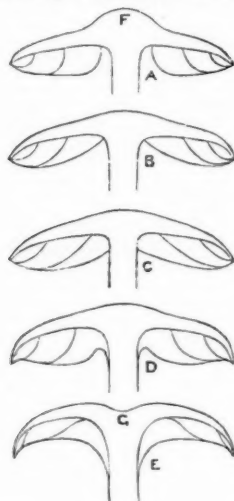


FIG. 2A.—Attachment of gills: (a) free; (b) adnexed; (c) adnate; (d) sinuate; (e) decurrent.



FIG. 3.—*Amanita phalloides* Secr.

accompanied by violent vomiting and diarrhoea of undigested food with blood and mucus, later becoming cholera-like. Thirst is very characteristic, incessant demands being made for drink, which, however, cannot be retained. If very little fungus has been eaten the symptoms may begin to abate after two or three days, but recovery

is very slow. Usually, however, violent diarrhoea continues with short periods of intermittence. Loss of weight is very great and the circulation is feeble; there is cyanosis with chilling of the extremities. The kidneys cease to secrete and muscular cramp occurs, especially in the calves. Usually the victim retains his lucidity. At the end of two days, sometimes a little later, there is an amelioration of symptoms—vomiting ceases, colic becomes less frequent, and the patient becomes apathetic and somnolent. This is, however, the most dangerous period. The symptoms soon reappear with intensity, blood is passed and most frequently the nervous system becomes paralysed by degrees, ending in collapse. In the majority of cases death occurs on the third or fourth day. If the period be passed jaundice frequently occurs, indicating fatty degeneration of the cells of the liver, a state resembling that brought about by phosphorus poisoning. In those cases where the patient recovers convalescence is very long."

No antidotal drug for *Amanita phalloides* toxin is known and the treatment usually followed is that for poisoning and septic intoxication in general. Several investigators have succeeded in immunizing animals to the aqueous extract of *Amanita phalloides*, but the attempt to prepare a curative serum has not yet been successful.

The poison concerned has been the subject of much investigation. Kobert (1891) found that extracts of *Amanita phalloides* contain a substance which attacks the red blood corpuscles of man and many animals. This blood-dissolving hæmolsin, *phallin*, was announced to be the active principle concerned. Ford (1906), however, found that this hæmolsin—an easily decomposed glucoside, insoluble in alcohol—is rapidly destroyed at a much less heat than that usually used in cooking and that digestive juices normally break it up. Further, the symptoms of *Amanita phalloides* poisoning are not those of hæmolysis. In addition to hæmolsin Ford found that there is also present a heat-resistant body, which produces in animals the majority of the lesions described in fatal cases of *Amanita phalloides* in man. This "amanita-toxin," an alcohol-soluble active principle, is the essential poison, resisting the action of heat, of drying, and of the digestive juices.

The records of poisoning attributed to *Amanita phalloides* no doubt include those due to two very closely allied species which are sometimes regarded as varieties of *Amanita phalloides*, viz., *Amanita verna* and *Amanita virosa*. Another species which has a very bad reputation is *Amanita mappa*, though it should be stated that evidence is accumulating to the effect that it is not poisonous. The older records may have been of *Amanita phalloides* poisoning, as the two species are sometimes a little difficult to distinguish. *Amanita mappa* never has the olive tinge typically present in *Amanita phalloides*; the cap is usually covered with patch-like fragments of the volva, and there is a groove between the bulbous base of the stem and the thick free margin of the volva.

Probably the best-known poisonous fungus we have is the fly agaric, *Amanita muscaria* (fig. 4), which has a bright scarlet cap with thick white spots and usually grows under birch trees. The appearance of this fungus deters older people from eating it, and further, unlike *Amanita phalloides*, it is bitter and unpleasant in taste. In spite of its popular reputation it is not deadly poisonous. The symptoms in poisoning by this species appear normally one to four hours after digestion. Sometimes the illness is not at all serious, merely colic, vomiting and diarrhoea; and recovery is rapid. At times it takes the form of hallucination, simulating alcoholic intoxication followed fairly rapidly by a deep coma. The gastro-intestinal type is usually accompanied by delirium, loss of memory, convulsions, or prostration, with a tendency to sleep. Recovery is rapid. Apparently the fungus does not cause the death of healthy individuals. Formerly atropine was used as an antidote to *muscarine*. Modern treatment consists in emetics and purgatives with chloral or potassium bromide to allay the delirium. The poison of this fungus has been investigated probably

more than even that of *Amanita phalloides*. Schmiedeburg and Koppe (1869) extracted a substance which they called *muscarine*; at first regarded as an alkaloid but probably a complex ammonia derivative, myceto-muscarine is very active and has well-marked effects, but these, however, are not those characteristic of *Amanita muscaria* poisoning. Further, there is little of it present. An additional fact is that muscarine does not kill flies whereas the fly agaric (broken up in milk) has been used for this purpose since the Middle Ages. At least two other bodies are present, and their different properties possibly bring about the difference in symptoms: myceto-atropine—the effects of atropine on the cerebral nervous system are identical with those produced after ingestion of *Amanita muscaria*—and a large amount of choline which (probably with several other related substances and resinoid bodies) is responsible for the gastro-intestinal disturbances. The way in which many Siberian tribes use an extract of the dried cap of this fungus as a stimulant was described by Krashmannikoff in 1733 and by many later travellers. The habitual use shatters the nervous system and the sale of the fungus by traders was made a penal offence under Russian law. Fungus intoxication enters into religious ceremonies and apparently is regarded as being sufficient to account for any crime and to ensure immunity from retribution. An interesting fresco was described a few years ago from a ruined chapel at Plaincourault (France), dating from 1291, representing the fall of man (fig. 5). There is the traditional serpent twined round the tree of good and evil offering an apple. The "tree" is a branched *Amanita muscaria* and Eve apparently has eaten of the fruit, though Boudier, who described the fresco, suggests that she appears to be suffering from colic rather than shame.

Amongst the other species of *Amanita* which are poisonous is *Amanita pantherina* (fig. 6) which has a brownish cap with numerous white, fairly persistent warts, and a striate margin. The effects brought about by eating this fungus are similar to, but more severe than, those of *Amanita muscaria*. This species, or a closely allied one, is said to be used in Japan for killing flies.

There are other rarer species of *Amanita* which are poisonous, but there are also several which are edible. The two commonest of these are *Amanita rubescens* (fig. 7) and *Amanita spissa*.

Apart from species of *Amanita* there are few fungi that are really dangerous. Indiscriminate eating of toadstools is not however a pastime to be indulged in as the recent experience of certain of His Majesty's forces with *Inocybe incarnata* clearly shows. (See *Journ. Roy. Army Med. Corps*, Jan. 1925.)

The most important genera from a gastronomic standpoint are as follows:—

AGARICACEÆ.

(1) WHITE SPORES.

Amanitopsis: differs from *Amanita* only in having a ring. The four British species are edible, the most valued being *Amanitopsis fulva*.

Lepiota: with ring but no volva; gills free, cap usually scaly. The large species, usually known as parasol mushrooms, *Lepiota procera* (fig. 8), *Lepiota rachodes*, &c., are all esculent. A few of the species of smaller size, e.g., *Lepiota helveola* and *Lepiota carcharias* are said to be suspicious but are probably quite safe.

Armillaria: with ring but no volva; gills adnate or slightly decurrent. None of the fifteen British species is known to be poisonous though the majority are too tough for the taste of most. The very abundant *Armillaria mellea* is frequently eaten on the Continent. This species, incidentally, is our worst tree parasite, and infected wood is often luminous.

Tricholoma: gills sinuate. About one hundred British species. A large number are known to be edible, a few are labelled suspicious, and *Tricholoma tigrinum* is known definitely to be poisonous. This species has a pallid brown to violet grey cap with darker crowded spots and a white involute margin. The incubation period is one to two hours. After a period of stomach pains, nausea and chill, there is abundant and repeated vomiting and diarrhoea with headache and cramp in the calves. The victim is unable to retain any nourishment of any kind. There is complete recovery after from two to six days. The best known

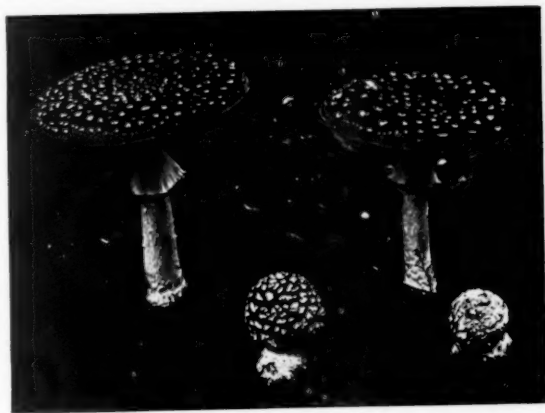


FIG. 4.—*Amanita muscaria* Pers



FIG. 5.—Plaincourault Fresco.



FIG. 6.—*Amanita pantherina* Secr.

edible species are the St. George's mushroom, *Tricholoma gambosum* and blue-leg, blewitts or bluetts, *Tricholoma personatum* and its allies.

Clitocybe: gills decurrent. About eighty British species, probably all of which are edible. *Clitocybe nebularis* is the best of the edible species.

Collybia: characterized by a cartilaginous stem and an involute edge to the cap when young. About eighty British species, probably none of which are poisonous.

Pleurotus: cap usually excentric. About forty British species none of which appears to be poisonous. *Pleurotus ostreatus* is the best known of the edible species.



FIG. 7.—*Amanita rubescens* Pers.



FIG. 8.—*Lepiota procera* Quél.

Paxillus: gills readily separating from the cap. About fifteen British species, mostly edible.

Hygrophorus: gills waxy in appearance. About seventy species, mostly edible, probably none poisonous. *Hygrophorus pratensis* is the best known of the edible species.

Lactarius: all parts of the fungus exude a white or coloured "milk" when broken. About seventy British species. All mild forms are reputed to be edible; probably the acrid forms

only cause discomfort when insufficiently cooked. *Lactarius deliciosus* is the best of the edible species.

Russula: gills fragile, with acute edge and vesicular trama. About sixty British species. No mild species is poisonous; the acrid species are usually avoided but are probably harmless after careful cooking.

Cantharellus: gills decurrent, thick fold-like. About twenty British species. Several are edible, the best known being the chantarelle, *Cantharellus cibarius*.

Marasmius: leathery. Of the fifty or so British species some are edible, but many are used only for flavouring. The fairy ring champignon, *Marasmius oreades*, is a well-known esculent.

Lentinus: leathery; gills decurrent, with serrated edges. About twelve British species. *Lentinus cochleatus* is edible.

(2) PINK SPORES.

Volvaria: with volva but no ring. About ten British species. The larger forms, *Volvaria volvacea*, *Volvaria bombycina*, *Volvaria speciosa*, and *Volvaria gloiocephala*, are all usually regarded with suspicion, though recently many French mycologists have eaten them with no ill-effect.



FIG. 9.—*Entoloma lividum* QuéL.

Pluteus: gills free. About fifteen British species. *Pluteus cervinus*, common on stumps, is the only one definitely known to be edible.

Entoloma: gills sinuate. About thirty British species. Most of these are open to suspicion. *Entoloma lividum*, "le grand empoisonneur de la Côte d'Or," is one of the best known poisonous species (fig. 9). The cap is livid tan, the stem shining white with a pruinose apex and the gills whitish, then flesh colour. The fungus smells of new meal. The effects of poisoning by this fungus are very similar to those of *Tricholoma tigrinum*. Diarrhœa sometimes lasts for four to five days. There is often pupillary trouble and a period of syncope. There is great thirst, the throat being so dry that it is impossible to speak. Illness lasts from three to six days and except in very exceptional cases recovery is complete.

Clitopilus: gills decurrent. About twelve British species, none known to be poisonous. The best known species is *Clitopilus prunulus*.

(3) BROWN SPORES.

Pholiota: ring on stem. About forty British species, none known to be poisonous, but mostly tough.

Inocybe: cap minutely scaly or fibrillose-scaly. Over fifty British species; none regarded as edible, and several, e.g. *Inocybe incarnata*, known to be poisonous.

Hebeloma: gills sinuate. About thirty British species, none known to be edible.

Cortinarius: arachnoid cortina connects edge of cap with stem in young stage. There are over two hundred British species, many of which are small. None of the larger forms is known to be poisonous, though only a few are esculent.

(4) PURPLE SPORES.

Psalliota: stem with ring; gills free, whitish, then often pink and finally deep brown. About twenty British species. All are probably edible though *Psalliota xanthoderma* and *Psalliota silvicola* cause discomfort to some people. *Psalliota arvensis* is the horse mushroom, and *Psalliota campestris* the field mushroom (fig. 10). *Psalliota campestris* has been cultivated for about two centuries and so far as can be ascertained its culture began in France. The only other fungus which is cultivated on a large scale is the Japanese Shiitake (*Collybia shiitake*.)

Stropharia: with membranous or fibrillose ring; gills adnate. About twenty British species, none known to be edible.

Hypholoma: gills sinuate; ring composed of hairs. About twenty-five British species only two or three of which are known to be edible.

(5) BLACK SPORES.

Coprinus: cap "deliquescent." About fifty British species. All the larger species are apparently edible, *Coprinus comatus* and *Coprinus atramentarius* being the best known.



FIG. 10.—*Psalliota campestris* Quél.

Gomphidius: gills mucilaginous, decurrent. Four British species, all of which are edible.

POLYPORACEÆ.

Boletus: cap smooth, tubes coherent. About seventy British species probably all of which are edible if eaten young. *Boletus edulis* (cèpe) (fig. 11), *Boletus reticulatus*, *Boletus pinicola* and *Boletus scaber* are the best flavoured. *Boletus luridus* and *Boletus satanas*, which were formerly regarded as poisonous, are probably quite wholesome.

Fistulina: tubes free. *Fistulina hepatica*, the liver or beefsteak fungus, is the only British species and is best eaten when mature as it is then more tender and less acrid.

Polyporus. One or two of the less leathery bracket fungi such as *Polyporus squamosus* have been eaten, but "taste like saddle-flaps" is not a very enticing invitation to sample any of them.

HYDNACEÆ.

Hydnum. The larger species of this genus with the possible exception of *Hydnum acre* are all edible. The best of them are *Hydnum repandum*, *Hydnum imbricatum*, *Hydnum coralloideum* and *Hydnum erinaceum*.

Tremellodon: gelatinous. The single British species, *Tremellodon gelatinosum* is edible.

THELEPHORACEÆ.

Craterellus: like *Cantharellus*, but the hymenial surface is rugulose. The six British species are edible, the best known being the black, sordid-looking "horn-of-plenty," *Craterellus cornucopioides*, which was formerly sold in Covent Garden.

CLAVARIACEÆ.

Sparassis: much-branched fruit body looking somewhat like a cauliflower. The single British species *Sparassis crispa* (including *Sparassis laminosa*) is esculent (fig. 12).



FIG. 11.—*Boletus edulis* Bull.

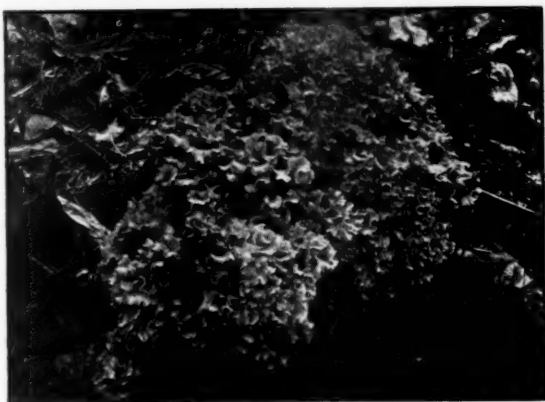


FIG. 12.—*Sparassis crispa* Fr.

Clavaria. Fairy clubs. About forty British species, probably none poisonous.

TREMELLINACEÆ.

Hirneola. The Jew's Ear (*Hirneola Auricula-Judæ*) is edible, but more to the taste of the Oriental than to that of the Briton as it gives "a tactile sense to the mastication." An allied species *Hirneola polytricha* is largely exported from New Zealand to Hong Kong.

GASTEROMYCETES.

Phallus. The "eggs" of the stink-horn are eaten in French country districts—presumably as an aphrodisiac.

Lycoperdon. All the twenty or so British species of puff-ball are probably edible in the young stage. The giant puff-ball (*Lycoperdon bovista*) which is a foot or more in diameter is the best known species.

In this survey of the edible and poisonous fungi which occur in Britain it has not been possible to enter into much detail. Sufficient has been said, however, to indicate that poisonous toadstools are comparatively rare. If the poisonous species of *Amanita*, together with *Tricholoma tigrinum*, and the species of *Entoloma* and *Inocybe* are known, little is to be feared.

I am indebted to my friend, Mr. Somerville Hastings, M.S., F.R.C.S., for the photographs of fungi; to my assistant, Mr. E. H. Ellis, for the copy of the plate of the Plaincourault Fresco (from *Bull. Soc. Mycol.*, France, xxvii, 1911), and to the Trustees of the British Museum for the loan of the blocks of the two line drawings from the Museum "Handbook of the Larger British Fungi," a work from which more details can be obtained on matters dealt with in this lecture.

A Cinematograph Demonstration of the Biology of Bilharzia Disease was given. The film (the property of the South African Institute for Medical Research, and kindly lent by Dr. W. Watkins-Pitchford) was shown.

Dr. J. B. CHRISTOPHERSON said that the film had primarily been made for educational purposes for use in South Africa in those districts in which schistosomiasis was prevalent. It had been sent over for the British Empire Exhibition, Wembley, 1924, to be shown on the cinematograph together with other representations of South African subjects. It had not been found possible to exhibit it at Wembley. It had been shown at the International Congress of Public Health, at Bordeaux, June 5, 1924, and it was being demonstrated that evening at the meeting of the Tropical Diseases Section, by permission of Dr. Watkins-Pitchford. The film had been constructed and worked out by Dr. Annie Porter, Parasitologist to the South African Institute. All the stages of the biological development of the bilharzia worm, outside and inside the body of the host, were shown, including living miracidia and cercariae which were microscopic and not easy to represent by the cinema. Still, success had been obtained. The film included a representation of a demonstration by a doctor in South Africa giving the intravenous injections of antimony tartrate for schistosomiasis. It would be noticed that the doctor was giving the injections to the patient standing. In Egypt the patient received the injections sitting, whereas in England the injections were given to the patient lying down.

Dr. Christopherson reminded Members that Dr. Annie Porter was exhibiting that evening: (1) Specimens of South African molluscan hosts of schistosomes. (2) Specimens of plants on which the water-snails fed and lived. (3) A map showing the distribution of schistosomiasis in South Africa.

After the demonstration a vote of thanks to the South African Institute for Medical Research and to Dr. W. Watkins-Pitchford for the loan of the film, was proposed by Sir Leonard Rogers, and unanimously carried.

Section of Tropical Diseases and Parasitology.

President—Professor LEONARD S. DUDGEON, C.M.G., C.B.E., F.R.C.P.

Some of the Infections of Captive Animals.

By NATHANIEL S. LUCAS, M.B.

IT is usual, when discussing infection, to classify the various types as acute, subacute, and chronic. Unfortunately when this division is applied to captive wild animals it breaks down to a very great degree, so frequently is it impossible to say from the post-mortem into which division the case should go. Sometimes it is easy enough; the animal is in the best of coat or plumage, fat without being over-fat, and from the macroscopic and microscopic inspection obviously dead of an acute illness. Conversely, extensive wasting (I say extensive, because many mammals and birds are singularly devoid of fat when in health) and conditions such as intense fibrosis or calcification indicate a chronic infection. But in between these extremes lie a large number of cases, wherein the duration of the illness cannot be estimated. There is no real history; at the best the keeper can only say that the patient was out of sorts, or inclined to mope, or off his feed, and even this last or rather its converse is not always a reliable indication. I have carried out a post-mortem on a monkey which had died of a well-advanced pneumonia, with both hands as well as its stomach full of boiled rice. I think, therefore, that the division "subacute" must drop out.

Let us first consider what are the commonest sites of acute infections. Without a doubt the two commonest sites of infection are the pulmonary and alimentary systems. In the pulmonary system all grades of lesion are found, from a slight generalized congestion, combined with some cedema, to a massive red hepatization of almost the whole of the lung area.

I have never met with pleurisy with effusion, except when tuberculous. Pleurisy of the dry order occurs, but not commonly. In the lungs all degrees of infection are noticeable. I have on the table two specimens, one showing complete hepatization (in a capybara), the other in a very much earlier stage (in a coypu); and this brings me to a very important point which I wish to make, in the description of infections, and more particularly pulmonary infections, and that is the extraordinarily low powers of resistance exhibited by so many captive animals. A classical example of this was afforded by the polar-bear cubs, which invariably died of pneumonia within a week of birth; at least, when I say invariably, I must except those few cases where accident had been the cause. The result of this poor resistance, as seen in the post-mortem room, is that death occurs when the state of the lungs is that corresponding to a very early stage of disease. Dr. Gaskell, to whom I sent specimens illustrating some of these pulmonary infections, said that the state of the lung was comparable to that seen in animals inoculated artificially with a heavy dose of a virulent bacterial culture. This accounts for the fact that grey hepatization is so rare. The animal dies before that stage is reached. It becomes evident in another way. I have at various times stepped aside from strict pathology and become a clinician. I have frequently learned something in doing so. On several occasions I have attempted to treat animals with acute pulmonary infections. In spite of every attempt to maintain the strength of the patient, death has invariably resulted.

A true lobar pneumonia is never seen, or, at least, I have never seen one. Not infrequently the external appearance of the lobes suggests a lobar pneumonia, but

on section some part of the affected lobe is found to be aerated, and the microscope shows that the real state of affairs is a broncho-pneumonia with many small foci of disease.

There is no particular type either of mammal or bird which is specially immune or specially subject to acute pulmonary infections, although one may perhaps say that the larger birds are the least frequent denizens of the post-mortem table. Among the mammals, size is no criterion of power of resistance whatever. I have seen a giraffe apparently well one evening and dead on the next morning, with a congestion of the right lung in quite an early stage, while I have seen a marmoset put up quite a good fight against a pneumonia, and be one of the few instances of an animal reaching grey hepatization.

It is not unlikely that you are wondering what are the causal organisms. So far the pneumococcus and the streptococcus, both hæmolytic and non-hæmolytic, have been identified, and the Pfeiffer bacillus has been suspected in sections but not isolated in cultures. The tubercle bacillus I shall refer to later.

Under the microscope are examples of lung containing organisms. One is the pneumococcus in a lion-cub. The other specimen is from a mona cercopitheque, showing what appears to be Pfeiffer's bacillus in large numbers. This last is one of Dr. Gaskell's sections.

Purulent pericarditis is not uncommon among animals. Under the microscope is a specimen showing what appear to be streptococci, but I have never been able to cultivate one of these. Birds sometimes have pericarditis with more or less effusion, but the usual form of avian pericarditis takes on the appearance of a thick white chalky pericardium, which is generally supposed to be of gouty origin. I have never, either microscopically or chemically, been able to prove the presence of uric acid.

Let us now turn to the other great site of infection, the alimentary canal. Enteritis among mammals, birds and reptiles always has been a most prevalent cause of death. Since the introduction of greater cleanliness and better quality of food, the incidence has fallen somewhat among the mammals, but in the birds there is no falling off except among the carnivora, where a better quality and more suitable meat has been given.

When we come to the description of the causal organisms, I must confess to a complete inability to give a satisfactory display. I have isolated a pure culture of *Bacillus pyocyaneus* in a kangaroo, which had extreme diarrhoea, of which it subsequently died. Also I have found the same organism in several other animals, such as a civet cat, a panda and cat-bear. Among the birds only one organism has been isolated, and this was done by your Secretary, Colonel Perry. It was the *Bacillus aertrycke* ("mutton" type), and was found by him on two occasions, in a touracou and a little parakeet.¹ The lack of organisms isolated is not an index of the number of attempts made, both from faeces during life, and from scrapings of gut post mortem. As a result of this, one point has struck me, which is, that while in the healthy bird the crop of colonies of *Bacillus coli* on a plate is not large, in a diseased bird the colonies are very numerous.

I wish to put forward the theory—and I admit it is only a theory—that the *Bacillus coli* plays a far more important part in the pathology of enteritis in birds than it does in the human subject. Excepting some of the carrion and fish eaters, there are no birds which are receiving a diet anything like approaching their natural diet, and I submit that in this way their resistance, both local and general, is diminished, until the stage is reached at which *Bacillus coli* becomes pathogenic.

I have not, so far, said anything about the actual path of infection of the gut, and I have little to say. I am able, however, to produce two instances showing

¹ *Brit. Journ. Exper. Path.*, 1920, i, p. 131.

paths of infection. One should be of special interest to this Section. The specimen has already been shown by Colonel Perry; but as this was four years ago I may be allowed to show it again. It is from a case of helminthiasis, in a clouded tiger, the infection being one of *Uncinaria perniciosa*. In the section shown the intestinal mucous membrane is seen to be intact, and the submucosa to be invaded by the worm, in the gut of which are bacteria of various kinds. The other case is that of a jungle-cat, which had osteomyelitis of the lower jaw. The stomach shows numerous ulcers, which in the section shown contain at their base large numbers of a large Gram-positive bacillus, presumably a secondary infection to that of the jaw. The skull and stomach are also shown.

The last class of acute infections with which I have to deal are the septicæmias. From these I have isolated one, if not two, organisms. The first is that of *Bacillus pyocyaneus*, mentioned above, which in some of the cases, notably the panda, seemed to produce a general septicæmia rather than a localized lesion. The other organism belonged to the *Pasteurella* group, and was isolated respectively from a marmoset, a chevrotain and a rat-kangaroo. The marmoset was the first case in which I attempted a bacteriological examination. It raised hopes which were doomed to grave disappointment.

Turning now to the chronic infections, the most important is tuberculosis. There is, I find, an idea very prevalent among the general public, both lay and medical, that tuberculosis causes the bulk of deaths among the mammalia in the Gardens. This is not so. It is, I think, a diminishing disease, but occasionally there are epidemics of it, which keep up the numbers of the cases. Such an epidemic occurred in the anthropoid ape house in the early part of 1924. With regard to the incidence of the disease, among the mammalia, the apes, monkeys and herbivora appear to be the most susceptible, while the carnivora and rodentia are very much less so. Among the birds the disease has diminished greatly in the last two or three years, and scarcely a case has occurred during the last year outside the western aviary, which is somewhat ancient, crowded and, owing to the Mappin terraces, rather sunless. It is worthy of note that here the ground-feeding birds are almost entirely the ones affected.

Both the human and bovine types of infection exist among the mammals. It is something of a puzzle how bovine infections occur in some instances. One can quite understand it in those animals whose diet consists, in some part, of milk, such as the anthropoid apes, only they have died of tuberculosis of the human type; but how an animal such as the rhinoceros living in solitude acquired the bovine type it is difficult to see.

Both pulmonary tubercle and abdominal tubercle occur, and it is not always easy to say what was the original site, so generalized have the lesions become by the time the case comes to the post-mortem table. I have only once seen joint tuberculosis, and that was in a dove, both knees being affected. The cases are nearly always of fairly long standing, though in the one exhibited, that of a bear, the disease was an acute tuberculous pneumonia. This is the only case of the kind I can recall. Pure intestinal tuberculosis as a primary lesion seems somewhat uncommon. The uro-genital system is rarely infected. I have seen only two or three cases.

Aspergillus fumigatus has, in the past, been a frequent cause of death among the birds, though at present the incidence is small. The perpetual existence of the spores in the soil, however, always constitutes a danger, though at the present time the authorities are alive to it and keep the disease in check by frequent liming and re-turfing.

The disease takes the form either of a moderate growth of mycelium with free growth of spores, in which case it appears to kill by a toxæmia, or sometimes the reverse is the case, and death is apparently due to mechanical causes. The spores

contain a toxin extractable with ether, but up to the present I have not succeeded in getting it in state of purity.

The air-sacs of birds are the commonest site, and I have never seen it in a mammal, though my predecessor, Dr. Plimmer, records an epidemic of it in kangaroos and wallabies.

What I have seen in these animals is a disease starting in the jaw, generally in the tooth sockets, where a septic focus is formed, with a spreading suppuration, causing death by inhalation-pneumonia. Dr. Fox attributes this to a streptothrix, which he has with difficulty isolated in pure culture and which he calls *Nocardia macropodidarum*. So far, experimentally, he has failed to reproduce the lesion by the inoculation of this streptothrix into the jaws of animals; hence its being the cause is still open to doubt.

So far I have said nothing about the communicability of these infections to man. With the exception of the *Pasteurella*, they are all found in man, although the pathogenicity of *Bacillus pyocyaneus* is doubtful. We must, I think, view with caution the idea that epidemics of pulmonary infection which occurred in the Gardens and in the streets simultaneously were in any way a cross-infection. As I have shown elsewhere certain climatic influences render both the higher and lower mammals equally susceptible to any prevalent infection.

As far as I am aware an instance of man infecting the animal, or vice versa, has never occurred at the London Zoological Gardens. At the Zoological Garden at Ghizeh it was found that the mortality of the animals from tuberculosis was diminished by increasing the distance between the animal and its visitor, until the expectoration of the latter could no longer reach the former.

Discussion.—Professor F. T. G. HOBDAY said that it was interesting to note that the diseases of animals in captivity were much the same as those of animals kept in a state of domestication. Like Dr. Lucas, he had seen an animal (the one he referred to was a cat) which had died of well-advanced pneumonia whilst actually eating food. He agreed that the two commonest sites of infection were the pulmonary and alimentary systems, and his experience of what few wild animals had come under his care had been like that of Dr. Lucas, that when they suffered from acute pulmonary infections they almost invariably died.

Dr. Lucas stated that purulent pericarditis was not common amongst mammals. This condition was by no means common amongst the domesticated varieties; and when it did occur, it was usually traumatic in origin.

He (Professor Hobday) agreed in the observation made by the author that the incidence of death from enteritis was less in animals which were fed in a cleanly manner than in those in which the feeding and hygienic conditions were bad. This had been especially noticeable of late years in large kennels and avaries, where the animals were valuable and the owners had in consequence made the matter of feeding and hygienic housing one of important attention. It was emphasized by one of Dr. Lucas's remarks, in which he said, in speaking of tuberculosis, that the ground-fed birds were almost entirely the ones affected.

In regard to the case of the rhinoceros in which bovine tuberculosis infection was shown post mortem, he wondered whether this animal's domain was anywhere in direct or in indirect contact with the cattle pens, and whether there would be a prospect of infective material being carried into the rhinoceros's quarters by the one or other means. He (Professor Hobday) would also like to know whether the epidemic amongst the kangaroos and wallabies, in which the septic disease originated in the tooth-sockets of the jaw and eventually caused inhalation-pneumonia, chiefly affected the older animals. It was satisfactory to learn that so far as could be ascertained no case had occurred of infection being spread from the animals to man or vice versa.

Lieut.-Colonel H. MARRIAN PERRY said that with reference to the alimentary infections discussed by Dr. Lucas, there were a few points which called for comment. The isolation of *Bacillus pyocyaneus* from the intestinal contents, whether in cases of human or of animal enteritis, was of little value as an indication of active infection with this organism in the absence of positive blood-culture. It was well known that this bacillus was very frequently present in the intestinal tract as a secondary invader, and in such cases could be regarded as

an aetiological factor. Regarding the hypothesis that *Bacillus coli* assumed an enhanced virulence and played an active rôle in the production of these conditions, he (Lieut.-Colonel Perry) did not consider that it could be accepted in default of any observations on the development of antibodies in the blood for this organism—such as agglutinins. These observations, if positive, would afford some evidence of an active invasion of the intestine by this bacillus.

Dr. LUCAS (in reply to Professor Hobday) said that the rhinoceros and the cattle lived at almost opposite ends of the Gardens. In the case of the infection of the jaw in wallabies it always occurred in the fully adult, but not necessarily in old, animals.

Lieut.-Colonel Sir LEONARD ROGERS, F.R.S., read a paper on "Points of Resemblance in the Epidemiology and Treatment of Leprosy and Tuberculosis," which was discussed by Sir PERCY BASSETT-SMITH, Sir WILLIAM LEISHMAN, the PRESIDENT and Sir LEONARD ROGERS.

The paper is being published in two parts:—

(I) "Tuberculosis Incidence and Climate in India. Rainfall and Wet Winds." (*British Medical Journal*, February 7, 1925, p. 256).

(II) "The Resemblance between Tubercle and Leprosy." (*British Journal of Tuberculosis*, April, 1925 issue.)



Section of Tropical Diseases and Parasitology.

President—Professor LEONARD S. DUDGEON, C.M.G., C.B.E., F.R.C.P.

Kala-azar in China with Special Reference to its Histopathology in Experimentally Infected Hamsters.¹

By HENRY EDMUND MELENEY, M.D.

Peking, China.

ALTHOUGH the presence of kala-azar in China was suspected as early as 1904 [1], and was established by Aird in 1905 [2], it was not until Cochran's survey in 1913 [3] that it was generally known to be one of the important diseases of China. Since then data have rapidly accumulated, and Young's report in 1923 [4] indicates that the endemic area in China is perhaps as large as that in India. It covers the provinces of Chihli, Shantung and Honan, the southern third of Manchuria, and those portions of Kiangsu, Anhwei and Hupeh Provinces which lie north of the Yangtze Valley. This represents an area of about 350,000 square miles, which is approximately the size of the endemic area of kala-azar in India as given by Napier and Muir [5]. Cases have also been reported from the provinces of Shansi and Shensi, but the data are too meagre to warrant the inclusion of this territory in the known endemic area at present.

Kala-azar is not known to have reached epidemic intensity in China as it has in Assam, and in most places it attacks a relatively small proportion of the population. In southern Chihli, for instance, Young surveyed a village and found twenty cases, all except two of which were in separate houses. Young states, however, that in the region about Hsuehowfu, in the north-western part of Kiangsu Province, the disease deserves the name of a scourge, for in one village every second child is said to be infected.

One of the striking features of the distribution of kala-azar in China is its limitation to the dry region north of the Yangtze Valley. Heavy rains occur only in July and August, and from November through April there is very little precipitation. The humidity is correspondingly low. The country is intensely cultivated, the chief crops being wheat, millet, kaoliang and other crops requiring relatively little moisture. This condition is in sharp contrast to the climate of Assam, where the humidity and precipitation are very high. Another interesting feature of the distribution of the disease is that it is much more prevalent in small villages than in the walled cities; that is, it is most prevalent among the agricultural population.

Clinically, kala-azar in China is a moderately acute disease. If untreated some cases probably succumb within six months of the onset of symptoms. It is more acute in younger subjects. A few cases recover spontaneously, usually because of the occurrence of an intercurrent infection which produces a leucocytosis. Very chronic cases are rare, and the severe cirrhosis of the liver often seen at autopsy in Calcutta has not been reported in China. The age incidence among 762 cases collected by Young [4] showed percentages for the first three decades of 31.5, 40.1, and 18.4, making a total of 90 per cent. of cases in the first half of life.

In the clinic at Peking diagnosis is usually made by spleen or liver puncture. This procedure is only attempted on patients with a normal bleeding time, and, as a result, no untoward effects have been encountered. The method devised by

¹ From the Department of Medicine, Peking Union Medical College, Peking, China.

Young [4, 6] for the detection of Leishman-Donovan bodies in the peripheral blood is, however, practically as accurate as spleen puncture, and is the method of choice where time and equipment are available. The globulin precipitation test, as refined by Sia [7], is used for a presumptive diagnosis, and has been found to be as accurate as the more cumbersome "aldehyde" test.

In the treatment of kala-azar, the various antimony preparations are being widely used in the Chinese endemic area. Clinics for treatment as out-patients exist at many hospitals, and at Hsuehowfu Dr. MacFadyen and his staff go out to endemic villages to administer treatment. In the clinic at Peking colloidal antimony sulphide has not proved to be as efficient as the preparations of tartar emetic. As in other countries, a few cases are resistant to antimony treatment, but the large majority are cured if they continue treatment as long as advised.

EXPERIMENTAL INFECTIONS.

The same difficulty in producing infection in experimental animals was at first met with in Peking as has been encountered elsewhere. In December, 1922, however, Smyly and Young [8] inoculated striped hamsters intraperitoneally with an emulsion of the spleen from a fatal case of kala-azar, and secured infection in every animal inoculated. Since then a large number of these animals have been inoculated with material either from human cases or from infected hamsters, and infection has occurred in almost every case. In one case the hamsters became infected from human spleen emulsion even when smears and cultures from the same organ were negative.

In view of the probability that other workers may wish to use the hamster for the experimental study of *Leishmania* infections, it may be of interest to summarize the available information concerning the classification and distribution of this animal.¹ In the family Muridæ there are three subfamilies, distinguished not by their external physical features but by the characteristics of the molar teeth. These subfamilies are: (1) the Murinæ, the true rats and mice, all of which are native to the Old World; (2) the Microtinæ, the voles and lemmings of both the Old and the New World; and (3) the Cricetinæ, which include the hamsters of the Old World and all the native so-called rats and mice of the New World. The animals which are ordinarily known as hamsters may, for practical purposes, be defined as those members of the subfamily Cricetinæ inhabiting the Old World, which live in burrows in the ground, and most of which have short tails and cheek-pouches. These hamsters all belong to three genera [9]. The genus *Cricetus*, the common European hamster, to which the German name "hamster" was probably first applied, is found in Central Europe from Belgium and Northern France eastward into Russia and Asia Minor. The eastern limit of its habitat is not known. The second genus, *Mesocricetus*, somewhat smaller in size, inhabits the Caspian region, Asia Minor, Roumania and Bulgaria. Its limits are also unknown. The third genus, *Cricetulus*, is still smaller, ranging from about 75 to 175 mm. (3 to 7 in.) in length of head and body. Its habitat extends from the eastern part of Greece and the Balkan Peninsula eastward through Asia Minor, Persia and Turkestan to the eastern coast of China and into Korea. As far as I know there is no publication which lists all the known species of *Cricetulus* with their distribution. I was able to find the names of twelve species which are probably authentic. It is interesting to note that none of these three genera are known to exist in the endemic areas of kala-azar in Italy, North Africa, or India.

Cricetulus griseus, the striped hamster, which is the species used in Peking for kala-azar work, has been found in the provinces of Shensi, Shansi, Chihli, Shantung,

¹ The information given here was obtained from Mr. Martin A. C. Hinton of the British Museum of Natural History, South Kensington, and from books and papers in his possession. I wish to record my thanks and appreciation of his co-operation.

and Kiangsu, and is probably present also in Honan and Anhwei, which are very close to that portion of Kiangsu where Young has found the species. A subspecies, *Cricetulus griseus obscurus*, is found in Inner Mongolia, and another, *Cricetulus griseus fumatus*, in Manchuria.

Cricetulus griseus has the general appearance of a short-tailed field mouse. The head and body together measure 86 to 100 mm. ($3\frac{1}{2}$ to 4 in.) in length, the tail 27 to 29 mm. (1 in.). Its colour is greyish-brown on head, back and sides, white on the belly. It has a distinct black stripe extending the entire length of its dorsal midline. It possesses cheek pouches. It inhabits burrows in small banks in gardens or sometimes between the rows in wheat-fields (Malcolm Anderson) [10]. In the country about Peking it is so plentiful that farmers bring many specimens to the Peking Union Medical College for the price of a few coppers each, and they are used for the rapid typing of pneumococci. They do not breed in captivity unless provided with burrows or with considerable freedom of activity.

THE HISTOPATHOLOGY IN THE HAMSTER.

The material for the present histopathological study was obtained from hamsters experimentally infected by intraperitoneal injection of emulsified liver, spleen or bone-marrow from human cases or from other hamsters.¹ I am indebted to Dr. Smyly and Dr. Young, of the Peking Union Medical College, for the opportunity of describing the tissues. Sections of all organs were examined from twenty-five animals sacrificed at intervals from one day to fifteen months after infection. The first Leishman-Donovan bodies in tissue sections were found in the liver of an animal infected six days. The bodies were present in small numbers in a few of the Kupffer cells lining blood-capillaries. They were first found in sections of the spleen in the nine-day animal, in lymph nodes in the fifteen-day animal, and in the bone-marrow in the twenty-four-day animal. Young, Smyly and Brown [11] found Leishman-Donovan bodies in smears from this same series of animals in the spleen three days, and in the liver and bone-marrow four days, after infection. The difficulty of recognizing single organisms in sections and of getting thin sections of bone-marrow accounts for the discrepancy between their results and mine.

Following the first appearance of the parasites in these organs, there developed a true tissue-reaction to the invading organism, which I interpret as the fundamental pathological lesion of kala-azar, and which, I believe, has not heretofore been sufficiently emphasized. It consists of the proliferation of that type of cell variously known as the clasmatocyte, histiocyte, endothelial leucocyte or large mononuclear phagocyte. This type of cell is represented in the liver by the Kupffer cell, in the spleen by the "spleen pulp cell" and in the lung by the "heart-failure cell." Its function is to phagocytize red blood-cells, pigments and foreign particles of low toxicity. In response to the *Leishmania* infection the clasmatocytes of the spleen, lymph-nodes and bone-marrow proliferated into masses of tissue which replaced the normal cells in the reticulum of those organs (fig. 1). At first most of the cells making up these islands of tissue were free from parasites, but as the infection progressed and the clasmatocyte tissue became more abundant, most of the cells became parasitized. Finally in the fifteen-months animal, the spleen increased to twenty times its normal size, and the entire pulp of the organ became replaced by parasitized cells (fig. 2). Some of these cells were so distended with parasites that one could count 200 in one cell in a single focal plane. Others, on the other hand, contained very few organisms, but were nevertheless much larger than the ordinary clasmatocyte. Many of the cells contained more than one nucleus, and in the lymph nodes parasitized giant cells, containing a ring of nuclei, were occasionally seen. The venous sinuses of the spleen were compressed by the swollen clasmatocytes, but the

¹ This material is described in more detail in a paper entitled "The Histopathology of Kala-azar in the Hamster, Monkey and Man," *Amer. Journ. Path.*, in the press.

normal flat endothelium of these vessels could usually be made out, limiting the encroachment on the lumen. Frequently, however, parasitized cells were present in the blood-vessels. In the lymph nodes and bone-marrow, parasitized cells finally

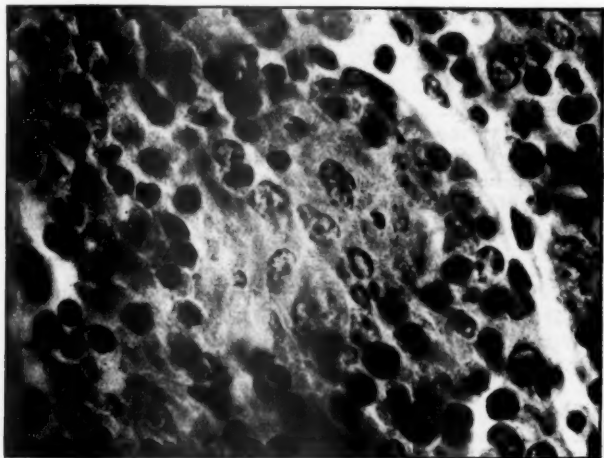


FIG. 1.—Lymph node of hamster infected three months. The pale area is a mass of clasmatocytes replacing part of a lymph follicle. Only a few cells contain Leishman-Donovan bodies. $\times 800$.

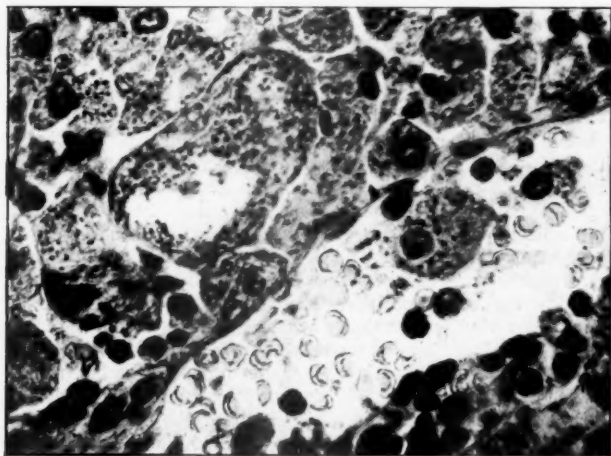


FIG. 2.—Spleen of hamster infected fifteen months. Spleen pulp filled with parasitized clasmatocytes. Some are also free in blood-vessel. $\times 800$.

replaced from one-third to one-half of the normal tissue, and hæmatopoiesis in the marrow was definitely impaired.

In the liver, where the reacting cells (Kupffer cells) lined the lumen of blood-vessels, the proliferation produced masses of cells within the vessels, ultimately

occluding the lumen and crowding back the adjacent liver cells (fig. 3). In the fifteen-months hamster this had produced atrophy or even disappearance of many liver cells, and wide blood spaces, filled with huge, heavily parasitized cells, dominated the



FIG. 3.—Liver of hamster infected one year. Blood-capillary distended by the proliferation of parasitized Kupffer cells. Several parenchymatous cells of the liver also contain Leishman-Donovan bodies. $\times 800$.

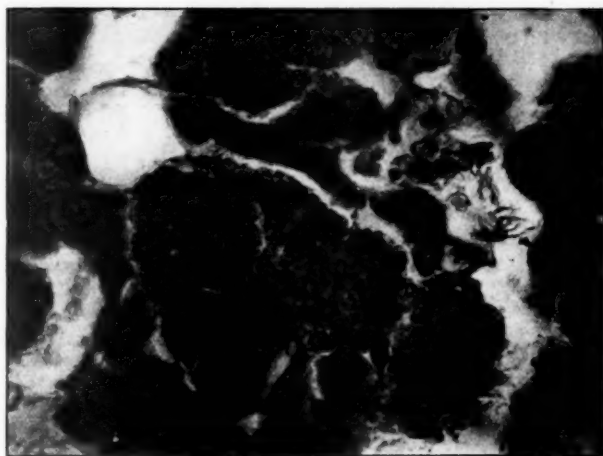


FIG. 4.—Liver of fifteen-months hamster. Liver cells compressed by huge parasitized Kupffer cells. $\times 800$.

picture (fig. 4). In the animals infected four months and over, the parenchymatous cells of the liver became parasitized, until in the fifteen-months animal some areas showed practically every cell involved.

Elsewhere in the body, proliferation of clasmatocytes was encountered wherever

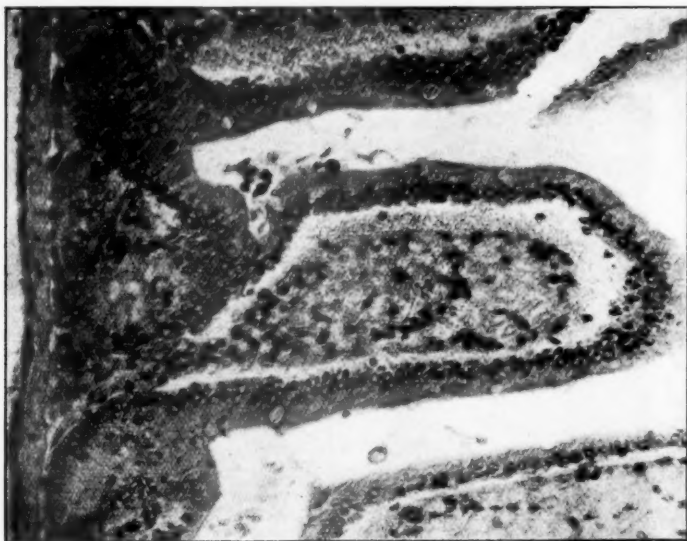


FIG. 5.—Duodenum of fifteen-months hamster. Stroma of villus filled with parasitized cells. Epithelium intact. $\times 250$.

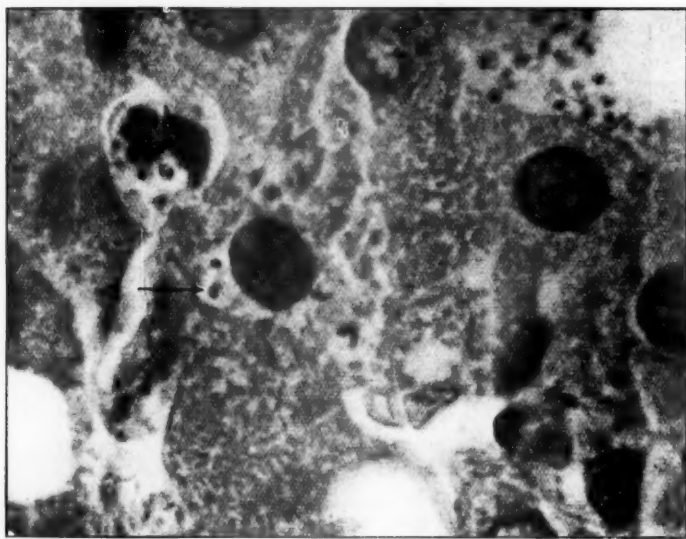


FIG. 6.—Suprarenal of fifteen-months hamster. Arrow points to Leishman-Donovan bodies in parenchymatous cell of cortex. $\times 2,000$.

I am indebted to Mr. Schmidt, of the Illustration Department of the Rockefeller Institute for Medical Research, for taking the photomicrographs shown in figs. 5 and 6.

they are normally present. The best example of this is in the stroma of the intestinal mucosa. In the fifteen-months hamster the villi appeared to be somewhat distended, and the stroma was completely filled with parasitized clasmatocytes, compressing the blood and lymph vessels (fig. 5). This was most marked in the duodenum, and decreased progressively towards the rectum. The epithelium over the villi was intact, in contrast to the condition found by Perry [12] in human cases of kala-azar in India. I believe that the absence of epithelium in Perry's cases was probably due to post-mortem autolysis. The deeper coats of the intestinal wall contained many single parasitized clasmatocytes.

Other localities especially involved were the peritoneal fat tissue, the submucosa of the prostate, and the interstitial tissue of the pancreas, salivary glands, suprarenals, lungs and testes. In the fifteen-months animal there was also an irregular atrophy of the convoluted tubules of the kidneys, with many parasitized cells in the interstitial tissue. This animal, indeed, showed infected cells in almost every section examined, including the subcutaneous tissue and the meninges. A few of the parenchymatous cells of the cortex of the suprarenals also contained Leishman-Donovan bodies (fig. 6).

These observations indicate that *Cricetulus griseus* is an ideal animal in which to study visceral Leishmaniasis. The parasite apparently has very little toxic effect on the animal; but the animal, on the other hand, has little power of destroying the parasite. This leads to an extreme picture of the tissue-reaction to the disease which is seen much less clearly in human cases, or in monkeys, which usually die within two months after infection. Since the hamster is easily infected, it may prove to be of value also in testing the ability of possible insect vectors to transmit the disease.

SUMMARY AND CONCLUSIONS.

(1) The endemic area of kala-azar in China is about as extensive as that in India. In the region about Hsuechow, in northern Kiangsu Province, the disease is almost of epidemic proportions.

(2) The disease exists in China only north of the Yangtze Valley, so far as is now known. The endemic area is notable for its dryness during ten months of the year.

(3) The clinical features of the disease are very similar to those met with in Assam.

(4) A statement is made of the known distribution of the three genera, *Cricetus*, *Mesocricetus* and *Cricetulus*, which constitute the group of rodents usually known as hamsters.

(5) The striped hamster, *Cricetulus griseus*, is an ideal experimental animal for the study of visceral Leishmaniasis. It is very easily infected, but is very tolerant to the parasite, so that an intense pathological picture of the disease is produced.

(6) The fundamental tissue-reaction to *Leishmania* infection is the proliferation of the large mononuclear phagocyte (clasmatocyte) of the body into masses of "clasmatocyte tissue" wherever these cells are normally present in considerable numbers.

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Discussion.—(a) In reply to Dr. BALFOUR's questions Dr. MELENEY said:—

(1) In regard to the reason for the ease with which hamsters are infected, I can only say that in white mice, which are also fairly easily infected, I found in Baltimore last autumn that the percentage of polymorphonuclear leucocytes in the peripheral blood was very low, and that the percentage of clasmatocytes in the normal peritoneal fluid was very high. These two points may have some bearing on the question.

(2) If hamsters infected with kala-azar are not sacrificed, I am sure that nearly all of them will die of the infection. But doubtless most of them will live for many months. We have had none which are known to have recovered.

(3) We feed our hamsters on a stock animal-house diet consisting of moist beans and a little "green stuff."

(4) The toxin liberated by the organism seems to have little effect on the general health of the hamster, despite the fact that an intense tissue-reaction to the organism is produced.

(5) Hamsters are known to harbour fleas, lice and ticks. So far as I know, none of these insects have been found to harbour *Leishmania*.

(b) In reply to Dr. WENYON:—I believe that the proliferation of clasmatocytes, or macrophages, is a protective response on the part of the mammalian body. The same thing occurs to a less extent if one injects a rabbit repeatedly with carmine intravenously. This was demonstrated to me by Dr. Sabin. The Kupffer cells of the liver fill up the lumen of capillaries and their cytoplasm is distended with particles of the dye. There are several other diseases, Oroya fever in South America for one, and typhoid fever for another, in which the body responds by the proliferation of clasmatocytes. *Cryptococcus* infection, mentioned by Dr. Thomson, produces a picture which may be called a combination of that seen in visceral and in cutaneous Leishmaniasis. Not only are the clasmatocytes diffusely proliferated and parasitized throughout the body, but they collect also into localized nodules in the lung, &c.

(c) In reply to Dr. PARKES WEBER:—His references to non-infectious conditions in which the clasmatocyte is functionally active shows the importance of this cell in the body. There is no doubt that more attention will be paid to it in the future, and that the study of it is still in its infancy. Beside its defensive activity, it may have an important function in the formation of bile-pigments from the red blood-cells which it normally digests. This has been suggested by Rich. Parasitized cells in kala-azar have not been found in either the urine or the sputum, as far as I know. Leishman-Donovan bodies have been recovered from the urine, however.

(d) In reply to the President, Professor DUDGEON:—I believe that parenchymatous liver cells can be easily differentiated from swollen Kupffer cells both by the character of the nucleus and by the staining properties of the cytoplasm. In both hamsters and monkeys there was evidence of liver-cell proliferation, in the form of mitotic figures, but these were easily distinguished from the mitoses in Kupffer cells.

Section of Tropical Diseases and Parasitology.

President—Professor LEONARD S. DUDGEON, C.M.G., C.B.E., F.R.C.P.

Tropical Ophthalmology (Egypt).

By A. F. MACCALLAN, C.B.E., M.D., F.R.C.S.

(ABBREVIATED.)

THE clinical material for the present paper is drawn from twenty-three ophthalmic hospitals in different parts of Egypt, which were under my control, and which were staffed by Egyptian surgeons, my former pupils.

The problem of subtropical ophthalmology may be considered under two heads; first as affecting the European population, by which I mean people born in, or the children of people born in, Europe: and second, as affecting the native population.

The European patients are usually dealt with in private practice, though the less highly paid of these, such as engine-drivers, and other technical ratings, as well as clerks in business offices, have difficulty in finding the ordinary fees for ophthalmic consultations.

The richer classes of the native population are able and willing to provide themselves with adequate specialist treatment, especially when it can be obtained from their own countrymen. It is in Egypt, alone of oriental countries, that a comprehensive effort has been made to deal with the ophthalmic question, by the establishment, in each of the fourteen provinces of this small country, of specially built ophthalmic hospitals, and by the provision of ophthalmic treatment at each of the Government Primary Schools at Cairo and Alexandria, and in the capital town of each province.

OPHTHALMIC CONDITIONS MET WITH AMONG EUROPEANS TO WHICH THE CLIMATE CONDUCE.

(1) *Sun, Dust and Wind.*—The healthy, emmetropic eye becomes painfully affected by the sun's glare in the summer when one is motoring or riding along country, or over the desert. It is the heat rays from the red end of the spectrum which cause the discomfort or injury. This may be prevented by wearing protective glasses of Crookes' B material, or the still darker B2 material. In all cases in which discomfort from the sun is complained of, however, it is of the greatest importance to see that the patient is really emmetropic, and that he has not got a small error of refraction. This can only be done by an expert refractionist after paralysing the muscle of accommodation with homatropine or atropine. If an error of refraction is detected and can be corrected, it may be prescribed to be made up with Crookes' B glass if the patient is hypermetropic, or with Crookes' B2 glass if the patient is myopic; the darker glass being prescribed in the case of the thinner lenses. However, I do not often order them for my own patients. There are other varieties of protective glasses which, theoretically, are more absorbent of the heat rays, as recently shown by Sir Arnold Lawson¹ at the Section of Ophthalmology, such as peacock-blue, Fieuzal, and amber: the latter, especially, is favoured by the laity.

Dust and wind are great conjunctival irritants. At certain times of the year daily, fierce gales sweep up animal, vegetable, and mineral refuse from the roads, so that the conjunctiva becomes pervaded with foreign particles. The force of the wind irritates especially the small triangular part of the conjunctiva at each side of the cornea, which is exposed even when the lids are screwed together to avoid the

¹ "Tints and their Values," *Proceedings*, 1925, xvii (Sect. Ophth.), pp. 24-34.

dust as much as possible. This irritation may cause the formation of small yellowish red elevations, usually to the inner side of the cornea, caused by a cellular change in the conjunctiva with a deposition of elastic fibrils, called pinguecula. Or it may cause the formation of a pterygium; this is an encroachment on the cornea by the conjunctiva, and is said to be derived from a pre-existing pinguecula; however, neither I nor my assistants have ever observed this transformation. The pinguecula is stationary and requires removal only for cosmetic purposes, while the pterygium, unless removed in such a way that it cannot recur, may spread over the cornea, acting disastrously upon the vision.

To protect the eyes in dust-storms or during motor journeys along dusty roads protective goggles should be worn. In all cases if there is any error of refraction the correcting lens should be included. It should be remembered that even ordinary glasses prevent red-hot sand and dust from impinging on the cornea and conjunctiva, though of course they do not prevent entirely the access of foreign matter to the conjunctival sac.

(2) *Fatigue, Ill-health and Worry.*—Fatigue, ill-health and worry bring out latent defects in the eye in any country. A hot climate causes greater fatigue than a temperate one, and this fatigue in many cases leads to fatigue indigestion, with resulting physical depression. Worry, whether of official or private origin, similarly causes ill-health.

Under any of these conditions trifling errors of refraction if present, or slight degrees of heterophoria (lack of normal muscular balance of the eyes), exert effects quite out of proportion to their severity. It is, therefore, of the greatest importance that correction of these errors should be made as carefully as possible, and that the spectacles ordered should be adjusted correctly. In a highly sensitive patient, whose nerves are constantly being jarred by physical or mental discomforts, the correction of a very small degree of astigmatism may make all the difference between happiness and misery.

The symptoms which patients with an uncorrected error of refraction complain of are familiar to you all—headache, neuralgia, aching of the eyes, especially after a late night, frequent frowning with consequent fatigue of the subcutaneous muscles of the forehead, indigestion, inflammation of the eyelids, with or without suppuration of the glands of the lid-margin, and so on. Inability to see as well as other people in the dusk is a not infrequent symptom, especially when the general health is impaired.

(3) *Acute Conjunctivitis.*—I shall bring to your notice my observations on acute conjunctivitis more fully in speaking of its incidence among the native population. It is enough to say here that the various organisms which cause this condition among natives may also attack European adults and children. I have seen acute gonococcal infections in English children of two or three years of age, which had no doubt been contracted from servants, without the slightest suspicion of venereal infection in either case.

(4) *Chronic Conjunctivitis.*—The chronic conjunctivitis prevalent in Egypt is trachoma. It occasionally attacks English children who have been taken care of by Egyptian servants. This is the reason why it is incredible folly to allow an Egyptian servant to touch or carry about English children, as practically all Egyptians have this contagious disease.

(5) *General Principles of Treatment.*—In dusty seasons it is most advisable to wash from the eyelashes the dust which collects there, and which may contain septic material. This should be done three times a day: in the morning, before lunch when washing the hands, and at night. In many cases this simple precaution is specific for preventing continual attacks of slight conjunctivitis.

The necessary search for septic foci should never be omitted. Dental apical abscesses and pyorrhœa are most fruitful causes of ophthalmic trouble.

OPHTHALMIC CONDITIONS AFFECTING THE NATIVE POPULATION.

It is impossible to deal even cursorily with all the eye affections peculiar to a subtropical climate, nor even in any detail with those affections in which I have been particularly interested. I have, however, selected the following conditions about which to make some remarks:—acute ophthalmias, chronic ophthalmia, ankylostomiasis¹ and changes in appearance of the fundus oculi, bilharziasis¹ and eye conditions, an epidemic of chancres of the eyelid,¹ fly-blown orbit¹ or orbital myiasis.

(a) *Acute Ophthalmias*.—During the period from 1917 to 1922 inclusive I studied the seasonal incidence of acute ophthalmias, and published the results in the Annual Reports of the Ophthalmic Section of the Department of Public Health. I have used the term "acute ophthalmia" as a synonym for acute conjunctivitis, which does not include, of course, that variety of chronic conjunctivitis called trachoma. The figures and curves hereafter given have not hitherto been published.

The six months, June to the end of November, corresponding to the warmer months of the year, are those in which the amount of clinical work is at its maximum. When these figures are compared with the monthly variations in the atmospheric temperature, a remarkable correspondence is found to exist between them. The figures for the temperature were provided by the Survey Department of the Egyptian Government. During the comparatively cold weather, beginning at the end of December and lasting until the middle of February, only about 3,500 to 4,500 new patients per month seek treatment at the ophthalmic hospitals all over Egypt. By the middle of February the temperature has begun to rise, and continues so to do until the end of July. This rise is accompanied by a rapid increase in the number of new patients, so that by the end of June they are more than 16,000 a month. The notch in the curve in the month of May is explained by the fasting month of Ramadan and the subsequent Bairam festivities, during which only absolute necessity impels a Moslem to attend a hospital. From the end of July the temperature-curve falls, at first slowly and then very rapidly, and is accompanied by a diminution in the number of new patients. The latter is checked in September and October by an annual autumnal blaze of gonococcal conjunctivitis.

The increase in the number of patients during the warmer weather is partly due to an increase in those requiring treatment for acute conjunctivitis. All such cases, especially all cases with much conjunctival discharge of an acute nature, are examined microscopically by means of a stained film under a $\frac{1}{8}$ oil-immersion lens, a diagnosis being recorded upon the morphological characters. With such large numbers of cases it is impossible to provide means for examining all cases by culture methods.

Gonococcal conjunctivitis in Egypt is a disease *sui generis*; it is non-venereal in origin, and is transferred from eye to eye mainly by means of fingers, garments and towels. The part played by flies in the transmission of bacterial eye infections is not certainly known, but it is believed to be a large one. At the present time our knowledge of the special habits of Egyptian flies requires considerable enlargement before further bacteriological or clinical work can be done in relation to them.

Gonococcal conjunctivitis in Egypt is characterized by the frequency of subacute and chronic forms. A research into the identity of the organism causing gonococcal conjunctivitis was undertaken at the laboratories of the Department of Public Health by the late Dr. Beaton and myself and published in the *Bulletin of the Ophthalmological Society of Egypt*, 1920.

Besides the gonococcus the other organisms most frequently found in acute conjunctivitis are the bacilli of Koch-Weeks, and of Morax-Axenfeld. Although I have studied their occurrence at different times of the year, I have not been able to trace any constant relation with variations in the atmospheric temperature.

¹ Omitted for lack of space.

44 MacCallan: *Tropical Ophthalmology (Egypt); Dye: Schistosomiasis*

There is no relation whatever between the incidence of any form of acute conjunctivitis and atmospheric humidity. I have not sufficient data about winds and dust to express any opinion on their relation with acute ophthalmias, though we all know that the conjunctivæ of our own eyes become red and inflamed during a dust storm.

A rare form of acute ophthalmia, which I described in the *Bulletin of the Ophthalmological Society* for 1923 (with Dr. Rasheed), was an epidemic of membranous conjunctivitis, which in twenty-two cases was proved in the bacteriological laboratories of the Department of Public Health to have been caused by the *Bacillus diphtheriæ*. Three of those infected died, six lost both eyes, five lost one eye, and three recovered after serious ulceration of the cornea. The average age of the patients was twenty months. The epidemic occurred during my absence on leave, and official anxiety was exhibited, rather in limiting the outbreak in the same manner as outbreaks of plague and cholera, by isolating the patients in cordons. It would have been better to have treated the patients' general and ophthalmic condition by more vigorous administration of antitoxin and more skilled treatment in the hospital ward.

In the journal referred to I described in detail the prophylactic and therapeutic measures which are necessary, and which will be employed in any future epidemic. It must be remembered that there are at least five organisms besides the diphtheria bacillus which may, if sufficiently virulent, cause a membranous condition of the conjunctiva, namely, the staphylococcus, the streptococcus, the gonococcus, the pneumococcus, and the Koch-Weeks bacillus. And the non-diphtherial organisms are nearly as dangerous to the sight as the diphtheria bacillus itself.

(2) *Chronic Ophthalmias*.—The main variety of chronic conjunctivitis found in Egypt is trachoma. It affects about 90 to 95 per cent. of the population. Since the organization of the ophthalmic hospitals and the training of skilled ophthalmic surgeons, the incidence of the disease has begun to diminish. This diminution is most marked in the upper classes. I consider that the establishment of school ophthalmic clinics has done a great deal for the practical teaching of ophthalmic hygiene. The organism causative of the disease is still unknown; I may mention that the infective material will pass through a Berkefeld filter, and will infect chimpanzees. It is, however, not to my purpose to speak inadequately of this disease, as would have to be done, on account of the size of the subject. My "Annual Reports on the Ophthalmic Section of the Department of Public Health," and my book on "Trachoma and its Complications in Egypt," contain a good deal of work on the subject. These are now out of print, but may be seen in the library of the Royal Society of Medicine.

Schistosomiasis and Splenomegaly in Nyasaland.

By W. H. DYE, M.R.C.S., L.R.C.P., D.T.M. and H.,
L.D.S.Eng., Capt., R.A.M.C.'

THE subject of this paper is the result of some observations that I was able to make while stationed as medical officer to the North Nyasa district of British Nyasaland.

This little-known part of the world is typical of the East African highlands, with an excellent climate for the greater part of the year. There is a fairly heavy rainfall, lasting over a period of about three months, but the remaining nine months of the year are dry and therefore the areas of permanent marshland, so essential to many tropical conditions, are very limited. In fact they can be said to be almost wholly confined to the edge of the lake shore, with very few and limited exceptions.

Like much of the East African hinterland the topography of the district varies enormously, consisting of flats, foothills and high cold plateaus, jumbled together, and affording the epidemiologist every opportunity, as all varieties of country are populated to a certain degree, although certain areas, owing to their fertility, are much more densely peopled than the rest.

Of these areas, Karonga, on the lake shore, contains the greatest percentage of the whole population of the district. This is not a township, but an irregular area of about twelve square miles, containing five or six white people, chiefly Government officials, and about 25,000 natives, men, women and children. The natives, therefore, still live in their original primitive surroundings, with no drainage or sanitary schemes to alter the natural conditions under which they have always lived.

Owing however to the stability of the British rule, with the absence of inter-tribal warfare and slave raids, the population is increasing most rapidly, and the villages are now necessarily placed much closer together, and in fact are often continuous, giving rise to many problems, both administrative and medical.

The people belong to typical Central African Bantu-speaking races, with a fine physique, the average man being 5 ft. 10 in. Their diet is mainly vegetable, but as it is in many cases supplemented with milk and fish, their ability to work, and above all, their resistance to disease, is fairly good. In fact their ability to recover from injuries such as crocodile bites, and injuries inflicted by the larger carnivora, is often remarkable. They also appear to live to a considerable age. I have stressed these points, as I think that they in part account for the resistance of the above races to the condition I am about to describe, and in fact account for the typical adult cases of splenomegaly, which otherwise would more frequently die in childhood.

On starting medical work at Karonga, I was at once struck with the very large numbers of children and young adults presenting a palpable, and often visible, enlargement of both the liver and spleen. This latter organ was frequently enormous, and protruded visibly as the child walked about.

Contrary to expectation, many of these young people appeared quite unaffected and unconcerned about the matter, and many of the less spectacular cases were only discovered when they attended for other complaints, such as yaws, which is endemic in the neighbourhood and brings a large number of people to hospital yearly.

The prevalence of the enlargement had been noticed by other observers in passing through the neighbourhood, notably Dr. Stannus, but, owing to lack of opportunity, these observers had not been able to attempt to do any systematic work on the subject. Otherwise they would, I am sure, have quickly come to the conclusion, that between this condition and schistosomiasis there was a close relationship. The unaltered condition of the country and its varying nature combined to make such a conclusion very much easier of acceptance than it might have been.

In general appearance these cases appeared to bear a marked resemblance to cases of Egyptian or tropical splenomegaly, figured in the text-books and in papers on the subject. I had not had the opportunity of seeing any of the Egyptian cases, and of course at that time Dr. Day, of the Royal School of Medicine, at Cairo, had not published his work on the subject.

Certain villages showed an extremely high incidence, while others appeared free, and as post-mortems among these primitive people were practically impossible to obtain, it was by making a spot map, and studying the topography, together with the local customs, that it was first suggested to me that the cause must be due to a schistosome or other water-borne parasite, requiring a fresh water mollusc as an intermediate host. Eventually I was able to come to the conclusion that the condition was entirely due to an intense infestation with *Schistosoma mansoni*. My reasons for drawing this conclusion I will detail later.

Owing to the very large numbers of cases obtainable for observation, it quickly became obvious that the dual enlargement of both the liver and spleen was a definite clinical condition, running a definite regular course, with well-defined symptoms, which, however, might vary in degree. Although with a disease lasting for such long periods it was not possible to follow any particular case through every stage, yet, with such a wealth of material, a very good idea can be obtained of what normally happens.

For convenience in description I originally divided the course of the disease into four stages—three acute stages and a chronic stage, but, as the so-called acute stages may last over very long periods, I think it is better to speak of three progressive stages, and a quiescent stage.

Taking the first stage, which I have called the "early stage without symptoms." Fig. 1 shows a very early stage of the complaint, with only slight enlargement of the liver, and no enlargement of the spleen. The child was suffering from yaws, for which it attended for treatment, and the hepatic enlargement was only accidentally discovered. But as can be seen the child is well nourished and appeared in the best of health. These people are little if at all affected by yaws in the so-called secondary stage.

Fig. 2 shows a more advanced type, still of this same early stage, presenting no symptoms of general ill-health. This picture is typical of hundreds of children in the infected villages. It is almost incredible the number of children who can be found in certain villages with this enormous enlargement of both organs, but who appear quite happy and contented.

Fig. 3 is a side view of one of these cases and shows the characteristic enlargement of the abdomen as compared with a normal child. The lower ribs tend to bulge out, giving a very different appearance from the common pendulant abdomen of native children living on a bulky diet.

If the hepatic enlargement is very great the outline of the liver can be seen when the child is standing, but it is very much more obvious when it lies on its back. The abdominal wall at this stage of the complaint is quite flaccid, often unusually flaccid, so that it falls away over the projecting edge of the liver. At this stage the spleen, although already often bigger than even the malarial state of the country would give reason to suppose, is not the most marked feature, as it becomes later. On palpation of the abdomen the surface of the liver feels soft and smooth, with a distinctly sharp edge, under which the fingers can often be slipped for a considerable distance. The border is marked with a deep notch lying to the right of the middle line, and the organ is quite painless to the manipulation just mentioned.

The spleen, as already mentioned, is not such a marked feature in this early stage of the complaint, but it slowly and progressively enlarges, and towards the end of the stage often reaches a considerable size. As such it is hard and board-like, fairly mobile and quite painless so far as can be made out.

The general condition of these children is, as above stated, excellent. There is no anaemia, nor any oedema of the face or limbs. The tongue remains clean and the appetite is good; in fact it is difficult, if not impossible, to make the parents understand that these cheery little people require treatment. This is especially the case in the heavily-infected villages, where they are so used to this condition.

I carried out a very large number of blood-examinations and blood-counts in this stage of the disease. With the exception of occasional malarial forms, no parasites were found and the counts were normal as compared with counts in children of similar ages taken from uninfected villages and showing no signs of the disease. The eosinophils averaged 14 per cent., but all these people suffer from multiple helminthic infection.

There is no doubt that a very large number of cases never advance beyond the first stage. An explanation of this, I think, can be found when we consider the method



FIG. 1.



FIG. 2.



FIG. 3.



FIG. 4.



FIG. 5.



FIG. 6.



FIG. 7.



FIG. 8.

of infection and the influence of native customs on the point. This I will discuss later. But, on the other hand, a certain number of these cases do definitely begin to go downhill and develop into what I have called the second stage, or "*early stage with the symptoms*." While patients in the first stage, owing to absence of unpleasant symptoms, are rarely if ever brought to hospital for treatment, those in this second stage are common visitors, and the parents will readily bring them for the repeated injections necessary.

Fig. 4 shows one of these patients. The important point to note is that wasting has definitely set in. As soon as the child begins to lose weight a very marked change takes place. Anæmia of the secondary type becomes evident and the attitude of the child is entirely altered. When well, these children are extremely active and cheerful, and, when they have lost the natural fear of a stranger, are very happy little people to deal with. But in the second stage of the disease they can be easily picked out when walking through a native village. All brightness is gone, and they sit aside by themselves quietly and make no attempt to join the crowd of playing children. When brought to hospital they are shy and easily frightened, which is a very typical attitude of the sick negro, who above all things fears the unknown. On inquiry it will be found that the appetite is lost, pain in the epigastrium is more or less continuous, and the child is very definitely wasted. The temperature yet remains normal, but the liver is harder, and, more important still, the spleen will be frequently much more evident.

As will be realized, such a definite disease as this is well known to the local inhabitants. They do not, however, trouble about it as long as the child remains healthy, and do not look upon it as a disease; but when once the child begins to waste, the parents, who have a very great affection for their children, look upon the condition as serious, and state that without treatment they expect the child ultimately to die. In this I consider them to be correct. From my own observations I believe that when this stage sets in, the liver has been so severely damaged that, unless the infection can be stopped, a fatal result must be anticipated. These cases do most definitely react to tartar emetic treatment if it is vigorously pushed. In the ordinary way, however, they usually slowly get worse, and in a variable period of time proceed to the third or last stage of the disease.

Fig. 5 shows the last stage of the disease as seen in these young people. It will be noticed that wasting is now much more evident and is well advanced. The child is very definitely ill and can only with difficulty be persuaded to stand up for its photograph to be taken. The tongue is heavily furred and the breath is foul. Anæmia is most marked and an irregular type of temperature comes into evidence, ranging between 99° F. and 101° F., which is quite unaffected by quinine. The epigastric pain is now most marked and causes much distress, and increases as the disease progresses. It will be noticed that the spleen has gradually increased in size during these stages so that it has almost come to dominate the picture. If there had not been so many cases in all stages to study, one might easily have come to the conclusion that this was a different condition to what is present in the earlier stages. The particular case had developed a considerable amount of ascites which, though common in the later periods, does not always occur.

Fig. 6 shows a case in the third stage, in which the patient lived a very short time after the photograph had been taken. Here emaciation has become extreme, but ascites is absent.

The temperature tends to remit a few days before death, and consciousness is retained nearly to the last.

On routine examination of the fæces of these cases in all the above-described stages, the ova of *Schistosoma mansoni* was found, frequently in such large numbers that it was only necessary to look at an ordinary smear preparation under the $\frac{1}{2}$ in. lens to see two or three eggs in every field. It was rarely necessary to search far.

Neither diarrhoea, nor the passage of blood are common symptoms. In most cases they are absent, in any case over long periods, although of course blood can be found microscopically. Intestinal complications generally are usually absent, no case of polypi and only one of prolapse having been seen.

The actual time that these three stages last is a very difficult point to determine. Admittedly these divisions are very arbitrary. I think, however, they are of value for descriptive purposes. That a large number of children do not go beyond the first stage can, I believe, be confidently stated, and these, when well advanced, constitute the chronic, or perhaps better, quiescent stage so commonly seen in the adults.

Fig. 7 shows a woman in a quiescent period. It will be seen that, physically, she appears perfectly fit, which is well borne out on examination. In spite of the enormous enlargement of the spleen there is no alteration in the blood as compared with other presumably healthy women of the same tribe. The liver, which retains its characteristic shape, is considerably harder than it is found to be in the earlier stages but is no larger than to be seen in many children. But the spleen now dominates the picture entirely. It is hard with a board-like hardness, and usually shows two deep notches dividing the median border into three lobes.

Fig. 8 represents another similar case. This woman again shows no signs of wasting, and had borne a large family of healthy children. She did not attend for this condition, and in fact it was rare for the patients to complain of any discomfort. As long as they do not suffer pain, or any definite disability, they view all their complaints with a very fatalistic eye. But although they appear healthy, it is doubtful if this condition is not indirectly fatal, by lowering their resistance to other infections. Although I saw on an average nine to ten thousand patients a year at the Karonga Hospital, and very large numbers when touring the villages in search of these cases, I was never able to find anyone who could be placed at over forty years of age presenting this form of splenomegaly.

Although it is impossible to obtain any definite dates or periods from these people, as they have little or no idea of time, and very fleeting memories, when questioned as to the length of time that they have possessed these enormous growths, they reply that the growth has always existed, or that it started in childhood, indicating in any case a long period of time.

Having had the opportunity of seeing so many cases in all stages and at all ages, it appeared impossible to doubt that these enormous splenomegalies, associated as they are with a very definite amount of enlargement of the liver, are the end-results of the cases so frequently seen among the children, in whom the re-infection with the schistosome has ceased before it has damaged the liver beyond hope of recovery.

Examination of the stools of these cases would always yield ova if sufficient search were made, but in many cases great patience and much time had to be spent upon it, in marked contradistinction to the ease with which they can always be found in the early, or what I have called the progressive stage of the disease.

Leave for only one very modified post-mortem examination was obtained. In this I was able to remove the liver and spleen. The liver showed an advanced stage of bilharzia cirrhosis. I do not propose to discuss the pathology of the condition as I had so little material available for the purpose, and this has been so ably done by Day, in Egypt, in a paper lately read before the Royal Society of Tropical Medicine and Hygiene.¹

While this one post-mortem examination was of use in clinching the cause of the enlargement of the liver, I had to base my diagnosis, and the connexion between the bilharzia cirrhosis and the splenomegaly, on epidemiological grounds. These I propose to discuss; the reason why the active stages are usually seen in child-

¹ *Trans. Roy. Soc. Trop. Med. and Hyg.*, 1924, xviii, pp. 121-130.

hood, and the quiescent stage in adults, depends on the local conformation of the country, and still more on the local customs.

As before mentioned, the tribe known as the Wankondies, living in the Karonga area, number about 25,000 men, women and children, and occupy an irregular area of about twelve square miles. The villages can be roughly grouped into three sections, depending on the available water supply. The inhabitants of the first draw their water directly from the lake, the second from the swiftly-flowing rivers, and the third from river beds dry in the summer, from which water can be obtained by digging.

On taking a census of the children suffering from the condition described, it was found that, with few exceptions, these were all in villages lying parallel with the lake shore, and drawing their water direct from the lake. Now, as the rainfall in this part of the world is not excessive, and only occurs during a period of three months, it is only along the lake edge that permanent marshland can be found. Examination of this area showed that the villages were situated on rising ground, separated from the lake by a deep depression full of thick vegetation, and holding stagnant water at all times of the year. The village paths lead through this swamp to the lake proper, as the inhabitants do not care to drink the water from these pools.

(Slides were shown to demonstrate this swamp.)

Search among the dense vegetation yielded large numbers of fresh-water molluscs, including one species of *Planorbis*, which has been identified as allied to *Planorbis sudanicus*, and experimentally was found to be the only species capable of acting as intermediate host to *Schistosoma mansoni*. Although non-operculated, it is very hardy, will breed readily under the most artificial circumstances, and in extremely foul water.

In the second section, where the inhabitants drew their water from the swiftly-flowing rivers, there are usually no permanent pools, and here the children were found unaffected. In a few places, however, where the river banks are low, permanent stagnant pools are again found and at once a certain number of cases could be found.

A careful examination of all these villages revealed the fact, that without the close proximity of the local *Planorbis*, neither the cirrhosis nor the splenomegaly would be found. On the other hand, all villages built close to stagnant water showed an extremely high incidence, chiefly among children. In a few places, however, one would find villages swarming with mosquitoes—indicating the presence of stagnant water which appeared to be free from this complaint. When search was made the water in these cases was found to be at the bottom of dense gullies, and therefore could not act as an infective agent.

While this explained the peculiar distribution, the reasons for the high incidence in childhood and for the sporadic cases in adults still remain to be cleared up. A study of the local customs, however, explained these points.

Local superstition demands that the act of defæcation must be an extremely private affair. Heavy penalties may be incurred if by accident a senior relative, such as a father-in-law, is viewed even going to or coming from the performance of this act. For this reason it is customary for adults to rise just before dawn and proceed some distance into the bush for this purpose. The young children are, however, not affected by these customs until they reach adolescence, which, however, is early with them. They defæcate at any time of the day, and in a country harbouring many dangerous animals they, of course, never go out of sight of the village. The edges of these pools, full as they are of tall rank vegetation, make excellent local cover, and examination of these places showed them to be consistently used by the children.

It is, therefore, easy to conceive that a child once infected, can infect the molluscs of the area he is accustomed to use, and at the end of twenty-eight days, when the

snail begins to shed cercariæ, he stands a chance of daily re-infection. I have kept experimentally infected molluscs alive up to 100 days under very indifferent surroundings, and no one that has not watched these infected snails shedding cercariæ can have any idea of the number produced.

Dr. Christopherson has shown that the adult flukes can live for a very considerable time, twenty-eight years being the maximum, I believe. It can thus be easily understood that, given the chance of daily re-infection with a long-lived parasite, the eventual production of ova will be enormous, and, by their wanderings in the liver tissue, these set up the fibrous tissue-formation at last to be found.

It might be objected that, as the adult natives use the flooded paths daily they would also become heavily infected. This puzzled me for some time, but I eventually found, by experimenting with the cercariæ in a small glass vessel, that if any considerable disturbance of the water was kept up, the cercariæ, whose swimming powers are very limited, would fail to get attached; if, on the other hand, the water was allowed to come to rest, a diminution in the number would be quickly apparent. For this reason I do not think that the use of the paths gave much opportunity for any, except occasional, infection. And this condition appears to require very repeated infections before the symptoms described are produced.

The distribution of what I have called the quiescent stage in adults, was much less restricted to any definite group of villages, and sporadic cases might be found anywhere. Careful questioning would, however, always bring to light the fact that their childhood had been spent in the infected villages adjacent to the marshes. These cases, without careful inquiry, might therefore lead to much confusion, especially as the spleen has by this time developed so much out of proportion to the enlargement of the liver, infection has ceased, and ova are more difficult to demonstrate in the stool. But taking into consideration the history of the cases and the facts enumerated, I do not think that there is any doubt that these are the end-results of the early childhood infections which have not progressed sufficiently to cause death and in which the repeated infections have been stopped by the child reaching adolescence and thus ceasing to use the adjacent marsh as a latrine. In fact all the intermediate stages can be found, from the early childhood cases, which are the key to the disease, up to the enormous spleens shown.

With reference to the relationship between the bilharzial cirrhosis and the splenomegaly, which has been doubted, no one working in this particular district could fail to draw the conclusion that they were part of one and the same disease. The incidence from village to village was always the same for both, and except for the sporadic cases in the adults, which I think have been satisfactorily explained, they were both absolutely dependent on the proximity of the breeding places of the necessary mollusc. The early cases are never variable in this respect.

If the splenomegaly were independent of the cirrhosis, it would be reasonable to suppose that, over such a large number of people, cases would be found presenting the typical splenomegaly without the cirrhosis. This however was not the case. Also, it might be supposed that the adjacent villages would be infected, as they are frequently only a few hundred yards apart. But unless the stagnant pools were sufficiently close to be used by the children, then the children were definitely free from both the cirrhosis and the splenomegaly.

The natives associate the two conditions, and are well aware that certain villages suffer while others are free, but of course they are quite unaware of the cause. They have the usual primitive man's fatalistic notions, and while accepting the fact that drinking dirty stagnant water would cause disease, they cannot conceive that any harm can possibly come from walking through it.

As regards the history of the disease, they do not associate bilharzia generally with the advent of the Arab slavers. They associate both yaws and syphilis with the arrival of the Arab, so that it is possible that bilharzia has been present for

some time in the country, as the vesical form exhibits sufficiently simple and definite symptoms to make identification easy.

I do not propose to advance any new theory as to why this progressive enlargement of the spleen takes place. It has of course been known and accepted for a long time in the case of the closely allied *Schistosoma japonicum* infection. In fact these heavily infected cases bear a very close resemblance to the classical description of *Schistosoma japonicum* infection, and I am inclined to believe that the two diseases are very closely allied in regard to symptoms, if a similar degree of infestation is taken in both cases. But it is usual to describe *Schistosoma japonicum* infection in terms of heavily infected cases and *Schistosoma mansoni* in those of a light infection.

Why, in these heavily infected people, the ova are deposited in the hepatic vessels and wander through the liver-tissue is hard to explain, as in this case they are bound to ultimate destruction. It is, however, due to this cause that the cirrhosis takes place, and presumably, indirectly, the splenomegaly, which always appears at a slightly later date, but once started is progressive. This would, in my opinion, point to a toxic cause rather than to any form of passive congestion.

The main points that impressed themselves on my mind during the two and a half years that I worked among these people were the following:—

(1) The long course of the disease, which is apparently due to the time taken for subjects to become sufficiently heavily infected. This of course is a saving clause, as they are only exposed to infection over a certain number of years, and therefore the mortality is thus limited.

(2) That until a very advanced stage of cirrhosis is reached no constitutional symptoms develop, and no changes take place in the blood. The blood appears also entirely unaffected by the degree of splenomegaly, as the later cases, with enormous spleens, show no change in the count. This may account for the fact that it has been found in Egypt that these cases stand the removal of the spleen well.

(3) Lastly, that although the cirrhosis becomes stationary when the chance of repeated infection is removed, the spleen continues progressively to increase in size. Although infection has ceased, these adults undoubtedly continue to harbour these blood-flukes, and the resulting increase in size of the spleen may be due to the continued toxæmia.

As regards treatment. It was difficult to get the patients to attend for the long course of treatment needed unless they had arrived at the stage when they were becoming definitely ill. It was therefore in the second and third progressive stages that treatment was mostly tried. In the second stage, when wasting had actually begun but was not excessive, treatment with full doses of tartar emetic, 30 gr. in all being given, was most satisfactory. Neither of the affected organs, however, was ever found to decrease in size, even when seen twelve months after treatment. The wasting and epigastric pain, however, disappeared, and the health appeared to return to the normal. If, however, the disease had advanced to the stage at which emaciation occurred, and an irregular temperature had set in, then the results were not so good. These cases, too, did not stand the full dosage required at all well, and altogether they were very unsatisfactory patients to treat. I fear I did not save the lives of many cases in the later stages.

Owing to lack of funds no attempt at preventive measures could be undertaken. The size and extent of the area involved was such that the cost of drainage would be excessive, especially as it would have to be repeated every year after the rains. Chemical destruction of the molluscs would also necessitate clearing the marsh of the dense vegetation. A partial attempt to sterilize the population was made, but the great difficulty was to get children in the early stages, when they are apparently healthy but highly infective.

However, in spite of the large numbers to be dealt with, I think a great deal could eventually be done in this way at a minimum cost.

There are so many vast areas in Africa in which the white population is sparse and which are so unproductive, that wholesale sanitary schemes are, and will be, for many years quite out of the question. Also, at the present time these people are still too primitive and retain too many important local customs to allow them to fall in with our European views. In Nyasaland an attempt was made to enforce the building of pit latrines, one per hut. In certain districts this was complied with, chiefly owing to the influence and popularity of the local administration. But examination of these latrines showed that, while in many cases they were well constructed, they had never been used. To fly in the face of age-long tradition, with the ever-present dread of being accused of bringing down calamity on the whole tribe, is, I fear, asking too much at the present moment.

We must wait for a future and better educated generation to spring up. At the present time these places offer a wonderful field for observation, owing to the steady increase of the native population and the large number of patients who will attend the hospitals and dispensaries as soon as the medical officer has gained their confidence.

Discussion.—Dr. H. B. DAY said that the clinical stages described by Captain Dye in Nyasaland were exactly the same as those in Egypt. In certain details, however, his experience of the disease was not identical. Thus, in Egypt they had the opportunity of seeing cases occurring in older children or young adults, and these generally gave a very definite and unprompted history of an onset with fever, and indeed frequently showed recurrences of fever in the later stages. That this fever was bilharzial in origin was proved by its disappearance under antimony treatment, while quinine remained ineffective.

Apparently cases of bilharzial dysentery were uncommon in Nyasaland. While in such cases in Egypt abundant bilharzial ova were found, they were generally scanty in cases of splenomegaly. It was this contrast that made the identity of the two conditions set up by *Schistosoma mansoni* infestation so puzzling in Egypt.

Alterations in the blood-picture were, in his (Dr. Day's) experience, always found in patients with marked splenomegaly and hepatic cirrhosis. There was a notable leucopenia and a distinct anaemia, which much decreased after splenectomy. He regarded the progressive splenomegaly as due rather to mechanical congestion than to a toxic effect of the worms. Otherwise it was difficult to explain why it should persist when the worms and their toxins had been removed by specific treatment, and also why it should not be a permanent feature of the closely allied *Bilharzia haematobia* infestation. The progressive cirrhosis that occurred in some cases in Nyasaland, where exposure to re-infection was unusual, was in harmony with his own experience. The last stages of hepatic cirrhosis often appeared to depend on some complicating factor, possibly intestinal in origin. At autopsy the signs of bilharzial infection were often absent, but the condition of the liver showed progressive degeneration and often recent inflammation. He had not had the opportunity of noting the effect of antimony treatment in many cases. While it seemed to have little or no effect in advanced cases, in early cases he had seen a striking diminution in the size of the enlarged viscera.

Dr. HENRY E. MELENEY (Peking, China) said it was very interesting to compare the epidemiological, pathological and clinical features of *Schistosoma mansoni* infection with those of Asiatic schistosomiasis. Some of the pictures which Dr. Dye had shown of the places at which the intermediate host of his parasite was found might very well be those of the habitat of the intermediate host of *Schistosoma japonicum*. In the lower Yangtze valley, as in the valley of the lower Nile, the disease was mainly a canal problem; whereas in the upper Yangtze valley, as in the region where Dr. Dye was working, the intermediate host was found mainly on the shores of lakes and ponds.

The slow development and chronicity of *Schistosoma mansoni* were in sharp contrast with the course of many cases of the Asiatic disease, some of which terminated fatally within a year. He (Dr. Meleney) believed this difference was due mainly to the difference in the amount of egg production by the two parasites. While the female *Schistosoma mansoni* had in its uterus at one time usually not more than two eggs (Fairley), the *Schistosoma japonicum* female might contain as many as 300, a large proportion of which she expelled at one time into

the mesenteric venules. This produced much more pathological change in both the intestine and the liver than the few eggs of the other parasite, and many more intense clinical symptoms, with fever, diarrhoea and emaciation.

In regard to the splenomegaly produced by the disease, he believed that the plugging of thousands of portal capillaries in the liver, with subsequent abscesses about the eggs, followed by scarring and contraction of the tissue, and obstruction of the portal circulation, was doubtless the most important factor in the splenic enlargement. Thrombosis of the portal vein, which sometimes occurred, was probably also a factor in some cases. Although a circulating toxin most likely existed in this disease, its rôle in the production of the splenomegaly was very difficult to estimate in the presence of other factors, and deserved careful study.

Dr. V. S. HODSON said that his remarks on the subject of bilharziasis were based on Sudan experience, where the opportunities for heavy infection did not exist as they did in Captain Dye's district in Nyasaland, or in Egypt. In his experience *Schistosoma mansoni* infections were, usually, only discovered in the northern Sudan on routine examination of the stools, and not because the patient made any complaint of symptoms suggestive of this disease. Of 1,290 hospital cases examined in the ordinary routine, 101 had been found to be infected with *Schistosoma mansoni*, but the great majority of these patients made no complaint of rectal symptoms. Of 247 schoolboys at the Gordon College, thirty-four were discovered to be carrying *Schistosoma mansoni* infection without symptoms, and of 586 boys, examined at Omdurman by a native observer, seventeen positives were reported and all without symptoms. He (Dr. Hodson) had recorded a case of enlarged liver and spleen, in which lateral-spined eggs had been found. The patient in this case came to hospital during a malarial epidemic, so quinine was given as well as tartar emetic. The liver was 2 in. and the spleen 4 in. below the costal margin. The girth was 26½ in. At the end of twelve months the patient was seen again and the liver was noted to be of normal size, the spleen half an inch below the costal margin and the girth 23 in. No serious diminution in the size of the spleen took place until some time after the cure of bilharziasis had been completed. It was clear that in early stages and in mild cases recovery was possible and could be complete, but it was certain that it took a considerable time for the patients to recover.

Dr. CHRISTOPHERSON said that Captain Dye's paper, taken with Dr. Day's recent work in Cairo, was important. There appear to be very good grounds for the view that Egyptian splenomegaly was schistosomiasis, or the result of schistosomiasis, and that it was not Banti's disease. The enlargement of the spleen and liver was probably due to the same cause in Nyasaland as it was in Egypt, both being the result of schistosomiasis infection.

It was a mistake to suppose that a chronic enlargement of the spleen in Africa was always malarial; it was frequently not malarial, especially when the liver was also appreciably enlarged. Enlargement of the spleen appeared to be a usual symptom of intestinal schistosomiasis, comparable with the enlargement of the spleen in *Schistosoma japonicum* infection.

Neither the adult worm nor the ova were usually found in the spleen in these cases, but this he (Dr. Christopherson) thought did not negative schistosomiasis being the cause. The spleen was enlarged in certain diseases, such as typhus and typhoid, the enlargement being due to toxin, and the enlargement both of spleen and liver in schistosomiasis was probably due to an intestinal toxin. It was interesting to know that early cases of bilharzial splenomegaly were amenable to antimony tartrate treatment; one would not expect the more chronic cases to respond to antimony. They were suffering from the indirect effects of schistosomiasis rather than from the direct effects, and would only respond if any worms or ova were present.

Capt. DYE (in reply) stated that his experience in Nyasaland was contrary to that of Dr. Day, inasmuch that no increase of temperature was found in the early cases, that is, in cases which, while showing marked enlargement of the liver and spleen, had not progressed sufficiently far to show wasting and anaemia. Also the adults in the so-called quiescent stage, with enormous spleens, showed no alteration in the blood-count. But it must be remembered that, owing to various circumstances, these cases in Nyasaland had ceased to be exposed to repeated infections and were only suffering from the end-results of the damage done in childhood. The course of the disease could undoubtedly be very much altered by environmental circumstances. He agreed with Dr. Christopherson that the enlargement of the spleen was probably due to long-continued toxæmia. He was interested to hear from Dr. Meleney that the condition as found in China, due to *Schistosoma japonicum*, was very similar in clinical appearances to that seen in Nyasaland. It was also interesting to know that in infection with

Schistosoma japonicum the size of the liver and spleen could be reduced by tartar emetic injections. This had not been found to be the case with the Nyasaland cases infected with *Schistosoma mansoni*. It was not found that there was any difference between the number of males and females infected. Both were equally attacked, but this, of course, depended on local conditions, and the rate of attack might vary from place to place.

Blood-smear showing Experimental Infection with *Herpetomonas*.

Shown by H. W. FANTHAM, D.Sc.

(Professor of Zoology, Witwatersrand University.)

PROFESSOR H. B. FANTHAM exhibited and made remarks upon a blood-smear taken from a rat which he had experimentally infected, by intraperitoneal inoculation, with *Herpetomonas muscae domesticæ*. The rat was bred in the laboratory and was clean, as it had been kept isolated in an insect-proof cage from the time that it was able to feed itself. The herpetomonad flagellates for inoculation were obtained from an emulsion in normal saline of the gut-contents of a house-fly naturally infected with *Herpetomonas muscae domesticæ*. The blood-smear shown was made on the thirty-fifth day after inoculation, when a relatively good infection with the flagellates occurred in the peripheral blood of the host.

Daily examinations, at least, were made of the blood of the experimental rat. A few young, developing, flagellate herpetomonads were seen in the ear blood on the ninth day after inoculation. Possibly there was a sparse infection of the rat on the twenty-fifth day, but the infection was too slight to be confirmed from stained preparations. However, on the thirty-fifth day, a relatively heavy infection of the flagellates was found in the morning, at the usual time of routine examination of the rat's blood. In the afternoon of the same day, four hours later, the blood was again examined, but no parasites were then seen. The infection had fulminated and then faded out, for the results of subsequent daily examinations were negative, up to the end of 315 days after inoculation, when the rat appeared to be in good health, and cultures of its blood were negative for flagellates.

In such experiments, after inoculation it is necessary to examine the host's blood daily, and in the early days of the experiment twice, three, or even more times daily. Similar frequent examinations should be made on days on which herpetomonad parasites are found, as the infections may be slight and evanescent.

It is now well known that the various species of *Leishmania* develop into typical herpetomonad flagellates in cultures. These flagellate stages have also been seen by various observers in leishmaniasis in man.

Vertebrates other than man can be infected naturally with herpetomonads. In 1903 Dutton and Todd described herpetomonads from the blood of house mice in Senegambia. In 1915 Fantham and Porter recorded the natural occurrence of herpetomonads in a very few mice in England. Balfour mentioned a similar occurrence in the gerbil of the Sudan. In 1907 the Sergents recorded a natural infection of herpetomonads in birds, and in 1914 the Sergents and their collaborators demonstrated the presence of herpetomonad flagellates in the blood and organs of geckos in Algeria. Other examples might be quoted of the natural occurrence of herpetomonads, not only in the blood, but also in the gut of certain vertebrates.

Herpetomonads are known to be derived from the digestive tracts of various invertebrates, especially insects. In conclusion, it may be stated that the various species of *Leishmania* are, in all probability, herpetomonads of invertebrates (such as insects) that have been able to adapt themselves to life in vertebrates.

Some Flukes bred from Cercariæ recurring in *Schistosoma*-transmitting Molluscs in South Africa.

Shown by ANNIE PORTER, D.Sc.

(Parasitologist, Witwatersrand University.)

DR. ANNIE PORTER exhibited stained specimens of adult *Fasciola hepatica*, *Fasciola gigantica*, *Echinostomum fulicæ* and *Amphistomum calicophorum*, bred in the laboratory by her from cercariæ obtained from various South African molluscan transmitters of *Schistosoma hæmatobium* and *Schistosoma mansoni*. Specimens of adult *Schistosoma hæmatobium* and its cercariæ, and shells of the South African molluscs concerned in the transmission of both these human schistosomes were also shown and a few remarks were made.

In South Africa, the freshwater snails transmitting *Schistosoma hæmatobium* are *Physopsis africana*, *Physopsis conicum* and, very occasionally, *Limnæa natalensis*. The intermediate hosts of *Schistosoma mansoni* are *Planorbis pfeifferi*, *Physopsis africana* and *Isidora tropica*. Most of these species of snails can and do harbour other species of cercariæ, sometimes as a single infection, sometimes jointly, as mixed infections with *Schistosoma hæmatobium* and *Schistosoma mansoni*. Thus, from *Physopsis africana* no less than twenty-two morphologically different cercariæ have been obtained, of which the complete adult phases of four only have been ascertained, and the same condition prevails in the other snails mentioned.

Physopsis africana, the principal South African host of *Schistosoma hæmatobium*, also sometimes contained cercariæ of *Schistosoma mansoni*, of *Echinostomum xenopi*, parasitic in the clawed toad, *Xenopus laevis*, and of a monostome, parasitic in the same amphibian. Double infections of *Physopsis africana* with *Schistosoma hæmatobium* and *Schistosoma mansoni* have been found.

Limnæa natalensis, a rare South African host of *Schistosoma hæmatobium*, is also the intermediate host of the large African cattle fluke, *Fasciola gigantica*, and of the so-called "European" cattle- and sheep-fluke, *Fasciola hepatica*, and of *Echinostomum xenopi*.

Isidora tropica, one transmitter of *Schistosoma mansoni*, is also a molluscan host of *Fasciola hepatica*, of the stomach-fluke of cattle, *Amphistomum calicophorum*, and of *Echinostomum fulicæ*, a natural parasite of the red-knobbed coot, *Fulica cristata*. *Echinostomum fulicæ* has another invertebrate host, the mollusc, *Tomisia ventricosa*, but so far this snail has not been connected with the transmission of bilharziasis.

The ramshorn snail, *Planorbis pfeifferi*, is the principal transmitter in South Africa of *Schistosoma mansoni*. Whilst it also harbours other cercariæ, the adult host of these has not so far been ascertained experimentally. A number of other cercariæ, of which the adults are as yet unidentified, also occur in *Limnæa natalensis*, *Isidora tropica*, and other South African molluscs.

In determining the vertebrate hosts of various cercariæ, laboratory-bred animals—rats, rabbits, guinea-pigs, goats, sheep and ducks—whose diet had been carefully controlled, were exposed to infection by natural routes, such as bathing in or drinking water containing cercariæ from infected molluscs, or ingesting herbage on which encysted cercariæ were present; and various adult flukes were eventually obtained from them.

It was pointed out that several genera of snails were concerned in the transmission of the two human schistosomes, *Schistosoma hæmatobium* and *Schistosoma mansoni*, in South Africa, and that the same applied to other species of Trematodes. Also, that as

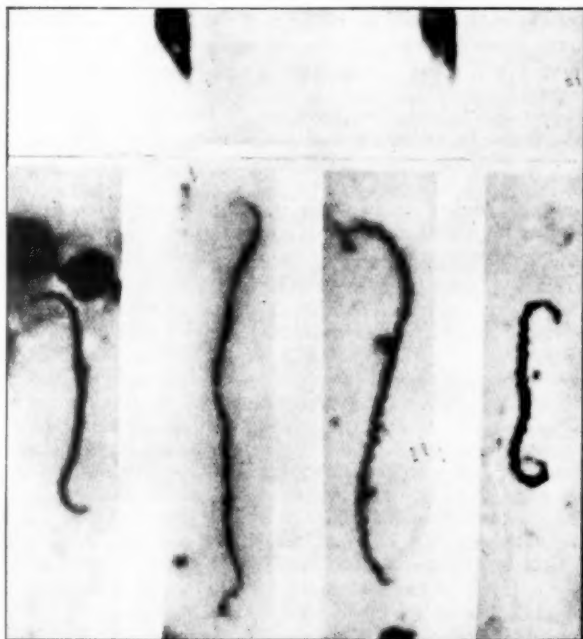
hybridization occurred between different species of snails, more hosts would probably be found as time went on, while there were a very large number of larval stages (cercariæ) known; the adult phases of which needed experimental determination.

Leptospiræ in Tap Water.

By A. S. BURGESS, M.D.

(Pathologist, Gold Coast.)

THE photomicrographs reproduced are of leptospiræ obtained from the tap water of Accra, Gold Coast, and stained by the Warthin-Starry process.



LEPTOSPIRÆ FROM TAP WATER.

Photomicrographs ($\times 4,000$) of leptospiræ from the tap water of Accra, Gold Coast, stained by the Warthin-Starry process. The lower print shows a $10\ \mu$ division of a stage micrometer at the same magnification. The "pitch" of the spirals is as follows: No. 1, (left) $0.47\ \mu$; No. 2, $0.59\ \mu$; No. 3, $0.35\ \mu$; No. 4, $0.37\ \mu$.

The organisms were found in the centrifugalized deposit from water straight from the tap, in the scrapings from a filter candle, through which tap water had percolated for twenty-four hours, and also in the upper and lower chambers of a household filter. But they were very scanty, there being only one or two in a fresh film under a $\frac{1}{8}$ in. cover-glass. They were somewhat more numerous in a bottle, which had been replenished daily with filter water for two or three weeks. There was a scanty

[March 2, 1925.]

brown deposit in the bottle and the organisms were found in the deposit. Those photographed were obtained from this source.

Specimens were prepared by spreading films of the deposit on albuminized cover-glasses, fixing while wet with osmic acid vapour, drying and staining by the Warthin-Starry silver-agar process. Fresh films were also examined with dark-ground illumination.

The organisms are of interest on account of their morphological resemblance to *Leptospira icteroides* and *Leptospira icterohæmorrhagica*. Their form is that of a close spiral, the axis having usually a straight middle portion and hooked extremities. Their length, measured between the convexity of the curved ends, ranges from $5\ \mu$ to $17\ \mu$, with an average of $10\ \mu$, and the "pitch" ranges from $0.35\ \mu$ to $0.6\ \mu$. (The pitch of *Leptospira icteroides* is given as $0.5\ \mu$.) In the dark field the organisms show all grades of activity. The most active rotate with great rapidity and persistence, but progress very little. When in motion their spirals cannot of course be seen, but the organisms can be recognized by their hooked extremities. Even with resting organisms the spirals seem, in some cases, to be too close to be made out clearly, except along the convexities of the terminal bends, where they are, so to speak, opened out.

Cultures and animal inoculations have not yet been attempted. As the leptospiræ have only been found in association with a variety of other micro-organisms, and then only in very small numbers, such experiments would, no doubt, be attended with difficulty.

The discovery of leptospiræ in water is not new. Buchanan (*British Medical Journal*, November 29, 1924) mentions a number of observers who have found them in tap and pond water, and Coles (*English Mechanics and World of Science*, November 2 and 9, 1923) has recorded their occurrence in tap water in England.

Section of Urology.¹

President—Dr. W. LANGDON BROWN.

A Lesion Resembling "Paget's Disease of the Nipple" occurring on the Penis. ("Malignant Dermatitis.")

By FRANK KIDD, M.Ch.

History.—Patient, H. E. V., aged 55, first seen August, 1923, suffering from severe strictures. Internal urethrotomy performed. A patch of "chronic eczema" noted on penis, which in one or two places showed a warty excrescence. A portion of the patch was removed for section, the report on which is given below. The whole area was treated with sparking from a diathermy machine. At first a portion of the area skinned over as the result of this treatment, but the lower end has been spreading gradually and taking on a warty appearance.

Histological Report.—"This little mass consisted of an outgrowth of thickened and altered squamous epithelium in the form of papillæ. The base is intensely inflamed, the tissue being thickly infiltrated by small round cells. This complicates the histological picture very considerably and obscures the outline of the bases of the epithelial papillæ. This apparent indefiniteness gives the whole a suspicious appearance and in fact I do not consider the available evidence warrants a positive assertion as to the nature of the lesion. I am personally rather strongly inclined to the view that it is a simple 'wart' of inflammatory origin and not malignant."

This patient exhibits a very interesting condition on the penis, which I have been watching for six months; it has been present four years. Dr. Sequeira, in charge of the Skin Disease Department at the London Hospital, agrees that it exactly resembles a case we both saw in 1904 of malignant dermatitis of the penis, resembling Paget's disease of the nipple. The latter patient died in hospital of another disease and the *post-mortem* examination revealed a small secondary deposit in the bulbous tissue of the urethra. The present patient has been undergoing treatment for a stricture, and I have tried the effect of diathermy. For a time the condition abated, but recently it has become decidedly worse, warts having appeared on the spreading edge. A week ago I told the patient that the time had now come for amputation. The patient went home and smeared patent ointment on it. The oil constituted a film, which covered over what lay beneath and completely spoilt the case for exhibition. Beneath the ointment there lay a raw red patch the size of a half-crown, very slightly raised above the surrounding skin of the glans penis. Its margin was sharply defined, and was gradually spreading so as to invade the frænum. Here the edge was slightly serpiginous. The margin was distinctly hard to the touch. From time to time warts appeared near the margin and fell off naturally. I have once previously seen a similar condition on the penis, and Dr. Sequeira has seen two such cases.

I have always regarded Paget's disease of the nipple as from its earliest stage a very chronic epithelioma of the superficial layers of the epidermis. From the

¹Continued from *Proceedings*, 1924, xvii (Section of Urology), p. 60.

2 Thompson: *Papilloma of Renal Pelvis*; White: *Renal Calculus*

nipple it spreads very slowly to the deeper parts of the breast, and I have seen secondary nodules in the deep lymphatics of the breast tissue itself. The cells in the secondary deposits are small and packed together inside a basement membrane and do not show any appearance of cell nests. There must be some peculiar specific difference between this type of chronic superficial epithelioma or malignant dermatitis and the ordinary epithelioma of the skin, whether of the nipple or penis or elsewhere. This type is hardly raised above the surface of the surrounding skin, and it has a hard edge which can be felt rather than seen. The clinical course extends over many years.

Papilloma of Renal Pelvis.

By RALPH THOMPSON, Ch.M.

I AM showing this case because the papilloma was nearly missed. The man was bleeding too extensively for cystoscopy to be performed, as he was very ill. A skiagram showed some evidence of calculus in the left kidney, therefore I cut down on the left side and found the kidney apparently normal; and I directed my assistant to make an incision into the kidney, but nothing could be seen in it. Then I asked him to make the incision a little longer, towards the upper pole. He did so, and at once a papilloma appeared. It was bright scarlet in colour, and looked like a papilloma of the bladder. I therefore sutured the kidney and removed it. That was done a year ago. I saw the patient again at the beginning of May, having in mind this meeting of the Section, and he was fit and well, without any signs of recurrence.

The reason why I sutured the kidney was, that three years ago I had a case of pyonephrosis of the kidney, and the patient (a female) had a recurrence six months afterwards, in the scar—a definitely infiltrating papilloma. That was removed, and six months afterwards she had a recurrence on the aorta. That I also removed, and later there was some involvement of the spinal column, judging from the symptoms. Therefore I sutured the kidney so as not to run the risk of papilliferous fragments getting access to the wound. These conditions are sometimes missed from lack of performance of an excision exposing the pelvis of the kidney.

DISCUSSION.

Mr. JOCELYN SWAN asked whether the skiagram showed the shadow of a stone in the kidney and whether a calculus was found. Mr. Thompson said the bleeding was too severe to permit of cystoscopy; but surely even in these severe cases one could cystoscope sufficiently to see from which side the blood was coming. It was not like vesical hæmorrhage.

Mr. RALPH THOMPSON (in reply) said that he did not mean cystoscopy was not possible, but that he would not do it; he did not feel disposed to subject the patient to it, as he was so ill. Yet something in the way of operation was plainly indicated, and the result of what he did showed that he could not have been far wrong. The shadow appeared to be in the pelvis of the left kidney.

Renal Calculus Complicating Hypernephroma.

Shown by H. P. WINSBURY WHITE, F.R.C.S.

THIS specimen was obtained from a man, aged 31, who came complaining of a urethral fistula.

Two years previously he had suffered from a peri-urethral abscess. This was incised, and a urinary fistula resulted. In the intervening period two operations were performed on the fistula; upon the second occasion the bladder was opened as

well. The resulting benefit was only temporary, as after each operation healing was incomplete, and the fistula persisted.

When I saw the patient there was a considerable amount of scar tissue and inflammation about the perineum. It was expected that the passage of a sound would reveal a tight stricture. A fairly large-sized sound was, however, readily admitted, although the presence of stricture was detected. It was hoped that intermittent dilatation of the stricture would be sufficient to remedy the perineal condition. This line of treatment was therefore followed for five or six months, but the fistula failed to heal up. Cystoscopy was then carried out, when turbid urine was seen coming from the left ureter. A radiogram revealed a stone in the left kidney. I operated on the kidney and removed from the pelvis an oxalate stone about the size of a filbert. A bluish bulging area having the diameter of a shilling piece was then noticed on the anterior aspect of the kidney at about its middle. On palpation with the forefinger in the pelvis it was noted that this area was somewhat softer than the rest of the organ. This was considered a sufficient reason for performing nephrectomy. On opening the kidney afterwards an encapsuled growth with a diameter of $\frac{3}{4}$ in. was seen.

The water-colour drawing of the specimen projected on the screen reveals several interesting features. It will be seen, for instance, that the tumour is exceptionally small, and is situated much nearer to the pelvis than such tumours generally are. It appears to be completely encapsuled.

Dr. Scott Williamson describes the growth as a typical hypernephroma which has already infiltrated its capsule.

It is interesting to note that the patient has never had pain in the left loin, nor has he had any hæmaturia, apart from an odd drop of blood after the passage of a sound. The most striking feature of the case, however, is the fact that the persistence of the fistula was due entirely to the infected urine, arising from the presence of the renal stone. From the appearance of the kidney it can be seen that the infective changes are very slight, and confined to the pelvis. Yet a fortnight after nephrectomy was performed, the perineal fistula, which had resisted all forms of local treatment for two years, was completely healed.

DISCUSSION.

Mr. C. A. R. NITCH said he was very interested in this class of tumour. Some time ago he searched the literature in regard to them, and saw all the specimens he could. He did not remember having seen a specimen like this one, with the adrenal tumour limited to the medulla. The majority of those he had seen were primarily in the cortex, though they might have invaded the medulla. He would be glad to hear of any cases originating in the medulla. He thought these must be looked upon as malignant tumours, for very often secondary deposits were found, in the bones for instance, long before the primary tumour had given rise to symptoms causing its presence to be suspected.

Mr. JOCELYN SWAN said he could give several examples of hypernephromata beginning as small tumours in the medulla. One was the case of a tumour about $\frac{1}{2}$ in. in diameter, which was found after death in the kidney of a patient who died of epithelioma of the tongue. That tumour was imbedded in the kidney medulla. Another specimen, which he had removed recently, occurred in a patient who had hæmaturia, and blood was seen coming down from her left ureter. He explored the kidney, and on getting it up into the wound, he found a tumour projecting apparently into the situation of the pelvis. At first he thought it was a large tumour of the renal pelvis, but on further separating out, the renal pelvis was found to be merely pushed aside by a tumour projecting at the hilum between the renal pelvis and the vessels. He removed the kidney. It was a pure hypernephroma, starting from the medullary part of the kidney; he had several other specimens in the Cancer Hospital in which the growth originated in the same site.

As to these tumours being malignant, though they showed suprarenal structure, one case under his care had shown absolutely the suprarenal type of tissue, apart from the hemor-

4 Thompson: *Cystic Disease of the Kidney; Ureteric Calculi*

rhage, and three months later that patient had a recurrence in the crest of the ilium. He had seen other instances in which recurrences had been fairly widespread, and the growths showed the same tissue as the original tumour.

In the case of hypernephromata he thought one was dealing with more than one kind of tumour. Some were definitely localized, apparently encapsuled. Some he had operated upon fifteen years ago, and the patients were still alive, and apparently well, having had no recurrence. In other examples, with a similar appearance microscopically, there was no encapsulation, the growths infiltrated renal tissue, the patients did badly, and recurrences took place.

Mr. Winsbury White's patient was very young to be attacked with hypernephroma; in no case in his own series was the patient under 51, and the average age in a series of cases was about 55. One of his colleagues had had a case of a tumour in the supraclavicular region; the tumour was removed, and upon examination showed definite hypernephroma structure. The patient was a female aged 37.

Mr. RALPH THOMPSON said that in the post-mortem records at Guy's Hospital for twenty years, of seventy cases of growth of the kidney none occurred between the ages of 20 and 30, but there were some under 20 and some over 30.

Stones from Cystic Disease of the Kidney. (Two Cases.)

Shown by RALPH THOMPSON, Ch.M.

Case I.—A large number of calculi were removed from the kidney of J. R., a middle-aged male, in August, 1917. There had been vomiting and great pain, with some hæmorrhage. He had at the time some uræmia and the blood urea was high. The man was so exhausted from the bleeding that he asked for an operation. Accordingly I cut down on the left kidney and found stones in its pelvis; I did pyelotomy and removed them. But the kidney was evidently cystic. Patient was kept under observation at Guy's out-patient department for two years and then he disappeared. He returned in November, 1922, and both kidneys could be felt. The urea-concentration test was to have been done, but the man developed a carbuncle. He died that month. There appeared, post mortem, to be very little tissue kidney left to the naked eye; both kidneys were cystic. The case shows what I pointed out a year or two ago, that, in a number of cases of cystic kidney, the patients do not die of their cystic disease. This man lived for five years after the operation.

Case II was that of a woman who was sent to me. I operated and found the left kidney cystic and full of stones, and she died in a year.

I should be glad to learn the views of Members as to the prognosis in cystic disease complicated by stone. When uncomplicated by stone, I think the average age at death is nearly 60, and as a rule the patients die of something else.

Ureteric Calculi.

By RALPH THOMPSON, Ch.M.

THESE cases illustrate what I regard as an important anatomical fact. When one goes through a large number of figures of cases it is found that growth, stone and tubercle are more common on the right side than on the left. And it is a very interesting fact that, in the proportion of six to one in cases of ureteric calculi, in which the clinical symptoms of those calculi have been quite pronounced and the patient has passed the stone *per viam naturalem*, the stone has apparently been passed from the left side; and of the cases upon which I have operated for impacted ureteric calculi, 75 per cent. have been on the right side. I therefore suggest it may

be a most important point in the question of the treatment of these cases as to whether one would be justified in injecting the ureter with oil more frequently on the left side than on the right side. Stones seem to be more easily passed along the left side than along the right. The relations of the right ureter differ from those of the left; the liver and duodenum come into relationship with the kidney itself, the liver pressing on the upper half of the pelvis of the kidney, the duodenum pressing on the lower half and the upper part of the ureter; the mesentery crosses the right ureter, the caecum and appendix come into relationship with the right ureter, the right lumbar glands are more in relationship with the right ureter than the left lumbar glands with the left ureter, as Barclay Smith pointed out; and the only important structure which crosses the left ureter is the rectum. And if the cases of ascending unilateral nephritis are analysed it is found to be more common on the left side than on the right. One finds out in this way how important a guide is anatomy in arriving at a prognosis, and especially in regard to treatment by oil injection.

Papilloma of Ureter with Hydronephrosis.

Shown by E. T. C. MILLIGAN, O.B.E., F.R.C.S.

THESE specimens were removed at autopsy by Dr. Davis from a patient who had been under treatment and observation for two years for carcinoma of the tongue. Broncho-pneumonia caused the patient's death at 70 years of age.

In the middle of the right ureter there is a small papilloma with the base about $\frac{1}{2}$ in. in diameter, and the growth projecting for $\frac{1}{2}$ in. into the lumen of the ureter. Above the growth the ureter is dilated and the pelvis of the kidney greatly distended, only about $\frac{1}{2}$ in. of shell of kidney substance remaining.

Such a specimen is not uncommon, but clinical interest lies in the fact that this condition gave rise to no symptoms during life. The patient, according to the history he gave, had noticed nothing abnormal in the urinary tract, nor was anything noticed by those who had him under observation for two years.

Extensive Carcinoma of Large Bladder Diverticulum.

Shown by E. T. C. MILLIGAN, O.B.E., F.R.C.S.

THE diverticulum containing the growth is 5 in. in diameter, lying deeply in the pelvis and communicating with the bladder by a stoma $\frac{1}{2}$ in. in diameter, just behind and lateral to the right ureteric orifice. The growth is a carcinoma with irregular villous, shaggy, sloughing surface, covering all but about one-eighth of the internal surface and almost filling this large diverticulum. The bladder and diverticulum were grossly infected.

The specimen was removed from a man aged 60, who had suffered from intermittent hæmaturia, frequency of micturition, and pyuria, for two years.

The outer aspect of the diverticulum was found to be firmly and extensively adherent to the extra-peritoneal tissues in the pelvis; and although it was felt that a cure of the cancerous disease was unlikely, it was considered that much benefit would be derived by exclusion of the growth from the bladder by removal of the diverticulum.

The patient made a satisfactory recovery after operation, the suprapubic wound closing in four weeks. Three months afterwards, however, growth could be felt in the right lateral aspect of the pelvis and seen on the right lateral walls of the bladder. A permanent suprapubic fistula was then established in order to alleviate his symptoms.

Cyst of the Urachus.

Shown by E. T. C. MILLIGAN, O.B.E., F.R.C.S.

THE specimen is a rounded cyst, 5 in. in diameter, containing 5 or 6 dr. of dark brown fluid. The walls are thin and rigid, with flat calcareous and bony deposit.

Histological examination shows that the walls are formed of fibrous tissue with much calcareous deposit, and the inner surface covered by flattened, much degenerated epithelium.

The specimen was removed from a woman, aged 70; it had been diagnosed thirty years before as malignant peritoneum, and years later, since the patient was still alive and well, as a calcified fibroid of the uterus.

The cyst was situated in the sub-peritoneal tissues, between the symphysis pubis and the umbilicus. It did not shell out easily but had to be dissected from adherent surrounding tissues. Removal was undertaken on account of pain having become a troublesome symptom.

DISCUSSION.

Mr. RALPH THOMPSON said that two years ago he had had a case of urachal cyst, which was incised and drained. Dr. Nicholson found a colloid carcinoma, and another pathologist found tubercle-bacilli. The patient was still alive, and showed no sign of colloid carcinoma. The man said he had passed colloid material, *per urethram*, ever since he could remember. He (the speaker) asked if Mr. Milligan was certain this specimen was a urachal cyst, as he was not sure whether his own case was not one of diverticulum of the bladder coming into position where one might expect a urachal cyst to be present.

Mr. FRANK KIDD said that these cysts usually contained viscid material, turbid and opaque, and probably Mr. Thompson's case passed such material into the bladder from the cyst. Surely all urachal cysts were in essence diverticula of the bladder, those higher up being cut off from their connexion with the bladder. The bladder represented the proximal portion of the allantois, and urachal cysts and fistulæ represented persisting remnants of the more distal portions of the same structure.

Mr. RALPH THOMPSON said he did not agree with Mr. Kidd's last remark. According to the anatomists, the allantois had nothing to do with the bladder coming up in that region. He thought there might be a diverticulum of the bladder with the urachus in the normal position.

Mr. MILLIGAN (in reply) said that the cyst had no apparent connexion with the bladder; he could not obtain a good histological section, but from its characteristics he could not regard the cyst as being anything else than a cyst of the urachus.

Postscript.—Sir Arthur Keith, after a full investigation of the tumour, reports that in his opinion it is a blood cyst, the internal lining being composed of flattened cells. Calcification of the wall is also more likely in a blood cyst.

Section of Urology.

President—Mr. CYRIL A. R. NITCH, M.S.

PRESIDENT'S ADDRESS:

Renal Tuberculosis.

By CYRIL NITCH, M.S.

(ABSTRACT.)

[This address is printed in full in the *Lancet*, January 3, 1925, p. 1.]

THE PRESIDENT pointed out that, as limitations of time would not permit of a complete review, he had selected certain aspects of the disease, some of which he knew were open to differences of opinion. The details he was bringing before the Section were based on complete records of sixty-five cases.

After referring to the clinical varieties and pointing out the differences between the medical and surgical forms, the paths of infection and the pathology of the surgical varieties were discussed in detail. Mr. Nitch inclined to the view that in a great majority of cases the path of infection was by way of the blood-stream, and that the primary lesion was situated in the cortex; thence it was carried to the base of the papillæ, where caseation and extension towards the pelvis gave rise to the well-known papillo-calycular ulceration. The treatment was early nephrectomy, combined in many cases with total ureterectomy. The results of nephrectomy alone, and of nephrectomy combined with ureterectomy, were summarized in three tables, which appeared to point to the greater benefit from the major operation. The late results were instructive, for twelve per cent. of the patients died two and a half years after the operation, fifty per cent. were cured, and ten per cent. still had urinary trouble.

In pleading for an early diagnosis Mr. Nitch pointed out the importance of regarding all cases of mild "cystitis" with suspicion, and of carefully examining a sterile, purulent urine for tubercle bacilli.

Discussion.—Sir JOHN THOMSON-WALKER said that a number of points in pathology had been raised that would give ample scope for discussion. He said that Mr. Nitch suggested that the earliest deposit of tubercle in all cases of surgical tuberculosis of the kidney took place in the cortex. This did not agree with the naked-eye observations that one made in specimens removed by operation. The earliest visible deposit was at the apex of a pyramid, and from this the tuberculous process spread outwards towards the cortex. A tuberculous collection might be found shut off from the renal pelvis and apparently isolated in the renal tissue. Careful examination would show that there had been a complete obliteration of the neck of a calyx, or of a division of the pelvis by the development and contraction of fibrous tissues. A condition short of complete obliteration might sometimes be seen when there was extreme narrowing at the outlet of the calyx with ulceration of the pyramid within. On a grand scale this occurred at the outlet of the renal pelvis, and it brought about what was known as the natural "cure" of renal tubercle by isolation of the kidney from the rest of the urinary tract. As the destruction of the pyramid proceeded outwards, small grey tubercles might be seen at the periphery of the ulcerating area. These outposts from the spreading margin might be visible at the periphery of any active focus of tubercle. They were well seen with the cystoscope in tuberculous ulceration of the bladder. They did not indicate a spread from periphery to centre but the opposite, and in the kidney did not prove a cortical origin of the tuberculous deposit.

The infection of the second kidney, by a spread down the lymphatics of one ureter across the bladder and up the lymphatics of the second ureter, had not, he thought, been proved. If a lymphatic route of infection to the second kidney took place there was a more direct path

by the intercommunication of the lymphatics of each kidney. But if the first kidney was (as most authorities held, with whom Mr. Nitch agreed) infected by way of the blood-stream, one would expect to find some difference in the gross pathology of the second kidney infected by way of the lymphatics. But the two kidneys were indistinguishable so far as this path of infection was concerned. In his (the speaker's) experience if the said cases were seen soon enough the diagnosis could be made at a very early stage. He had removed a kidney in which there was a single, very small tuberculous area at the apex of one pyramid. The difficulty was that the cases were not submitted to expert examination soon enough. Insufficient care was exercised by those in charge of cases of slight cystitis or passing hæmaturia. A more complete examination of such cases would lead to practically all cases being brought to operation in the early stage. This brought up two very interesting points in diagnosis: One was the value of the discovery of tubercle bacilli in the urine on one or two examinations without any other pathological elements, that is, without pus, blood, casts, or albumin. He (Sir John Thomson-Walker) had seen several such cases in which the problem of operation had arisen; the tubercle bacilli had been undoubtedly present on one or two occasions, but were not discovered on subsequent investigations. His view was that in these cases the tubercle bacilli were excreted from some focus elsewhere in the body and that there was no tuberculous disease of the kidney itself; no further sign of urinary tubercle had developed after several years. The absence of the tubercle bacillus in the urine in some cases of urinary tubercle was another important point. From time to time he had to diagnose urinary tubercle from the clinical signs or the cystoscopic examination when repeated examination of the urine by smears and animal inoculation had failed to discover the tubercle bacillus. Some were cases of "closed" tubercle of the kidney. The lumen of the ureter was obliterated and the kidney was completely isolated from the urinary tract, but in other cases there were tuberculous lesions in the bladder without bacilli in the urine.

In regard to treatment, the removal of one of two tuberculous kidneys was recommended in some cases. If this was done at all, it could only be justified in rare cases, when one kidney was the site of advanced tubercle and was causing severe local or general symptoms, and the other kidney was very recently and slightly affected. If cases of bilateral renal tuberculosis were followed up it would be found that, apart from intercurrent disease, death took place either from suppression of urine owing to the destruction of renal tissue, or from chronic toxæmia due to absorption of tuberculous toxin. Of these two, suppression of urine was the more common. It was generally agreed that tubercle of the kidney was a progressive disease, ending in all but the rarest cases in total destruction of the organ. By the removal of one of two tuberculous kidneys the second kidney was relieved of the irritating effect of excreting the tuberculous toxin, but at the same time a large amount of secreting kidney tissue was removed, and by so doing one hastened the time when suppression of the urine would take place without, as was generally admitted, anything being done to stop the process of the disease in the second kidney. In his (the speaker's) opinion, there were very few cases in which the removal of one of two tuberculous kidneys was justifiable. His practice in operating for tuberculosis of the kidney was to remove the kidney with the commencement of the ureter, below the lower pole of the kidney, and close the wound entirely. After six or twelve months, if it appeared that the ureter was the source of continued infection of the bladder, it was removed in its whole length by a median incision and extra-peritoneal dissection. The number of ureters that required to be so dealt with was very small. In the great majority of cases the ureter shrank and caused no further trouble.

Mr. S. G. MACDONALD said he considered that a disappointingly large number of cases were bilateral when they first came under observation; this was especially so in children. He did not think cases in which the remaining kidney subsequently became tuberculous should be as common as the President suggested. He had had only one return case; this occurred seven years after nephrectomy, and in the interval the urine was free from pus and tubercle bacilli. As, during the interval, the patient was operated upon elsewhere for a tuberculous abscess in the abdomen, it was possible that this was a case in which the second kidney became tuberculous *de novo*. He did not now do a nephrectomy until a guinea-pig had been inoculated from the second kidney. He agreed with Sir John Thomson-Walker as to the method by which an "open" could be converted into a "closed" renal tuberculosis; this was supported by the appearance seen in a kidney he had removed, in which the upper half of the kidney was destroyed by tuberculosis and shut off from the urinary tract; the lower half was healthy, so

that the patient had an efflux from both kidneys and the urine was free from pus and tubercle bacilli. Diagnosis in such a case depended upon the "dragged out" ureter found on cystoscopic examination. This occurred only in renal tuberculosis. The question whether tubercle bacilli could occur in the urine without urinary tuberculosis had been settled by Phillipowitz (*Wien. Med. Blätter*, 1883) from clinical and post-mortem evidence; the case cited by the President would appear to come under this heading, since no kidney had yet been produced showing healed tuberculous scars.

As regarded treatment of the ureter at the nephrectomy, he divided the ureter as low as could be done conveniently from the nephrectomy wound and he had found this sufficient. Sir John Thomson-Walker referred to cases diagnosed as tuberculous with the cystoscope, in which tubercle bacilli could not be demonstrated, and he (the speaker) had three such cases under observation at the present time.

CLINICAL AND PATHOLOGICAL MEETING.

Suprarenal Tumour, Carcinoma.

Shown by W. LANGDON BROWN, M.D., and W. GIRLING BALL,
F.R.C.S.

THE specimen is one half of the whole tumour, and shows upon its surface a portion of the suprarenal gland from which it has originated. Part of this remnant is clearly defined, and shows a well-marked border and a characteristic colour. Towards the lower extremity it broadens out and becomes attenuated, to be eventually lost in the general surface of the specimen.

The tumour as a whole is lobulated, and was easily dissected from the fat which surrounded it. There is no evidence of infiltration of the surrounding tissues. The surface is traversed by numerous large blood-vessels.

On the cut surface the outer portions of the tumour appear as a pale solid tissue, with dilated blood-vessels or small hæmorrhages here and there. More deeply the growth has broken down to form irregular shaggy spaces surrounded by degenerate tissue of an almost gelatinous appearance. In other places there are smaller degenerate areas of a more opaque appearance and brighter yellow colour.

Microscopic Examination.—Sections stained with hæmalum and eosin, and hæmalum and Van Gieson's picro-fuchsin. Two pieces were taken for section: (1) from the surface at right angles to the suprarenal remnant, and (2) from the cut surface in the depth of the tumour.

(1) The first shows the suprarenal remnant to be of normal structure. The normal gradually blends into a freer growing tissue, which resembles that seen in hypernephromata or suprarenal adenomata. Gradually the cells assume a more alveolar or grouped arrangement, sometimes with a blood-vessel in their midst. They stain more deeply, and in the more actively-growing areas are less vacuolated. Considerable areas of necrosis are seen.

(2) In the second section there is no normal tissue. The growth appears to have a definitely perivascular arrangement, at first sight suggestive of endothelioma or epithelioma. The cells, however, are of the same type as those in the first section derived from suprarenal cortex, and so are epithelial and not endothelial in origin. Here and there are large areas (remote from the blood-vessels) which have degenerated.

A Perirenal Lipoma, with Myxo-sarcomatous Changes in One Portion.

Shown by W. McADAM ECCLES, M.S.

FROM a woman, aged 57, who had noticed three months before operation that the right side of her abdomen was beginning to increase in size. The enlargement had been more rapid in the last fortnight. She had *never had any increased frequency of micturition*, and there had *never been any blood in her urine*.

It was stated that she had lost several pounds in weight since the appearance of the tumour. The swelling occupied the whole of the right loin, and was smooth on the surface and elastic. It could be moved freely from side to side, but not much from above downwards.

A long incision in the right linea semilunaris exposed a *retroperitoneal tumour* apparently wholly fatty. After incision of the parietal peritoneum and the pushing over of the ascending colon, the whole mass was fairly easily enucleated. The kidney could not be seen at first, but when its lower pole was exposed it was decided that it would be impossible to leave it behind, consequently it was removed with the main mass. *The patient made a good recovery. The tumour weighed 6½ lb.* It will be seen that the bulk of the mass is *clearly lipomatous in nature*, but a part at the lower end is different, and resembles *myxomatous tissue*, with considerable hæmorrhagic areas.

On *microscopic section* the upper part is seen to be a typical lipoma, but the lower portion has the characteristics of a myxo-sarcoma (see separate note below).

There is no evidence of any disease of the kidney, and there is no interference with the functions of the ureter.

MICROSCOPIC REPORT BY DR. T. H. G. SHORE.

One section is a simple lipoma. The other section is composed of cells with oval or elongated nuclei, somewhat variable in size, and scattered through a hyaline or slightly reticulated matrix. The cells are separated widely from each other by the matrix. Most of the cells have delicate processes which interlace through the matrix. The blood-vessels are small and have thin walls composed of the cells of the tumour. This part of the tumour is free from fat, and is probably a myxo-sarcoma.

Specimens of Renal Tuberculosis.

Shown by H. P. WINSBURY WHITE, F.R.C.S.

(1) THE left kidney and the bladder from a case of unilateral renal tuberculosis. The pelvis is blocked with calcareous débris. The lumen of the ureter has almost completely disappeared. The left ureteric orifice has been obliterated, its site being indicated by a slight dimple. There were no urinary symptoms.

(2) The left kidney from a case of unilateral tuberculosis. The pelvis is blocked with caseous débris. There were no urinary symptoms.

Sarcoma of the Prostate.

Shown by A. CLIFFORD MORSON, O.B.E., F.R.C.S.

History.—Child, male, aged 11 months, admitted to hospital with acute retention of urine. Mother stated that for some days previous to admission the urine had dribbled away at frequent intervals and then finally ceased to be passed.

On Examination.—Well nourished and, for his age, normally developed. Marked mental irritability. Palpation of bladder revealed distension to 1 in. above umbilicus. Soft, elastic and pear-shaped swelling could be felt through wall of rectum in region of prostate and base of bladder. Tumour mobile. Olive-headed catheter 5, French size, passed into the bladder without much difficulty. Urine withdrawn contained no abnormal constituents.

The diagnosis of sarcoma having been made, I gave instructions for the child to be catheterized twice daily while arrangements were being made to obtain some radium which I proposed to insert into the tumour through the perineum. Unfortunately death took place before this agent had been procured.

Post-mortem Examination.—Both kidneys in an advanced state of pyelo-nephritis. A soft, friable tumour the size of a William pear occupied the region of the prostate and base of bladder and surrounded the prostatic urethra. There was no invasion of the rectum or bladder mucous membrane.

Histological Examination.—Round-celled sarcoma.

The literature on the subject of sarcoma of the prostate is meagre. Sixty cases only have been recorded and of this number the diagnosis of half has not been verified by histological examination. The majority of cases occur under ten years of age and are very malignant. The tumour appears to reach a great size before any symptoms are present. The symptoms are those associated with obstruction to urination.

Operative treatment has been entirely unsuccessful. The tumour may be shelled out either by the suprapubic or the perineal route, but recurrence is the invariable rule. Since the introduction of radium in the treatment of malignant disease it would appear that the prognosis in prostatic sarcoma should be more favourable, as the round-celled variety is the most sensitive of all tumours to these rays. In no case in which the clinical diagnosis of sarcoma has been confirmed by microscopic examination has there yet been recorded even an apparent cure by irradiation. However, there is one observer who claims to be the first surgeon to have cured a case of sarcoma of the prostate with the aid of radium, but in support of this contention he can bring forward only clinical evidence. Such a record has little scientific value.

Secondary Hypernephroma removed from Thigh (Intermuscular).

Shown by JOHN EVERIDGE, O.B.E., F.R.C.S.

PATIENT, a female, aged 56.

History.—For three months had noticed an increasing painless swelling at the back of the left thigh. No other symptoms. No urinary symptoms.

Result of Examination.—Circumscribed, mobile, discrete swelling, the size of a Jaffa orange, lying above left popliteal space. Right kidney slightly enlarged.

Treatment.—The tumour was easily shelled out. It lay in contact with, but not adherent to, the sciatic nerve.

Pathology, Naked Eye.—Typical appearance of a hypernephroma.

Microscope.—Cells, in active mitosis, arranged in columns. Some cells large and vacuolated. Structure resembled hypernephroma.

After-history.—Developed multiple tumours in liver and omentum, which were probably secondary deposits from a hypernephroma of the right kidney.

A Kidney, the Seat of Multiple Lesions.

Shown by JOHN EVERIDGE, O.B.E., F.R.C.S.

PATIENT, a male, aged 48.

History.—For two years vague pains in left kidney region. X-rays showed shadows suggesting calculi in the left kidney.

Symptoms.—None, excepting pain referred to. Never hæmaturia.

Result of Examination.—Urine contained albumin, casts (hyaline and granular), and a few pus cells.

Treatment.—Left nephrectomy. The kidney showed (1) an acute kink at the pyelo-ureteral junction, which had produced a hydronephrosis; (2) a large papillary epithelioma of the renal pelvis; (3) calculi.

Pathology.—The interest lies in the comparative absence of symptoms arising from a kidney the seat of so many lesions.

As the calculi were evidently firmly lodged in calyces it was a matter for debate as to whether the stones should be removed at all. Operation was indicated on account of the pus cells and casts found in the urine.

Hæmorrhage from a Retropelvic Vessel during Pyelolithotomy.

Shown by JOHN EVERIDGE, O.B.E., F.R.C.S., for
Sir JOHN THOMSON-WALKER, F.R.C.S.

A CASE illustrating the anatomical difficulties in delivering a kidney into the wound. Pyelolithotomy was being performed with the kidney *in situ*, when the incision into the pelvis opened this vessel. Efforts to check the bleeding were unavailing, so nephrectomy was carried out.

Case of Carcinoma of the Urethra.

By J. SWIFT JOLY, F.R.C.S.

F. M., AGED 60, admitted to St. Peter's Hospital, February, 1923, with overflow incontinence. Very tight stricture at peno-scrotal angle, bladder up to umbilicus. Internal urethrotomy and cystotomy, as urine was very foul. October, 1923, retention. Stricture closed down to 8 French; peri-urethral abscess. Incision of abscess, and internal urethrotomy. February, 1924: Stricture closed down again. Permanent suprapubic cystotomy, abscess still discharging, drained. July, 1924: Returned because "abscess" was so painful. Large epitheliomatous ulcer, with hard, everted edges involving lower surface of penis and front of scrotum. Induration of the whole of the corpus spongiosum. Enlarged glands in both groins. Amputation of penis and scrotum as a palliative measure to relieve pain, July 16, 1924. Patient still alive.

Pathological Report.—"Typical squamous-celled carcinoma, with small amount of keratinization. The growth can be seen to invade the underlying areolar tissue."

A New Cysto-Urethroscope.

Shown by J. SWIFT JOLY, F.R.C.S.

THIS instrument is a slight modification of the author's operating cystoscope introduced in 1922.

The sheath of the new instrument is exactly similar to that of the old in all respects, but it carries an additional window cut in its convex side. The centre of

this window is 25 mm. from the *coude* bend, and is only used for urethroscopy. This additional window necessitates a modification of the obturator, which now carries two blocks, one of which fits each window. The urethroscope proper consists of a telescope giving a slightly forward field of vision and focused for near objects. A lamp is attached to its distal end, while immediately behind the window is a short Albarran lever, with the usual control wires, soldered to the telescope itself. In the handle are: two water conduits for continuous irrigation, the catch of the interrupted screw lock, a milled head for moving the Albarran lever, and the usual type of cystoscopic attachment for the battery flex.

No change has been made in the single or double catheterizing, or in the retrograde attachments of the old cystoscope, all of which can be used with the new sheath.

Urethroscopy is performed as follows: When the bladder has been examined, the light is switched off, the flex removed from the sheath, and the telescope and catheterizing attachment removed. This allows the bladder to empty. The urethroscope is then inserted into the sheath, and locked in place. This cannot be done until the window of the telescope is opposite the opening in the convex side of the sheath. The water conduits are then connected up to an irrigator, and when the stream is coming freely from the outlet the flex is attached to the *urethroscope*, and the urethra examined as the whole instrument is slowly withdrawn. A flexible instrument up to 8 French in size can be passed through this urethroscope so that it can be used for fulguration, &c., in the posterior urethra.

Tuberculous Kidney.

Shown by S. G. MACDONALD, F.R.C.S.

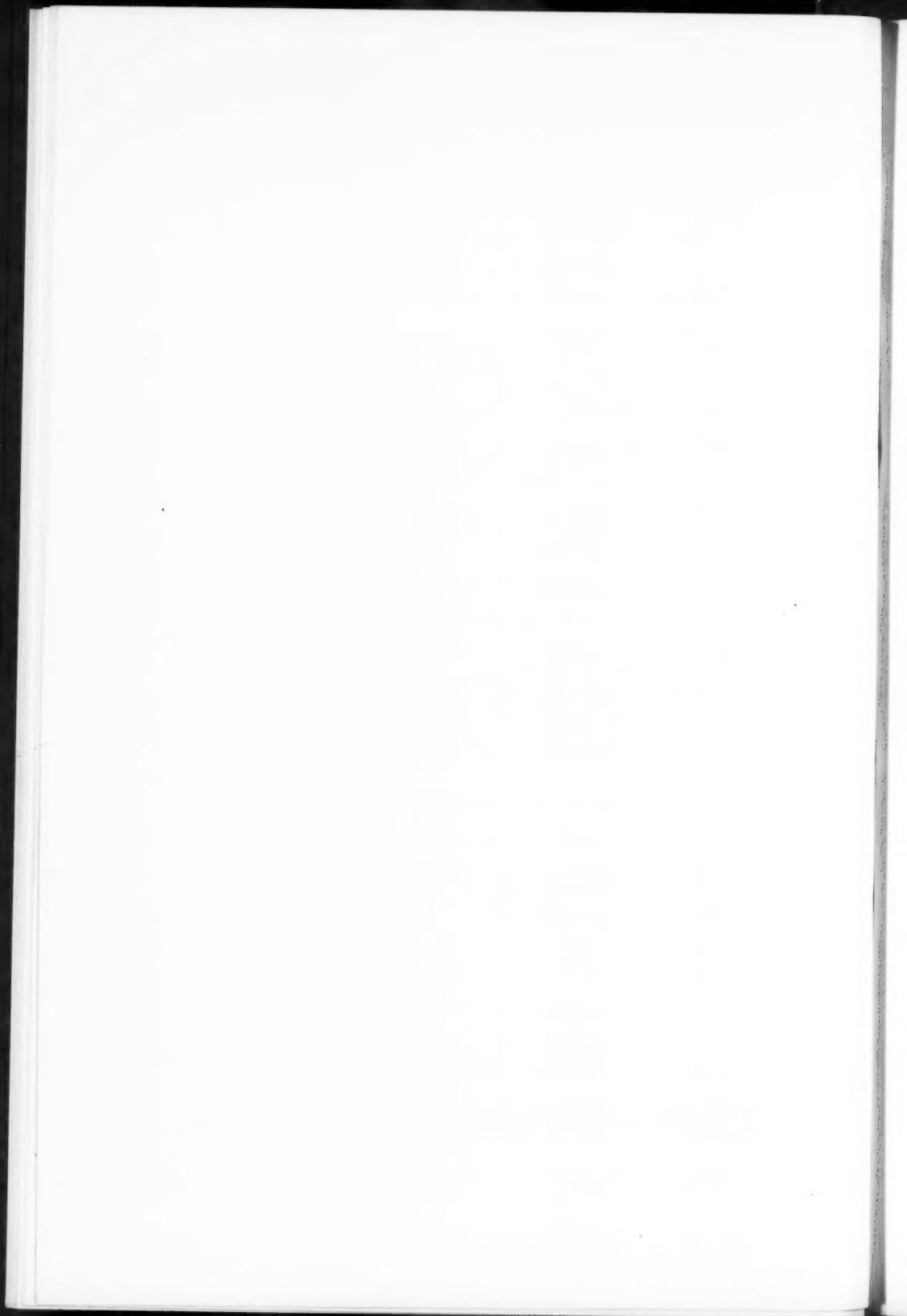
AT the last meeting of the Section, see p. 7, Sir John Thomson-Walker raised two points that are illustrated by this specimen: (1) The exclusion of tuberculous foci resulting in closed renal tuberculosis; (2) the failure to find tubercle bacilli in cases which clinically and cystoscopically are tuberculous.

This is a left tuberculous kidney removed from a patient in whose urine tubercle bacilli could not be found (until the day before operation) after repeated examinations extending over six months.

Congenital Hydronephrosis in Rats.

Shown by G. E. NELIGAN, M.B.

THESE two rats, which had been used for feeding experiments, on being killed were found to have a right hydronephrosis, with dilatation of the whole length of the ureter. The left kidney is hypertrophied.



Section of Urology.

President—Mr. CYRIL A. R. NITCH, M.S.

DISCUSSION ON RADIO-THERAPY AND X-RAY THERAPY IN DISEASES OF THE BLADDER AND PROSTATE.

Dr. N. S. FINZI.

A FEW WORDS first of all as to the methods of treatment used.

RADIUM.

On the Skin.—Radium in diseases of the prostate and bladder has been used outside the skin, over the pubes, the sacrum and even the perineum. It is ineffective unless very large quantities are employed and a fairly large series of ports of entry used. The skin must then be given as large a dose as it can stand short of blistering. The radium must be separated at least 2 cm. from the skin by wood, felt or wax. About 500 millicuries should be used to be really effective, and even that would need a pretty long application. Smaller quantities could be used, but the time would have to be increased more than one would expect from multiplying the millicuries by the days.

Per Rectum.—Radium has also been applied by the rectum, and although this has been given up as causing burns without being effective, I succeeded in 1909 in getting a successful result in a tumour in the prostate region by this method. The tumour, however, must have been exceptionally susceptible, and was probably a recurrence after teratoma of the testicle. This patient is, I believe, still alive; he was alive two years ago. He got some stricture of the rectum.

Per Urethram.—To give an effective dose by the urethra must cause so much sloughing as to render the method too dangerous. It was attempted with ineffective doses but has been given up. Even as an adjunct to other methods the urethra will receive too large a dose and the outlying parts of the prostate too little.

Embedded in the Prostate by Operative Measures.—This method is bound to supersede the others, except the use of large quantities on the skin, and may supplant even that. I originally suggested the perineal route in 1910, but was unable to get it carried out. For the last few years it has been extensively used in Brussels, the radium in platinum needles being introduced by an incision through the perineum. The improvement introduced in recent years has consisted in the use of a number of needles instead of one or two large tubes, with the result that the radiation is much more homogeneously distributed.

X-RAYS.

(1) The older method was to use a filtration of about 5 mm. of aluminium and give repeated doses through three or four ports of entry, and repeat the treatment every three weeks for long periods.

(2) The recent method is to use greater filtration: 0.5 - 0.8 of copper with 3 mm. aluminium in addition, and to select a larger number of ports of entry. A very penetrating radiation is employed, and the dose received by the tumour is calculated as accurately as possible. In other cases where perhaps this method is not feasible, and therefore only four ports of entry can be obtained—front, back and sides—the increased penetration is obtained by placing the X-ray tube at a greater distance and using large fields. This method is also employed if the disease has spread widely into the pelvis.

Combined Methods.—These will be found of value. For instance, radium and X-rays, diathermy and radium, etc.

SIMPLE ENLARGEMENT OF THE PROSTATE.

Radium has not been largely used for this condition. Its external application would seem to possess little advantage over X-rays, and as very large quantities would be required it would be very expensive. The internal application by embedding would not offer many advantages over other operative procedures. Applied by the urethra, and by the rectum also, it might cause ulceration, and would probably be ineffective.

X-rays, on the other hand, appear to be a very suitable method of treatment for this condition, and have certain definite advantages over operation.

Advantages.—(1) There is practically no primary mortality if the rays are carefully administered; probably less than 0.1 per cent.

(2) Usually there is no prolonged convalescence, and the patient need not lie up.

(3) No suprapubic opening is required, provided a catheter be tied in for twenty-four hours after the treatment when the case is on the verge of complete retention.

(4) There is no danger from hemorrhage, as after an operation.

(5) The only skin damage that may occur when under treatment by the most recent methods is a slight erythema.

Disadvantages.—(1) It is possible that the cure may not be a complete one and the treatment may have to be repeated. In my cases one treatment has been sufficient.

(2) If a subsequent operation were required it is conceivable that the previous X-ray treatment might make it more difficult. I cannot speak definitely, as my cases have so far not required operation.

(3) It takes three to eight weeks before the effects are manifest, and the FULL improvement is not obtained until after more than three months.

(4) In one of my cases a burn of the rectum occurred, an ulceration not appearing until three months after the treatment. I regard this as due to faulty technique. The rectum has to receive a fairly heavy dose, but one need not give enough to risk ulceration in a simple prostate; in a malignant case one must risk this.

(5) The possibility of treating a malignant case as a simple one must be held in mind. Every case, therefore, should be submitted to a urologist before X-ray treatment. One of my cases developed malignant disease of the abdomen subsequently, but I was not allowed to send this case to a urologist before treating him. At any rate most urologists would not regard themselves justified in extirpating the prostate in persons over the age of, say, 60, in case a proportion of the prostates might prove to be malignant.

Contra-indications.—(1) Considerable sepsis of the bladder; (2) pelvic inflammations.

MALIGNANT DISEASE OF PROSTATE.

Radium.—Radium needles embedded in the prostate are being used abroad, often in conjunction with X-rays from outside. The perineal route is adopted, and the most rigid asepsis must be employed. I have not used this method yet. Applications *per rectum et urethram* in the early days did not give much improvement.

X-rays.—The old methods were not of great value, but the new methods hold out much promise. I have seen improvement, resulting in freedom from symptoms for over a year, after a treatment. Whether these cases are ultimately cured I do not yet know. I believe some will be cured. I give the treatment in one dose lasting over two days, and repeat it in three months' time if thought advisable.

PAPILLOMA OF THE BLADDER.

Where there are a few small growths, diathermy has supplanted radium. When the whole bladder-wall is involved I have seen radium prove of great value. The difficulty is to get a homogeneous irradiation. I have no knowledge of X-rays being used for these cases.

MALIGNANT DISEASE OF THE BLADDER.

Radium has been passed through the urethra, being placed in position by a cystoscope, sometimes being supplemented by doses from the rectum or vagina as the case may be. Great relief has been obtained in some of these cases, but I have not yet seen a case which remained well. Often a subsequent application has much less effect. It is not so successful as in carcinoma of the prostate. Of X-rays in this condition I have very little knowledge. By methods that do not quite reach the present standard I have seen definite improvement.

To sum up then:—

(1) I consider that a case has been made out for an extensive trial of X-rays in treatment of hypertrophy of the prostate in selected instances.

(2) Cases of malignant disease of the prostate often do very well, and it needs a careful trial being made to determine whether the results are as good as operation. In inoperable cases, X-rays, radium or both should always be tried.

(3) Cases of malignant disease of the bladder do not do so well. If operable they should be treated by operation. If not, it is worth while using radiations even as a palliative.

(4) Papilloma of the bladder is best treated by other methods, unless too large a surface of the bladder-wall is involved.

Dr. DOUGLAS WEBSTER

said he would confine his remarks to malignant disease. It was hardly necessary to remind Members of this Section that the large proportion of failures or merely palliative results attained by *any* methods of treating bladder and prostate malignancy was due to the extent to which the disease had progressed before suitable treatment was begun. Thus one clinic (Barringer, Memorial Hospital, New York) had found that in only two per cent. of prostate cases referred for treatment was the disease confined to the prostate: in 98 per cent. the vesicles, bladder, pelvic walls or bones were already invaded. In another series of cases (Bumpus, Mayo Clinic) 28 per cent. showed at their first appearance X-ray evidence of bony metastases. As a rule, at the best, by the time the radiologist saw the case, the condition was similar in extent to a large uterine cervix growth with invasion of both parametria. The biological and technical difficulties in attempting to deal satisfactorily with such large areas of disease were very great. Dr. Finzi had outlined the different methods which were available: radium or X-rays alone, or the two combined, with or without surgical measures or diathermy.

Surface radium was not suitable, except in quantities hardly obtainable anywhere, and then only as supplemental to other treatment, owing to the large area involved and the depth from the surface to be treated.

The method of embedding multiple small radium tubes had been much used in France and Belgium, as Dr. Finzi had said, and it also was being much used in America. As an example the work of Scholl and Braasch in six cases might be referred to: they buried radium in quantities of about 900 mgn. hours and after two weeks performed prostatectomy. Portions of the tumour were obtained for section when the radium was buried, and compared with the condition found after two weeks: by that time in several cases no traces of the malignant cells were found to be left; in others they were greatly enlarged and degenerated, in others a few small assemblages of malignant cells were found apparently still viable.

A combined method had been used by Thomas and Pfahler in twenty-six cases. First, Pfahler gave a preliminary X-radiation of the whole area from the umbilicus to the symphysis, aiming to sterilize the growth, and the lymphatic drainage areas. Two weeks after this, X-ray radium tubules were buried in the prostate, either by a suprapubic or perineal route, 10 mgn. needles placed one to two centimetres apart, for eight to eighteen hours. A fortnight later the X-ray course was repeated, and renewed again in three or four weeks. In bladder cases diathermy was used to remove large tumour masses, and radium needles spaced out about a centimetre apart were buried into the base of the tumour. Such combined methods would appear to offer promise in dealing both with the original site of growth and its possible or probable lines of invasion.

As to X-rays alone, there were a number of technical methods employed which could not be dealt with here. Accurate technique was extraordinarily difficult, especially in treating large areas in stout patients. If one considered that one method used in treating cervical cancer—the so-called Röntgen-Wertheim method, under which almost 100 per cent. of apparent cures had been obtained—meant six areas (three front and three posterior) concentrated on the cervix, followed by six areas on the right parametrium and six more on the left parametrium, it seemed that the X-ray methods commonly employed in prostate cases usually allowed far too small a dose over the whole diseased area. It appeared to him (the speaker) that at least his earliest cases had been underdosed, partly from fear of possible damage to the rectum and bladder, and as it was probable that the usual histological types of malignant growth of the prostate rendered the prostate a less radio-sensitive tissue than the cervix to cervical cancer, this question of dosage was very important. In thin patients it might be possible to administer sufficient radiation by X-rays alone, but it probably would be found better to use X-rays as a supplement to radium, locally in stout patients at least. He (Dr. Webster) had had eight cases of prostatic and six of bladder malignant disease. In one prostatic case a recurrence in the obturator region, causing swelling in the thigh and leg, was relieved of this troublesome condition: the prostate had been treated with radium by Mr. Sumpson Handley. In another of Mr. Handley's cases in which an infiltration was felt along the lower end of one ureter there was improvement locally and in general symptoms. A patient sent by Sir John Thomson-Walker improved for a time in symptoms and general condition, and the size of the prostate was considerably reduced; but after some months secondary deposits were found in the liver. This patient showed a remote effect of the prostatic radiation which might be referred to. He was an Army doctor who had spent

much of his life in India, and as a result had a very irritable skin on the face: once a papule appeared which was excised, and was found to be squamous-celled carcinoma; later a similar papule was eradicated by CO₂ snow. Shortly before coming under observation for the prostatic trouble a similar papule had begun to enlarge. Arrangements were being made for this to be seen and treated, but the fourth day after the prostatic radiation it shrivelled up and disappeared: this suggested that biochemical changes (of which we were still largely ignorant) took place in radiation treatment, producing general or distant effects.

Several of his (Dr. Webster's) vesical cases were still under observation and treatment, showing slight improvement or no change over a period of many months.

If all the difficulties in the treatment of malignant disease in this region were taken into account, the late stage, at which patients as a rule, came for treatment, the type of growth, and the early invasion in prostatic cases, it would be agreed that the few apparent cures reported, and the frequent marked improvement in signs and symptoms, were promising. The results suggested that further improvements in technique, and specially perhaps combined radium, X-ray and diathermy methods, would soon lead to something better and more permanently satisfying.

Dr. REGINALD MORTON.

It is safe to assume that an audience such as this does not wish to be troubled with technical details, but rather to hear how far the radiologist can be of assistance in dealing with the more difficult cases that confront them. There are, however, a few points that might be touched upon with advantage, if only for the reason that I have been asked technical questions at times, and have even heard urologists express quite decided opinions on such matters. The various methods of treating cases with the X-rays may be classed under three main heads. First, we have the classical method of giving small fractional doses spread over a more or less prolonged period. At the other extreme we have the single full-dose method, as exemplified by the Erlangen school; and lastly we have various methods combining these two in varying degrees—that of Dessauer being as good an example as any.

For the first twenty years that I was engaged in this work the first method was the only one in use, though towards the end of that period we were reaching out, so to speak, and giving gradually increasing doses. Naturally, during that period I treated a large number of cases, and I venture to say that there are few, if any, modifications in technical detail that I have not tried at one time or another. In the last four years I have treated almost as many, more or less on the lines laid down by Seitz and Wintz at the Erlangen Clinic. I say "more or less" advisedly, for the simple reason that here we seldom get cases to treat at the early stage they are habitually treated there. Treatment on what may be called strictly Erlangen principles is not applicable to such cases as are handed over to the radiologist here, since in most instances the curable stage has been passed, the disease has spread so far as to preclude any possibility of success from surgical methods, and consequently our X-ray technique has to be modified accordingly. As a matter of fact the radiologist who really understands his subject does not confine himself to any one technique. Every case must be dealt with according to the conditions present, but, all the same, I regard it as of the first importance, as a general principle, to get in the full dose at the earliest possible moment. Any modifications or combinations of technical methods are permissible so long as this end is secured. Dissemination is the gravest complication that can arise in any case of malignant disease; local extension is bad enough, and may in itself be sufficient to prove fatal, but so long as the lesion *is* local there is still hope; there is no hope once dissemination has set in. No one knows the moment when dissemination begins, and once a case is brought to us I consider it our first duty to administer the full carcinoma dose to the whole of the involved area in the shortest possible time, and so sterilize as far as we may all cells that might escape from the parent growth. While we are "fiddling" with fractional doses dissemination may begin and render all efforts futile. I do not use the word "fiddling" contemptuously, but in view of the seriousness of the problem we are up against, fractional doses do seem to me a "fiddling" procedure. It has its uses in some advanced cases where nothing but amelioration can be looked for, but where the patient's signs and symptoms are such as to give him only a sporting chance we must strike out at once and hit hard.

In non-malignant cases fractional doses are permissible, but why make several bites at a cherry? Take the familiar instance of excessive bleeding at the climateric. Why spend

weeks, perhaps, in giving fractional doses, when in at least 90 per cent. of such cases a successful result can be secured at one sitting lasting not more than a couple of hours, without pain or discomfort to the patient and without further disturbing her ordinary life? Where results as good as this are not attained in this particular condition, the cause almost invariably is due to faulty technique.

To apply these principles to such cases as come within the scope of this Section, let us consider some of the affections of the prostate gland, beginning with the simple enlargement of the adenomatous type. Here we have a class of case that responds very satisfactorily to X-ray treatment. Accurate technique is essential, but, given this, a good result can be obtained in nearly every case. I wish it to be understood that I am not speaking of fibrous enlargements due to any of the various types of infection.

Even before it was my privilege to introduce the Erlangen methods to the profession in this country, just over four years ago, I had treated several cases of simple enlargement by means of fractional doses of X-rays, and, while a more or less satisfactory result was the rule, it took a long time to achieve; patients were kept up to their appointments with difficulty, and the frequently repeated applications tended to bring about undesirable changes in the skin and deeper tissues. A single, full dose, accurately applied, ensures a satisfactory result in a very large proportion of cases after an interval of about six weeks, and the changes in the skin and pelvic tissues are negligible for all practical purposes. I have no hesitation in saying that if this method of dealing with simple enlargement of the prostate is given a full trial under fair and proper conditions, it will very soon become a recognized and regular procedure. It is not necessary to administer so large a dose as to cause any pronounced reaction, and as a rule the patient is unconscious of anything taking place until about a month after the application, when he realizes that his more troublesome symptoms are abating. This is the ideal result. If the X-ray dose is equal to that given for carcinoma there is very likely to be some local reaction—usually during the second week—and this may give rise to a temporary aggravation of symptoms, such as increased frequency of micturition and discomfort and possibly a more frequent use of the catheter. This is never serious, and the ultimate result is even more satisfactory. As might be supposed, one only gets those cases to treat in which, for one reason or another, surgical methods are ruled out, but, so far as my experience goes, I am sure the method will be employed with increasing frequency.

Among the valuable contributions to radiology by the Erlangen school is a more or less accurate system of dosage and all that this entails. I use the phrase "more or less" advisedly since even Prof. Wintz himself stated that though the greatest care was taken there was always a possibility of error up to about 15 per cent. As everyone knows there has always been uncertainty as to how any particular case was going to respond to X-ray treatment, and we used to offer, as a partial explanation at least, that this was because we had no accurate idea of what dose we were giving, especially if the lesion was at any appreciable depth. I hoped that under the new conditions most, if not all, of these troubles would disappear, but this has not been the case. As regards early localized lesions of malignant disease, our results have been improved very distinctly; all the same we are just as unable now to predict with any certainty how any particular case is going to respond. To state the matter another way, the factors making for success are only a little more under our control than they were, and they lie within the growth itself; they are biological rather than physical; and it seems to me that the only way of getting more uniform reaction to our irradiations lies in finding some means of rendering malignant lesions more sensitive to the rays. Of one thing I am certain, and that is that we must do more in the way of preparing patients for irradiation and looking after them subsequently. I am convinced that treating them as out-patients is not good enough.

In the course of some comments I made here last year on the subject of the X-rays and malignant disease, I took occasion to say that, so far, the newer methods had not materially increased the number of successful results. This seemed to give a peculiar satisfaction to a few individuals, and at least one expressed his pleasure in a letter to a medical paper. I wonder if anyone seriously imagined that any modification of X-ray technique could possibly give a higher percentage of successful results so long as the radiologist is denied access to cases in a curable stage. I am stating less than the truth when I say that 95 per cent. of all the malignant cases that passed through my hands up to that time were so advanced through local extension or distant dissemination that anything more than a temporary amelioration was unreasonable to expect; and under such conditions further progress is not possible. I am far from despising anything that will give the unfortunate patient a chance of his life, or that

will make the last days more bearable; but this is not sufficient, and if we are to make progress the radiologist must be allowed to intervene at an earlier stage; the earlier the better, as time will show here as it has shown elsewhere. I am glad to say that during the last few months there are signs of a tendency for cases to come to us sooner, and if this continues I confidently predict a marked improvement in the results of our dealings with malignant disease. I trust that it is understood that I am pleading only for a closer and earlier co-operation with the surgeon: nothing more than that.

Let me quote an instance of what I complain.

In June last a case of cancer of the tongue was operated on by diathermy, a considerable portion being removed. Instead of being transferred to the X-ray department on or before discharge from the ward the patient was kept attending as an out-patient until about the middle of December. When he came to the X-ray department his condition was most pitiable. He had a foul ulcer of the floor of the mouth, bilaterally enlarged submaxillary glands, and a large secondary mass behind the angle of the jaw on the left side. Speech and swallowing were almost impossible, and he was severely wasted and cachectic. I need not dwell on the further horrors of the case, but I ask you, how are we to make any progress when this sort of thing goes on?

I have had a fair number of cases of malignant prostate, and the feature common to all of them was that they were inoperable for one reason or another, or had been operated on and had recurred. As might be expected I have no cures to report, but in a large proportion there was great improvement in both local and general conditions. Pain relieved, shrinkage of the primary growth, less frequency, and diminished need for the catheter—even dispensed with altogether. In a few cases the enlarged gland had shrunk to practically normal size, and the patients were so well as to consider themselves cured. Unfortunately extension gradually asserted itself. As a class, I regard malignant disease of the prostate to be favourable for irradiation, just as it is in the uterus and the rectum. The pelvis as a whole is more favourable than any of the other main divisions of the body. The first case of malignant prostate I treated by the Erlangen method is instructive. There was the usual hard enlargement with irregular surface, and for some weeks previously catheterization took place twice daily. The growth had been declared inoperable by a well-known specialist. The general health was beginning to fail, but the patient was going about fairly well though losing weight slightly. Treatment was given on June 6, 1921, and by the first week in August the catheter was no longer needed; he was gaining weight and enjoying life. This happy state of affairs continued for just on twelve months—no further X-ray treatment being given—when there were signs of extension. He slowly declined and died about sixteen months after I first saw him. This I consider a very remarkable result after a single irradiation, and subsequent experience has taught me that such a case ought to be followed up with further treatment applied more widely to the pelvis. Unfortunately he lived in Devonshire, and I fear both his doctor and himself assumed too much from the highly satisfactory result of the one application.

While improvement for a time is the rule, I have had a small number of cases which, for some reason, seemed to be quite unaffected. I have no explanation to offer, but I am sure it was not due to faulty technique.

Another case is worth mentioning. About two years before coming to me the patient was seen by Mr. Sydney Macdonald, who found an inoperable malignant prostate, and provided a suprapubic drain by which all urine has since passed.

In the early part of last year there had been increasing trouble with the bowels, and the patient was very anxious for something to be done. I gave a full X-ray dose on July 30, and on September 23 his doctor wrote that he was greatly improved in health, that he was gaining weight, and that local symptoms had subsided. It was considered advisable to follow this up, and treatment was repeated on October 1. In December his doctor again wrote to say that the patient was very well indeed and was gaining weight rapidly, and that the enlargement of the prostate could no longer be felt. One could scarcely expect a more satisfactory result than this. The pity is that the rays were not used shortly after the operation, and though it is just possible that this case may yet turn out something in the nature of a clinical cure, we must remember that a period of two years has elapsed since the real nature of the case was recognized.

I have not had nearly such favourable results in malignant disease of the bladder. Nearly all my cases have been very far advanced, and, what is also very important, a large proportion of them have been infected. Infection of any kind is a formidable obstacle to

success in the treatment of malignant disease. It is for this reason that our efforts are so unsuccessful in the region of the mouth and air-passages. In an experience of over twenty years I cannot recall a single successful result in a case in which the lesion was also the site of septic infection. A temporarily favourable effect may be secured at first, but the final result is always the same. A malignant lesion is a serious proposition under the most favourable circumstances—complicated with sepsis it is practically hopeless. It is in the experience of every radiologist to have occasionally achieved a brilliant, even sensational result; in virtually every such instance it will be observed that there was no septic or other infection. Success depends on a healthy and vigorous reaction on the part of the surrounding tissues, and this we cannot have if they are infiltrated with septic organisms and their products. Here again is an instance in which I feel we must take more care of our cases, both before and after irradiation, and not subject them to the usual unwholesome conditions that are the lot of the average out-patient. We must have proper cancer wards attached to the X-ray department and see that the patients are properly looked after in every detail, locally and generally. I am sure that it is only on these lines that we are going to be in a position to take full advantage of the resources at our disposal.

DR. R. H. BROWNE-CARTHEW

referred to the treatment of diseases of the prostate and bladder with radium applied *per rectum*. The quantity of radium he had used was 50 mgm. of the bromide of radium, having an activity of 24,000 to 25,000, as measured and estimated in the Institut de Radium, Paris. He had used it in a tube which had been specially made for the treatment of a case of recurrent malignant disease of the rectum, sent to him by Sir Arbuthnot Lane. It had occurred to him that with this tube radium could easily be applied to the prostate and base of the bladder. He had had at one time under treatment and observation twelve cases or more of simple enlargement of the prostate. The patients had found that nocturnal frequency was diminished and that they passed a better stream. In some of these cases, on palpation *per rectum*, the prostate seemed to have been softened by the treatment. But such palpation was indefinite.

Something more definite came to hand in a case of prostatic hæmorrhage of long standing. After the first application the patient wrote the same day to express his "surprise that when he passed water the first time after the application there was no blood in his water." His urine had, in fact, from being of the thick coffee-grounds consistency, become merely smoky. The result of this and of a subsequent application was so encouraging that one would have expected the patient to come for further treatment, but this he did not do. He died of prostatic hæmorrhage some two years afterwards.

About the same time he (Dr. Browne-Carthew) had under treatment a case of papillomata of the bladder. The cystoscope showed large numbers of papillomata over the whole base of the bladder. The patient's condition was pitiable. He was passing most foul and offensive-smelling urine, with blood and pus, at frequent intervals and with great pain. After nine or ten applications of radium *per rectum* he wrote: "I am pleased to say that I feel quite cured, as I have no trouble to pass my water. It comes quite natural and easy, and no sign of blood or discharge of any kind, so that I feel that the treatment has cured me entirely and I sincerely hope I shall not have a recurrence, but should I do so I shall most certainly come to see you again for treatment." This letter had been written four years ago. Subsequent cystoscopic examination showed that though there were still papillomata they had shrunk and become paler, and that there was no bleeding from them.

Another case was one diagnosed as malignant disease of the prostate. A suprapubic opening had been made and the bladder had been relieved by a syphoning apparatus. After a few applications the patient stated that he was now occasionally passing water through the urethra, which he had not done for a long time. He had become so used to his syphoning arrangement that this apparent tendency to return to the normal appeared to alarm him. Whether it was on that account, or that for some other reason he had been dissuaded from doing so, he did not continue the treatment. Patients were often dissuaded by their friends, or by their doctor, from following up a treatment of which little was known.

He (Dr. Browne-Carthew) had found that treatment with radium *per rectum* was useful in cases of chronic prostatitis of venereal origin. Also, in one case of simple enlarged prostate in which the patient suffered from hæmorrhoids, these were quickly relieved and eventually disappeared during the treatment.

After an experience of over twenty years in the application of radium, he had arrived, rightly or wrongly, at the conclusion that the beneficial use of radium was due to its power of causing contraction of the blood-vessels, thus diminishing and modifying the blood-supply. He thought this action should always be taken into account in using it. To employ it as a directly destructive agent he thought was more likely to do harm than good. In all cases it was necessary to proceed with great caution in order to avoid devitalization of normal tissue.

Mr. A. CLIFFORD MORSON.

It will be generally agreed that suprapubic prostatectomy in the hands of an expert is attended with excellent results and can be regarded as a radical cure as much as the operation for adenoma of the breast. Can the radiologist produce by his means the same result as the surgeon does? If he can, what is his evidence?

An expert authority should cystoscope the patient to ascertain the condition of the bladder before treatment. A renal function test should be carried out both before and after treatment, and it should be determined how much residual urine is present in the bladder before treatment, and again, after that has been carried out.

Sometimes the radiologist said the symptoms to a great extent disappeared after irradiation, but rectal examination showed that the prostate was still enlarged. Symptoms associated with an enlarged prostate are not entirely confined to the urinary organs. Cases in which the patients have been treated by X-rays and have come to me afterwards for surgical measures have been complicated (1) by retention of urine, and (2) by cystitis.

Mottram has shown that after the application of X-rays the mucous membrane has revealed definite degenerative changes preceded by inflammatory ones, and my own experience confirms these facts. The bladder mucous membrane is devitalized by the rays and so is an easy prey to infective processes. The mere removal of a prostatic obstruction is only half the treatment; the other half consists in dealing with the changes in the bladder and kidneys.

In the malignant prostate the only chance of eradication of the disease is by means of surgery, as in every other organ of the body. The operation which I have introduced for the treatment of cancer in this region, and which I described in my paper read before this Section in March, 1924,¹ gives the patient a better chance of prolongation of life than any other present-day method.

I have been experimenting recently with a new electrical instrument on the lines of the cautery-punch of Young, but my treatment of tumours of the prostate by this means has been disappointing. In about 1 per cent. of cases diathermy was of value. Where the obstruction in the prostatic urethra is of fibrous tissue, this electrical treatment offers a more hopeful outlook.

Mr. FRANK KIDD.

The Section of Urology is to be congratulated on bringing up this question for discussion, as the time is ripe for the urinary surgeons to take counsel with the radio-therapists. For instance, a short time ago a general surgeon said to one of my patients, "In all cases of enlarged prostate, unless complicated by stone, the X-rays can make the gland shrivel up and disappear." Such a statement showed an inability to realize the magnitude and diversity of the problem of prostatic enlargement. Another patient writes: "I am told by a really good practitioner that he will never allow the removal of the prostate until X-rays have been tried. He says that he has known patients saved from operation and cured; but the only fly in the ointment is that, if X-ray treatment is not successful, the subsequent operation is rendered more difficult." By this statement he implies that some of the practitioner's patients are not cured, and are given a worse chance of successful operation. Evidently he does not find that "in all such cases the gland shrivels up and disappears."

Tumour formation in the bladder and prostate is a local disease in its earliest manifestations. If brought to the surgeon early the best chance of cure is still to be obtained by the knife; I am speaking not of a symptomatic but of a radical cure. Enthusiastic radio-therapists find it not difficult to convince certain easily-satisfied practitioners of symptomatic cures, a cure that lasts but for a season. In cases of "enlarged prostate," I advise the earnest-minded not to be satisfied with X-ray treatment unless it can be proved to bring about a radical cure, that is to say, a disappearance of the prostatic enlargement which could be demonstrated, were it to occur, by a method which I shall explain later. Until then they should suspend judgment.

¹ *Proceedings*, 1924, xvii (Sect. of Urol.), pp. 43-52.

The improvement in the results of the modern operations of partial cystectomy and prostatectomy is still far from being appreciated. For instance, since the end of 1921 I have operated upon ninety-two consecutive cases of simple uncomplicated adenoma of prostate, the operation having been carried out in one stage and the after-treatment carried out personally by myself. Eighty-eight of these patients are alive and well to-day. I regret to say four of them died, one from secondary hæmorrhage, one from cardiac failure, one from coma and one from pulmonary embolus. These eighty-eight patients are cured in the sense that the pathological cause of their disability has been removed and they are thus in a position to enjoy many years of comfortable and useful life—years which would have been denied to them but for the operation. These are no mere symptomatic cures. During the same period I removed simple enlargements of the prostate gland in thirty-seven cases by means of a two-stage operation, and two of these patients died, both of uræmia. This gives a total of 129 cases operated upon by both kinds of operation, with a mortality of six. The operation of prostatectomy, therefore, *in expert hands* does not present such a high mortality as that with which it is often credited, especially if the age of the patients operated on is considered, and if it is remembered that many of the patients put off operation until they are at death's door.

I can report thirty-five cases in which I have carried out the operation of sub-total cystectomy for carcinoma of the bladder during the last fourteen years. In twenty cases the patients are apparently cured, one 14 years, three 11 years, two 10 years since the operation. Eight were relieved for a long time but were not cured by the operation. Seven died as a direct result of the operation within six weeks from its date.¹

The public are in danger of forgetting that surgery can cure cancer in a fair proportion of cases. For instance, out of twenty-five cases of cancer of the breast operated upon by myself more than ten years ago, seven patients are known to be alive and well at the present date, and are undoubtedly cured. Eight are known to be dead; ten cannot be traced.

As far as I am aware, no list of a consecutive series of similar cases, accompanied by accurate observation of objective facts, with an account of immediate direct X-ray mortality, X-ray burns and final mortality, which can show a percentage of recovery in any way comparable with these surgical results, has yet been published by the radio-therapists.

Physical agents in the nature of radium, X-rays and the diathermy current, are steadily advancing as adjuvants to purely surgical measures, and no one appreciates their help more than the urinary surgeons. For instance, as regards my own employment of diathermy, out of thirty-four cases of simple papilloma of the bladder treated by means of diathermy applied through the cystoscope, twenty-five are known to be cured, thirty-two remained cured as long as they could be followed up, two only have relapsed and have been cured by open operation. Out of thirty-three cases of malignant papilloma submitted to diathermy through the cystoscope, twelve are known to be cured, seventeen remained free from recurrence as long as they could be followed up, and sixteen patients resisted the treatment and had to be submitted to open operation. Diathermy applied through the cystoscope can therefore cure the large majority of simple papillomata and nearly half the malignant papillomata met with in the bladder. In the latter group of tumours, diathermy followed by the implantation of radium emanation "seeds" may give an even higher ratio of cure. In the last five cases in which I have employed this method four show evidence of cure.

In many of the operations carried out at the present day on the bladder and prostate I feel like one of those men who used to figure on the beach, playing some twenty musical instruments at a time. One now operates not only provided with a knife, but surrounded by a number of physical agents such as electric light retractors, electric suction apparatus, electric cautery, diathermy machine, radium emanation "seeds" and an X-ray apparatus. As surgeons, we use all these physical agents to aid us in operation. I had intended to-night to discuss what all these different agents can do for us in helping our surgical measures. I must, however, postpone the major portion of my remarks for another occasion as the discussion to-night has focussed itself more particularly on deep X-ray therapy, and I will confine my remarks to this method of treatment, which I have tried to employ to as full an extent as has appeared to me justifiable.

Knowledge cannot be gained merely by quoting, in the manner of some of the radio-therapists who have spoken to-night, a number of cases examined merely as regards symptoms by a practitioner and submitted to X-ray therapy *without the objective examination of a*

¹ See "The Treatment of Epithelial Tumours of the Urinary Bladder," *Lancet*, March 17, 24, 31, 1922.

urinary specialist. Such subjective statements obscure the issue and are insufficient evidence. What is needed is objective evidence, which can be obtained surely enough, if only it is sought for earnestly, as I hope to show later.

Since the closing months of 1921 I have endeavoured to employ deep X-ray therapy in as many cases as I could possibly and justifiably do so. I find on looking up my notes that I have records of twenty-five cases of malignant disease that have been submitted to this treatment during that time. It would be tedious to relate each case in detail at this point in the course of my remarks, but the subject is so important that I consider all cases treated should be reported fully in a consecutive series, and not merely a selection of successful cases quoted which would appear to represent the treatment too favourably. I will therefore conclude my remarks with a full list of cases giving a brief record of the result in each case.

Summarizing the cases, I may say that eight of them were those of carcinoma of the prostate. Four were cases in which no operation of a radical type could be carried out. All four of the patients in these cases are dead. In none of them did the X-ray treatment exercise any favourable effect, and in two it appeared to make the patients very ill indeed and to hasten their end. In three of the cases I had removed what seemed to be simple senile enlargement of the prostate. Microscopic sections of the prostates removed showed early malignant changes. In these three cases the patients were exposed to the deep X-ray therapy and are now alive and well. Nevertheless the result has been no better than in a considerable number of similar cases in which patients showing these early malignant changes were operated upon, but were not submitted to deep X-ray therapy. Recurrence is not the rule in these cases, whether submitted to deep X-ray therapy or not. Finally there was a case in which I had removed the malignant prostate by Young's operation; the patient subsequently underwent deep X-ray therapy, but he died within two years of the operation. My impression, therefore, is that deep X-ray therapy can do little, if anything, for malignant disease of the prostate, except as a prophylactic measure following operation.

There were eight cases of carcinoma of the bladder. The patient in the first case came to me in January 1923 with an inoperable carcinoma on the left wall of the bladder. His general condition was poor and I did not think he could live more than a few months; but on my advice he received several exposures to the deep X-ray therapy according to the Erlangen technique. At first the results were startling and impressive. He lost all his pain and hæmorrhage and in six months time he went back to work and stated it as his own opinion that he was cured. I exhibit an X-ray picture taken of the bladder filled with air showing the extent of the growth in January, 1923, and I exhibit another view of the bladder filled with air taken six months later when the patient alleged that he was cured. By these pictures you can see that the X-ray treatment has rendered the growth smaller and more defined, but has not caused it to disappear. At the present date this patient is going rapidly downhill and is suffering from severe and painful X-ray burns. Impressed by the prolongation of life and the improvement in the symptoms in this case I submitted five similar cases to the deep X-ray therapy, investigating the results of the treatment by means of X-ray pictures taken with the bladder filled with air. In all the cases the immediate result has been good. There has been evidence in the X-ray pictures that the growths have undergone regressive changes and the symptoms have improved so that the patients have been able to go back to work. Nevertheless in no case has there been evidence that the growth has been caused to disappear, and in all cases sooner or later the symptoms have returned and there can be no doubt that none of these patients will survive very much longer. Two are already dead, the others are nearing their end. In two cases there was exposure to the deep X-rays after an operation of sub-total cystectomy; one patient so far remains well, the other already gives evidence of recurrence, and is dying.

My impressions are that deep X-ray therapy cannot give a severe knock-out blow to the cells of a malignant tumour of the bladder. After two treatments the rays seem to lose their lethal effect, presumably because the surviving cells become more resistant to X-ray bombardment. Eventually the carcinoma will get the better of the efforts to check it. X-ray treatment will undoubtedly prolong life for one or two years and may render the patient's life more comfortable if burns be avoided, as for a time it may relieve or even abolish pain, hæmorrhage, and distressing frequency of micturition.

I have submitted six cases of malignant disease of the kidney to deep X-ray therapy. The first case was operated upon in October 1921. The patient, a female, had a large "Grawitz tumour" of the right kidney weighing over three and a half pounds. I had to dissect it off from the vena cava, to which it was adherent for some inches, and remove some enlarged

glands lying in the renal pedicle. The patient appeared to be perfectly well until March 1923, when she returned with several malignant glands that could be felt in the region of the right renal pedicle. These glands were submitted to bombardment by the deep X-ray therapy, and for a time they disappeared and we were hoping that she might be cured. Nevertheless, towards the end of 1924, malignant glands made their appearance on the left side of the neck and were removed for section by the local doctor. Further X-ray bombardment was carried out on the neck but it was then observed that enlarged glands could be felt in the abdomen, and at the time of writing the patient is obviously going rapidly downhill with numerous secondary deposits in the abdomen. Here the X-ray therapy undoubtedly prolonged life, but it did not cure the disease.

The second case also showed impressive results. In September 1923 I was asked to see a woman who was passing blood from the bowel and the bladder. She had an enormous tumour of the right kidney, with large secondary deposits in the glands of the renal pedicle. She was profoundly wasted and anæmic and I did not think she could live more than a few weeks. The friends were anxious for anything possible to be done, and she was submitted to deep X-ray therapy. The first treatment very nearly proved fatal, but she just pulled round, and at the end of six months she had had no more hæmorrhage, the tumour of the kidney had shrunk to less than half its former size, she had put on weight, and the blood-picture had improved. The treatment was persisted in, but the patient died in November 1924, exactly a year after she began to undergo the X-ray treatment. This case-record again shows that the deep X-rays can kill off a number of malignant cells and can delay, at first, proliferation of the cells left behind. They can also arrest hæmorrhage and relieve symptoms. Nevertheless, in the end the malignant tissues would appear to get the better of the X-ray bombardment.

Four other cases in which I have removed malignant tumours of the kidney since July, 1923, have been submitted to prophylactic doses of X-rays after the operation. In two of the cases in which the tumour was a Grawitz tumour the patients are alive and well at the present date. The patient in a third case, which was true carcinoma of the kidney—most malignant of all forms—has lately shown symptoms that suggest a glandular recurrence. In the fourth case, one of true carcinoma, the patient is already dead of recurrence. The remaining cases briefly recorded below speak for themselves. In some, deep X-ray therapy has produced a certain ameliorating effect, but does not appear in any case to have produced a real cure.

One can only conclude, therefore, that from personal observation deep X-ray therapy can effect a certain amount of amelioration in cases of carcinoma of the bladder and kidney, but one cannot infer from results that it is likely to produce a cure. I have come to the conclusion that more than two exposures should seldom be carried out in any given case, and that at intervals of three to six months, depending on the general and local reactions. After two treatments had been given my impression is that the cells remaining behind have become resistant to X-ray bombardment, so that no longer can any favourable effects be looked for; and in fact the X-ray applications may serve to stimulate to more active growth the cells still left behind, and may lead to severe burns.

Deep X-ray treatment is also not without its dangers. Everyone expects operative treatment to entail a certain degree of mortality, and for this reason the public dreads the mere word "operation." Nevertheless, I feel sure that if all cases which have been treated by X-ray therapy had been published it would be found that the treatment itself has quite a definite operative mortality. Reading between the lines of Dr. Finzi's address it is clear that he has had experience of mortality after deep X-ray therapy. So far I have been fortunate enough not to have a death in any of my cases, but several of the cases have been very near death after the treatment. One Member has related that in three cases of prostatic disease which he submitted to the treatment, two patients died within a few days, apparently as the result of the treatment. It is particularly dangerous in cases where there is profound anæmia and where the function of the kidneys is very much depressed by back-pressure. It is possible that it produces protein shock by the absorption of dead proteins from the focus bombarded. It certainly destroys a large number of blood-corpuscles. When applied over the abdomen it depresses the function of the cells of the kidneys, adrenals and liver. It destroys the cells of the skin and the hair follicles. The more embryonic and atypical the cell the more susceptible it seems to be to selective death by means of the gamma rays. But it must not be forgotten that the death of malignant cells will usually lead to increase in the fibroblasts surrounding the dead cells, and therefore that severe fibrosis may occur as the result of the X-ray bombardment, as it certainly does after radium and diathermy treatment.

Such fibrosis, if it were in the neighbourhood of the urethra or the ureters, might make things worse by the production of back-pressure on the bladder and kidneys. It might be shown that the deep X-rays cause the simple, enlarged prostate to "shrivel up," yet if that process resulted in a replacement fibrosis, the last state of the man might be worse than the first.

It is possible that the application of deep X-rays to malignant tissues produces necrosis of cancer-cells and the absorption into the blood-stream of the products of the dying cells. Such absorption might produce a certain degree of general immunity and the formation of antibodies to combat the remaining cancer-cells. Such a hypothesis should not be lost sight of, and it may be that some day better results will be obtained from small doses of X-rays frequently repeated, rather than from one or two massive doses that can only be repeated at long intervals because of the danger to the skin and surrounding tissues.

Summary.—Deep X-ray therapy may be expected to produce a certain amount of cancer-cell death, a lessening of vascularity of a tumour and an increase in fibrous tissue around the tumour. It may also produce a certain degree of active immunity. On the other hand, its immediate effect when applied over the abdomen is to cause protein shock, a destruction of red blood-corpuscles, and a depression of renal, adrenal and hepatic function which, debilitated patients, cannot incur without risk to life. If persisted in too long it may render the cancer-cells more resistant and even hasten metastasis. If given too frequently or in too great intensity it may produce severe, painful gangrene of the abdominal wall and sacral region. This took place in eight per cent. of my cases.

We are thus brought to the discussion of the problem as to what deep X-ray therapy can do for "simple senile enlargement of the prostate," "adenoma of the prostate," "hypertrophy of the prostate"—whichever term we prefer to apply to the condition. Here we are dealing with what appears to be in essence an overgrowth of all the normal tissue-cells of the prostate in response to the "rheums of age." On a *priori* grounds it would seem an unreasonable proposition, and one that savours of magic, to expect to bombard such an overgrowth of cells so nearly related to the normal, across a large expanse of normal tissue and to expect that the rays, as though endowed with reason, would only select for death the glandular fibrous and muscular cells of which the enlarged prostate largely consists, and yet lead neither to fibrosis nor to any destruction of the normal cells of the bladder, rectum and surrounding tissue. Radium and diathermy applied direct to a tumour undoubtedly destroy not only many malignant cells but also many of the normal cells of the tissues surrounding the tumour. They destroy particularly cells of the mucosa of the urethra and rectum, and lead to an intense fibrosis. Why, then, should deep X-rays prove an exception? If they really reach the part on which they are focussed and really destroy the cells of the hypertrophied gland, it then seems reasonable to expect them to destroy at the same time a portion of the surrounding healthy tissues and to set up a grave fibrosis. Nevertheless, the problem should not be dealt with on a *priori* grounds, nor on the evidence of symptoms, but by the accurate observation of objective facts. Let us apply the deep X-ray treatment to the simple senile prostate, and see what does actually and objectively happen. So far, all we have heard to-night on this subject from Dr. Reginald Morton, the protagonist of deep X-ray therapy, is that in cases of simple enlargement of the prostate gland, so long as accurate technique is practised, the deep X-rays produce good results in nearly every case. This is a mere expression of opinion, not accurate observation of facts, and it is facts we want when deciding on this very serious problem of the X-ray versus operative treatment.

Perhaps it is not fully appreciated by some practitioners or by the X-ray therapists how very easy it is to persuade a patient complaining of the early symptoms of enlargement of the prostate gland that his symptoms have become alleviated. All of us who are accustomed to deal with such cases know how common it is for nervous patients to come to us complaining of difficulty in passing water, poor stream, increased frequency of micturition—symptoms which are put down to senile enlargement of the prostate gland. Many of these patients, when examined by modern methods of precision, are found not to have enlargement of the prostate at all, and when reassured as to this fact they soon lose all their symptoms. How easy it must be to treat such patients with the deep X-ray and then to think you have cured an enlarged prostate, which never actually existed! On the other hand, many of these patients are suffering from early enlargement of the prostate gland, but they go away after a full examination and will write six months or a year later to say they have lost all their symptoms. They have had no treatment at all. Some of these patients live out their natural term and never suffer from back-pressure or from painful and urgent symptoms. Nevertheless, as time goes on, many return once more with increase of symptoms, perhaps

with retention of urine, or with signs of impending renal failure, so that in the end they have to submit to an operation, though previously they have been going about and saying they were cured. Again, you can submit such patients to drug treatment, electric stimulation with the Morton static wave or to suggestion treatment, and for a time many of them will lose their symptoms. The statements therefore that Dr. Reginald Morton has made do not furnish sufficient evidence in proof of the efficacy of the deep X-ray treatment.

ILLUSTRATIVE CASES.

Case I.—A man, aged 47, complained to his doctor that he had noticed for some years increased frequency of micturition, especially at night, and that his flow of urine seemed to require an extra effort to keep it going just as he was finishing the act of micturition. The doctor proposed to remove his prostate gland without delay. The patient asked for a further opinion. Examination revealed no organic disease of the nervous system, no stricture visible by the urethroscope, no enlargement of the prostate gland palpable *per rectum*. Cystoscopy failed to reveal the presence of residual urine, nor was there any evidence of trabeculation of the bladder or of upgrowth of the prostate into the cavity of the bladder. The urine contained phosphates but no other abnormal constituents. The patient had been worried by his business and was sleeping badly. The symptoms were due to worry and phosphaturia, and they disappeared completely as soon as these factors were removed.

Case II.—A doctor, aged 55, overworked and sleeping badly, complained of a smarting pain after micturition and vesical irritation. His partner told him he could feel a great enlargement of the prostate gland and proposed to operate at once. The patient sought a second opinion. A complete examination revealed the fact that there was no senile enlargement of the prostate gland. The effects of the overwork were corrected and the symptoms disappeared.

In further illustration of this point I can quote the case of a man with simple enlargement of the prostate gland brought to me by a physician who said that he wished X-ray therapy tried before operation. I said I could have no objection to this provided I were allowed to examine the patient before the treatment was applied and at intervals after it had been applied. I examined the man with the cystoscope, and saw that he had a moderate simple enlargement of the prostate gland, and the X-ray treatment was therefore begun. He was seen regularly for six months both by the physician and by the X-ray therapist. Their notes always read: "Great improvement, symptoms disappearing." Nevertheless, six months later I was called to see this patient in the middle of the night with complete retention of the urine. He besought me to waste no more time but to open his bladder at once. This I proceeded to do, and eventually I removed the largest prostate gland it has ever been my fortune to encounter. Here you have the case of a man who for six months steadily maintained that his symptoms were abating, and he even persuaded a learned physician and an accomplished X-ray therapist that his prostate was diminishing in size as the result of the treatment, they simply judging by the symptoms and not by physical signs. Nevertheless events proved that the X-rays had actually encouraged the growth of the prostate to an enormous extent, and the symptoms were shown to have had no bearing whatever on the question.

Discouraged by the result of this case I discussed with my deep X-ray therapy colleague the question as to whether he was willing to try the treatment in any more such cases. He expressed himself as not willing to do so, as he did not see, on *a priori* grounds, that the treatment could be expected to do any permanent good or produce a radical cure. I cannot, therefore, quote any more cases actually observed by myself. Nevertheless, I can point the way to a method by which actual objective facts can soon be gathered to test this question, as compared with subjective statements concerning symptoms, which cannot supply the whole truth.

AERO-CYSTOGRAPHY.

The following method should be employed in all cases submitted to X-rays, if true conclusions are to be reached. For some years I have been accustomed to fill the bladder with air and subsequently to take X-ray photographs. *This procedure possesses certain risks, and is one not to be lightly employed by those who are not familiar with its technique.* For that reason I have not published any account of the method before, though I have used it extensively in my own clinic. The method enables one to estimate in a fairly exact manner the actual size of an enlargement of the prostate gland or of a carcinoma of the bladder. From certain remarks made to-night it is clear that some are under misapprehension as to the power to estimate the size of an enlarged prostate or growth of the bladder by means of rectal

examination or by means of the cystoscope. In many cases of enlargement of the prostate the enlargements do not project towards the rectum at all, they grow up through the internal sphincter of the bladder and project into the bladder-cavity. In these cases rectal examination may not suggest that the prostate is enlarged at all, but it is possible to examine the prostate as it projects into the bladder by means of the cystoscope. Nevertheless, the nearer the cystoscope is held to an object the larger it appears; the further away it is held from an object the smaller does it appear. Only when a cystoscope is held at a distance of about an inch from an object does that object appear its natural size. It is therefore impossible to form an accurate estimation of the true *size* of an upward projecting prostate or of a growth in the bladder by means of cystoscopy alone, and even less so by means of rectal examination. Aero-cysto-radiography has now put into our hands the means whereby we can estimate accurately the size of an enlarged prostate or of a carcinoma of the bladder before submitting it to X-ray treatment, and at intervals after the X-ray treatment (figs. 1 and 2). There is therefore no excuse for the radiologist not to present us with accurate objective observations as to what does actually happen after X-ray treatment of a simple prostate; so that before



FIG. 1.—Aero-cysto-radiograph. Growth in bladder before deep X-ray treatment.

any effort is made to judge of the merits of X-ray therapy I urge that such observations should be made. I exhibit X-ray photographs which show the size of enlarged prostates when the bladder is filled with air (fig. 3).

If it can indeed be shown by this method that the application of the deep X-rays does cause an enlarged prostate gland to "shrivel up" *without any corresponding fibrosis*, then the discovery is epoch-making. I must emphasize once more that it would have to be shown that such regression of growth, or "shrivelling up," if it did occur, did not also lead to a fibrosis—a fibrosis which might result in worse obstruction to the neck of the bladder than the enlargement of the prostate gland itself.

I will now generally discuss the whole question of enlargements and disorders of the prostate gland, to make clear how complex the diagnosis of such troubles can be. Many seem to think there are only two diseases of the prostate gland—simple senile enlargement and carcinoma.

Prostatic symptoms often arise as functional manifestations in overworked, neurotic subjects suffering from phosphaturia and other functional disorders. In a large number of

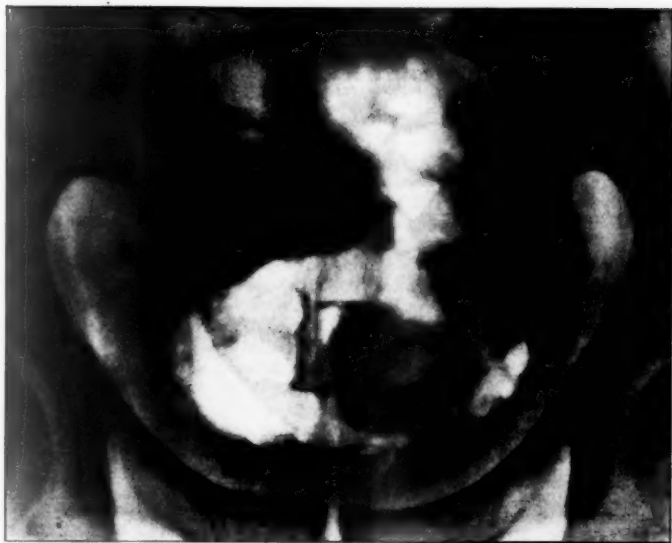


FIG. 2.—Aero-cysto-radiograph. Growth in bladder after deep X-ray treatment.

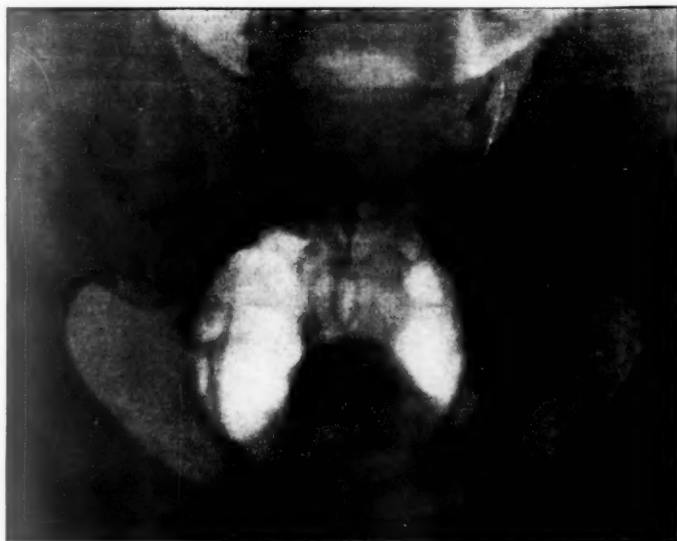


FIG. 3.—Aero-cysto-radiograph. Showing size of enlarged prostate.

cases, even in patients over fifty years of age, true prostatic disease is due to inflammatory trouble caused by the gonococcus, the *Bacillus coli* and related bacteria, sexual irregularities, horse-riding and bicycling. Such inflammations may give rise to temporary enlargements, or to the formation of prostatic bars and sclerosis of the neck of the bladder. These troubles can be eradicated by expert manipulations without the need for X-ray treatment or for open operation. Prostatic trouble may be due to tuberculosis, to late manifestations of syphilis, and may also be caused by stones in the prostate, or by fibrous contraction the result of past inflammation. Curiously enough, some of the most advanced cases of prostatic obstruction may be due to tiny little collar prostates, the nature of which is not yet fully worked out, and the removal of which results in complete cure. Prostatic enlargements may be caused by simple hypertrophy, by multiple adenomata, by adenomata in which the microscope reveals early malignant changes, by slow-growing "sclerosing" carcinoma, by rapidly growing "encephaloid" carcinoma, and, in the young, even by sarcoma. Some results of prostatic enlargement are vesical calculi, vesical pouches with or without calculi, papilloma and carcinoma of the bladder, acute retention, chronic retention with overflow, severe septic cystitis, back-pressure kidneys and pyelitis. Some of the cases are also complicated with urethral venereal stricture and by tabes. As a result of renal inadequacy or as the result of old age many of the patients are sufferers from profound anemia, failing heart, bronchitis, glycosuria, and other troubles.

A complete diagnosis, therefore, includes not only the exact nature of the prostatic trouble, but an evaluation of the secondary accompanying lesions in the urethra, bladder, and more especially the kidneys, and of the conditions of health or disease of all the other organs of the body, particularly of the blood, heart, lungs, and nervous system. Such a diagnosis can best be worked out by men trained in the examination of such cases by long years of study. Many of the cases therefore, either from the urinary complications present or from their poor general condition, are quite unfitted to be submitted to the strain of severe X-ray treatment.

To illustrate these points I worked out the complications present in a series of 139 cases that would have contra-indicated X-ray treatment, or made its employment doubtful and dangerous. They were as follows:—(a) Renal inadequacy with chronic retention and giant overflow bladder in 44; (b) severe infection of bladder and kidneys in 33; (c) lung troubles, failing heart or profound anemia in 26; (d) vesical stone in 19; (e) prostatic stone in 5; (f) small fibrous prostate in 7; (g) complete retention in 5; (h) early microscopic malignant changes in 8 (many authors give this complication as from 14—20 per cent.); (i) glycosuria in 2; (j) urethral venereal stricture in 3; (k) vesical pouches that needed excision in 4; (l) accompanying papilloma of bladder in 2; (m) tabes in 1. From these figures it can be seen that a large number of patients put off coming to the specialist until their trouble has reached its final stages, at a time when operative treatment is bound to present a considerable risk. At such a time they are long past the stage at which X-ray treatment could be considered as either safe or justifiable. I fear that if X-ray treatment becomes fashionable for these disorders, cures will not be forthcoming, and the treatment will be responsible for yet further delay in bringing up the patients for radical cure, as has happened in the case of cancer.

X-ray therapists, and the general surgeon whose remarks I quoted above, seem to me to miss the whole point of this problem. It is not simply the removal of the enlarged prostate that is the ultimate aim of our operation; it is the effects of back-pressure that this prostate produces on the bladder in the way of pouches, cystitis and stones, and on the kidneys by way of pyelitis and renal failure, that we wish to prevent by the removal of the prostate. It might turn out that X-ray therapy did destroy, partially or even completely, the enlarged prostate, and yet it might prove that such destruction brought in its train an obstructing fibrosis which was far more dangerous to the patient than the original enlargement of the prostate. It is not the size of the prostate that is important. Many quite small prostates produce advanced obstruction to the kidneys; many very large prostates produce hardly any obstruction at all. It is not the mere symptoms of poor stream and increased frequency of which the patients complain that we are really treating when we carry out the operation. In former days physicians treated symptoms. That method should long ago have been discarded. The radio-therapist appears to go back to the good old days of treating the symptoms. Our real aim and object is to prevent attacks of retention of urine, to prevent stone, to prevent pouch formation, to prevent cystitis and pyelitis, and, most important of all, to prevent the gradual silent death of the kidneys from back-pressure. The kidneys perish so insidiously from back-pressure. A man may come to the verge of the renal precipice without any painful symptoms.

This week he appears quite well to himself and his friends; next week he may die in coma. That is why the problem is so very much more complicated than the radio-therapist has so far realized. No case ought, therefore, to be submitted to deep X-ray therapy unless an accurate diagnosis of all the factors involved has previously been made by a trained urologist. In the first place, is an enlargement of the prostate present at all? This can only be determined accurately by means of cystoscopy and aero-cysto-radiography. In the second place are other complications present, such as stone, vesical pouches, cystitis, pyelitis, threatening renal failure, early carcinomatous changes, old venereal stricture in front of an enlarged prostate? Radio-therapists cannot seriously maintain that they are able to destroy a large stone behind an enlarged prostate, or a fibrous venereal stricture in front of an enlarged prostate. Yet these conditions are often present, and are missed if they are not looked for by modern methods. Nor can radio-therapists restore the function of damaged kidneys, and in fact they will by their treatment turn the scale in the face of many men with damaged kidneys if they are not extremely wise and careful, and their "operative" mortality will be very high indeed.

Again, an accurate estimate ought to be made of the amount of obstruction to the bladder and kidneys caused by the condition of the prostate. Is the bladder seen to be hypertrophied and pouched by means of the cystoscope? What is the amount of residual urine? Is cystitis present, for if so it will be aggravated by the X-rays. How much back-pressure change is present in each case as estimated by renal function tests? Is the bladder a painless giant bladder overflowing every hour? In such a case no catheter should be passed, and it is certain that the kidneys are in a condition of advanced renal failure. If the radio-therapists were to attempt to treat this type of case, I feel certain that there would be something like a 75 per cent. mortality from the treatment alone.

I think I have shown, then, that the problem is very much more complicated than the radio-therapist supposes, and it can only be attacked in co-operation. The patient in the case should be examined thoroughly by a urinary surgeon before the treatment is begun, and he should be examined thoroughly every three months for at least a year after the treatment is over. No cases should be published unless these conditions have been complied with, otherwise a false impression may be given to the public and many patients lose their lives from back-pressure kidneys who otherwise might have lived for many years.

I give below a complete list of cases of carcinoma submitted after my examination to deep X-ray therapy. I do not know that the results are very cheerful reading. The prospects of obtaining cure in cases of carcinoma by means of X-ray treatment alone appear to be meagre.

Carcinoma of Prostate.

(1) Aged 59. Seen October 14, 1921. Diagnosis: Carcinoma of prostate. General condition good. X-ray treatment, November, 1921. Result: March 2, 1922; large extension of growth, abdominal glands enlarged. Died shortly afterwards.

(2) Aged 58. Seen December 21, 1921. Diagnosis: Carcinoma of prostate. General condition good. X-ray treatment, January and June, 1922. Result: Writes June 14, 1922, that X-rays have upset him very much. Died October, 1922.

(3) Aged 74. Seen November 18, 1920. Diagnosis: Carcinoma of prostate. General condition good. Suprapubic drainage, November, 1921. X-ray treatment, December, 1921, and June, 1922. Result: Disastrous, made an old man of him, unable to do any work afterwards. Growth extended and he died a few months later.

(4) Aged 61. Seen November 7, 1922. Diagnosis: Carcinoma of prostate, early. Young's radical operation, November 22, 1922. General condition good. Healed up well. X-ray treatment, June, 1923. Result: June 20, 1923, apparently well. Died of recurrence, November, 1924, two years after operation.

(5) Aged 59. Seen February 5, 1924. Diagnosis: Carcinoma of prostate. General condition fair. Suprapubic drainage, February 14, 1924. X-ray treatment, March and May, 1924. Result: Died August, 1924.

(6) Aged 60. Seen December, 1922. General condition good. Diagnosis: Clinically benign prostate; removed January 2, 1923. Microscopic diagnosis: Carcinoma. X-ray treatment, March, 1923. Result: Good. March, 1923, alive and well, no sign of recurrence.

(7) Aged 73. Seen May 13, 1924. Diagnosis: Clinically benign prostate; removed May 19, 1924. Microscopic diagnosis: Carcinoma in adenoma. X-ray treatment, October, 1924. Result: March, 1925, alive and well, no sign of recurrence.

(8) Aged 61. Seen June 7, 1923. Diagnosis: Stricture, tabes, clinically benign prostate. Prostate removed, June 12, 1923. Microscopic diagnosis: Carcinoma. General condition fair. X-ray treatment, October, 1923. Antisyphilitic treatment for one year. Result: August, 1924, condition satisfactory except for lightning pains. No sign of recurrence, March, 1925.

Carcinoma of Bladder.

(1) Aged 58. Seen January 5, 1923. Diagnosis: Carcinoma of bladder. General condition very poor, wasted, pain, hæmaturia. X-ray treatment, February, May and July, 1923, June, 1924. Result: Put on weight, lost all symptoms, went back to work. Final result: July 13, 1923, bleeding again; aero-cysto-radiography; growth smaller, still present. February 7, 1924, still at work, has had pain, and a lump can be felt in abdomen. June, 1924, still at work. March, 1925, doctor writes that he is still walking about but obviously failing; hæmaturia profuse, done no work since July, 1924; four X-ray burns, one gangrenous and very painful.

(2) Aged 54. Seen May 11, 1923. Diagnosis: Carcinoma of bladder. General condition good. X-ray treatment, June and August, 1923. Result: August, 1923, no blood, frequency normal. Final result: Large ulcer appeared on back, September, 1923, as a result of X-ray treatment, died December, 1924. Post-mortem: Growth in bladder appeared black and dead, dense adhesions binding it to rectum. Died of renal failure as a result of these adhesions, which pressed on mouths of ureters.

(3) Aged 52. Seen March 1, 1924. Diagnosis: Papilliferous carcinoma of bladder, sessile, above left ureter. March 6, 1924, partial cystectomy. Sections showed true carcinoma. General condition good. X-ray treatment, June, 1924. Result: March, 1925, no sign of recurrence, quite well.

(4) Aged 64. Seen February 7, 1921. Diagnosis: Malignant papilloma. Two diathermy treatments. No trouble until November, 1922. Cystoscopy: Old tumour gone, fresh carcinoma on back wall of bladder. Diathermy, November, 1922. Diathermy, plus radium, February, 1923. Partial cystectomy, May, 1923. Small carcinoma (squamous-cell), abscess behind due to radium. X-ray treatment, February, 1924. No recurrence in bladder; recurrence behind in pelvic tissue. Second X-ray treatment, June, 1924. Result: July 1924, faecal and urinary fistula appeared in abdominal wall. February, 1925, fistula nearly healed, urine and faeces come through rectum. March, 1925, faecal fistula reopened. On morphia all the time, rapidly sinking.

(5) Aged 68. Seen June 30, 1924. Diagnosis: Large malignant growth on anterior wall of bladder, involving internal meatus. General condition good. X-ray treatment, July and December, 1924. Result: November 16, 1924, health improved, bleeding stopped. November 27, 1924: X-ray with air: growth much less extensive but still present. Final result: March, 1925, still alive but growth progressing.

(6) Aged 58. Seen July 16, 1924. Two years' hæmaturia. Diagnosis: Large malignant ulcer on right wall of bladder, involving internal meatus. General condition poor. X-ray treatment July and September, 1924. Result: Symptoms improved for a time. Final result: Died March, 1925.

(7) Aged 67. Seen August 5, 1924. Eight years' hæmaturia. General condition good. Diagnosis: Ulcerating carcinoma on right lateral wall of bladder, involving internal meatus. X-ray treatment, September, 1924, and January, 1925. Result: Writes October, 1924, to say he is cured, hæmorrhage has stopped. March, 1925, has had several attacks of hæmaturia since second treatment, condition not much improved.

(8) Aged 68. Seen October 25, 1923. Diagnosis: Whole bladder-wall a mass of ulcerating carcinoma. X-ray treatment, November, 1923, and March, 1924. Result: January, 1924, free from all symptoms, in perfect health. January 14, 1924: Aero-cysto-radiography: growth smaller but still present. December 8, 1924, no real pain, only one attack of hæmaturia during year; X-ray with air: growth on top of bladder still present but much smaller. Large recurrence could be felt in abdomen.

Malignant Disease of Kidney.

(1) Aged 47. Seen October 12, 1921. Diagnosis: Tumour of right kidney. Nephrectomy, October 19, 1921. Microscopic report: innocent, papillary-cysto-adenoma. General condition good. March, 1923, recurrence in abdominal glands. X-ray treatment April and October, 1923. October, 1924, recurrence in left supraclavicular glands and abdominal glands. Third X-ray treatment October, 1924. Final result: February, 1925, dying.

(2) Aged 45. Seen September 24, 1923. General condition: dying. Diagnosis: Inoperable tumour of right kidney. Hematuria, blood in bowel, secondaries in glands, anemia, wasting, swollen legs. X-ray treatment October and December, 1923; June, 1924. Result: March, 1924, growth half the size, no more blood, general condition improved. Final result: Died November, 1924.

(3) Aged 44. Seen July, 1923. General condition good. Diagnosis: Hypernephroma of right kidney. Two years' history. Nephrectomy, July 26, 1923. X-ray treatment October, 1923. Result: March, 1925, alive and well.

(4) Seen September, 1923. General condition fair. Diagnosis: Carcinoma of left kidney. Nephrectomy, December 16, 1923. X-ray treatment February, 1924. Result: Severe nausea and indigestion followed for a time. Final result: March, 1925, alive and well.

(5) Aged 64. Seen June 13, 1924. Diagnosis: Carcinoma of left kidney, early. Nephrectomy, June 17, 1924. X-ray treatment October, 1924. Result: January 21, 1925, severe pain in left loin, no recurrence to be felt; X-ray: *nil*. Final result: March, 1925, phlebitis of left leg, no definite evidence of recurrence.

(6) Aged 40. Seen December 17, 1923. General condition fair. For one year tender, enlarging right kidney, fever, no blood. Diagnosis: Carcinoma of right kidney. Nephrectomy, December 24, 1923. Carcinoma had not burst into pelvis; secondary glands removed. Internal vena cava had to be tied in course of operation. Healed without trouble. X-ray treatment, February, 1924. Result: Died shortly afterwards.

Miscellaneous.

(1) Aged 64. Seen July 1, 1921. Diagnosis: Carcinoma lower jaw. General condition fair. X-ray treatment July, 1921. Result: Died a few months later.

(2) Aged 42. Seen November 27, 1922. General condition good. Diagnosis: Spindle-celled sarcoma of mesentery of small intestine; removed with 1 ft. of small gut, December 7, 1922. X-ray treatment April and October, 1923, February, 1924. Final result: Died April, 1924, of recurrence in abdomen.

(3) Aged 60. Seen October 8, 1915. Pain in urethra for three months, frequency, no blood. Diagnosis: Cystoscopy, October 19, 1915; early carcinoma of urethra can be seen reaching bladder with cystoscopy. Can be felt and seen outside. Radium treatment November, 1915, for five days running. February, 1916, second radium treatment. Cystoscopy June, 1916; small extension in bladder, main growth had gone. July 7, 1916, third radium treatment. May, 1917, appeared to be cured. April, 1920, growth had returned and was more extensive; radium again. July, 1920, symptoms gone, growth has gone again. October, 1920, recurrence, hematuria. March, 1921, X-ray treatment. Final result: January 13, 1925, occasional difficulty with urination; hematuria; large bulging, extensive recurrence in urethra, with roots extending into pelvic tissues on each side.

Nevertheless deep X-ray therapy has established its position as a useful handmaid to surgery in the treatment of cancer, particularly as a prophylactic measure against recurrence after operation, and by its rendering some inoperable cases operable. It may also prove a wonderful resource in inoperable cases, as it may prolong life for a considerable period and by its merciful power in many cases to relieve pain and hemorrhage, it may make the increased length of days granted to sufferers to be altogether more bearable. It may increase the economic value of the bread-winner by enabling him to continue at work long after he could have done so without its aid.

Dr. GEORGE VILVANDRÉ.

The utmost care in establishing a correct diagnosis of malignant disease is needed before a claim of cure by X-ray therapy can honestly be made. Personally, I always ask for a section by a competent pathologist whenever such is at all possible and I will accept no claim of cure unless based on such diagnostic evidence. The pitfalls of clinical diagnosis are many. We have been told of the wonderful distant effect of X-ray therapy—such as the curing of a facial malignant lesion while a patient was treated for malignant prostate. There is no reliable evidence for such statements. I do not hesitate to say that I do not believe such a statement.

As a contra-indication to the X-ray treatment of enlarged prostate it has to be remembered that what appears to be a benign, enlarged prostate is sometimes malignant in its centre; and the risk of treating such an early lesion by X-rays, and not surgically where total removal is possible, must be carefully considered.

One speaker complained that bladder cases come to the radiologist in a septic state, but they come to the surgeon in a similar condition. The surgeon has also to deal with malignant bladder complicated with cystitis or ascending pyelitis.

In my experience carcinoma of the prostate has in a few cases shown some slight regression at first under X-ray treatment, but before long the disease has advanced again. More beneficent results followed in two cases of sarcoma of the prostate, but here again there was the usual fatal termination after a time.

The risk of a rectal burn, as reported by one of the speakers, aggravates the patient's condition without giving him the compensation of relief.

I know of two cases of carcinoma of the prostate treated by radium in which for a couple of months improvement was obvious, but following this the end came within a few weeks. X-ray treatment can alleviate some cases of carcinoma, can even cure in those few instances in which superficial early lesions are present; but long before we know it, however, secondary growths are often present, and treating the local focus does not arrest or cure the disease.

Dr. N. S. FINZI (in reply).

I quite agree with Dr. Webster as to the hopeless nature of most of the cases which are submitted to us, and am inclined to think that the bad results described by Mr. Kidd are due to this cause. I imagine that most, if not all, of the twenty-five cases of carcinoma of the prostate which he mentioned are of this nature.

As to Dr. Reginald Morton's statement that an infected malignant growth can never be cured by radiations, I venture to dispute this, as I have several cases of infected growths that have completely recovered; one of them was a very much infected round-celled sarcoma of the muscles of the front of the thigh, as proved by microscopical section. The patient completely recovered, and has remained well for thirteen years up to the present time. There is a method of dealing with this infection which I have used in treatment of cervix cases, but I do not know whether it has been applied to prostate or bladder cases, that is to ionize with copper or zinc. The effects of this on septic infection are very striking, and in very badly infected cases I carry this out as a preliminary to radiation treatment.

I do not believe that it is possible, as Dr. Browne-Carthew suggests, to cut off the blood-supply to a growth in the prostate by rectal application. I think the blood-supply to the prostate is far too widespread to be affected to any extent by radiations from the rectum.

With regard to Mr. Clifford Morson's remarks, I have already stated that I consider that every case should be examined by a urologist, and it is up to the latter to see that the proper tests are applied. I simply submit the case to a urologist, when the patient has not been sent to me by one, to whom I look to carry out all the tests that he requires. With regard to the reduction in size of the prostate, I can definitely state this occurs, not on my own evidence, but on that of the urologists who have examined the cases for me. If the removal of the obstruction is only half the treatment, then removal of the prostate is only half the treatment. As to retention of urine, this apparently also occurs sometimes after operative measures.

In answer to Mr. Kidd's question about statistics, I am not prepared to quote statistics for a number of years until I have a larger number of cases. In cases of simple enlargement of the prostate we cannot get the cases, as they are generally seized upon by the surgeon, who removes the prostate.

The increased resistance of carcinoma cells to subsequent application of X-rays is well known to radiologists, and definite experimental work lending support to this experience has now been carried out. The first treatment is always the most effective. The second may have some effect, and possibly even the third, but the carcinoma cell gradually acquires an immunity to the effect of the rays.

Section of Urology.

President—Mr. CYRIL A. R. NITCH, M.S.

The Tolerance of the Body for Urea in Health and Disease [1].

By H. E. ARCHER, L.R.C.P.Lond., M.R.C.S.Eng., and
G. D. ROBB, M.B.

The investigation consisted in the observation of the reaction of normal and abnormal patients to a dose of urea by mouth. The curve of the level of urea in the blood was traced in specimens taken before one hour, two hours and four hours after the dose, and the urine was similarly collected and estimated for urea.

The micro-method of Twort and Archer [2] was employed for the estimation of urea in the blood.

The healthy subject was first investigated, and the curve of blood urea, after a 15 grm. dose of urea, was as follows:—

Before urea	0'025 to 0'040 per cent.
Rise within sixty minutes	0'010 to 0'015 per cent.
Recovery complete within 120 minutes.	

An hourly fluctuation within 0'006 per cent. found in the normal subject is allowed for.

Dosage is important—25 grm. urea by mouth gives totally different results.

Abnormal subjects, mainly renal cases, were investigated.

Nine cases of severe renal deficiency were investigated, and showed a deficiency in the tolerance-result every time. The concentration test gave deficiency in eight only, and in five only was the resting blood-urea definitely above normal.

Twelve cases of less marked renal deficiency were observed. All of these were known to have, or to have had, some interference with the kidneys which was conceivably embarrassing their function when examined. In these the following results were observed:—

(1) Resting blood-urea level	Normal in every case
(2) Concentration test	Normal in all excepting two cases
(3) Tolerance test	Deficiency in all excepting one case

On these and other grounds, the tolerance curve was put forward as a physiologically sound renal test, which, in cases examined to date, would appear to be more sensitive than either the resting blood-urea level or the concentration test.

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[1] ARCHER and ROBB, "The Tolerance of the Body for Urea in Health and Disease," *Quarterly Journal of Medicine*, April, 1925, xviii, No. 71. [2] TWORT and ARCHER, "The Experimental Production of a Fatal Nephritis," *Lancet*, London, 1923, i, p. 1102.

A New Diathermy Punch Operation for Prostatic Obstruction.

By KENNETH M. WALKER, F.R.C.S.

IN the short time at my disposal I shall give a brief description of the diathermy punch and the steps that led to its construction. I shall then indicate the type of case in which it may conveniently be employed.

Several types of punch have been evolved since Hampton Young introduced his instrument in 1909. My own criticism of Young's instrument is that it is only possible to remove with it a very small amount of prostatic tissue, and it was for

this reason rather than on account of any subsequent hæmorrhage that I abandoned its use. Nine months ago I obtained from Mr. J. R. Caulk, of Louisville, U.S.A., an instrument which he designed some years ago, and named the cautery punch. This instrument resembles Young's punch in its main outline, but the removal of the prostatic tissue is effected by a circular platino-iridium knife heated to a dull red head by an electric current of low voltage and high amperage. Here there is no danger of hæmorrhage and it is possible to clear the passage more effectually than with Young's pouch. Unfortunately, however, it necessitates the use of a current that is not always easy to obtain in London without the employment of costly transformers, and after having burnt through my blade as the result of using an unsuitable current, I decided that it would be preferable if diathermy rather than a galvano-cautery could be utilized for this purpose.

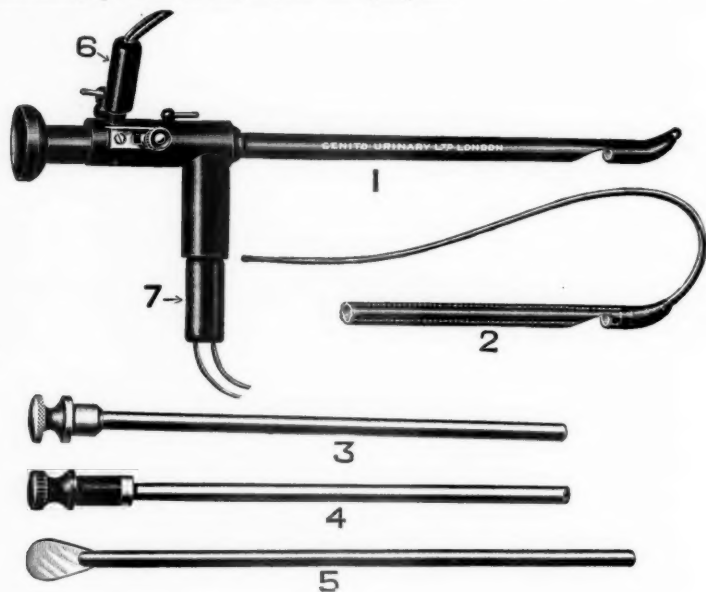


FIG. 1.

Fortunately, Mr. Schranz, of the Genito-Urinary Manufacturing Co., was able to overcome the technical difficulties of construction, and after many trials and modifications the instrument shown in fig. 1 was evolved.

In external form it resembles the Young punch, but the sheath is constructed of bakerlite—an excellent insulating material allied to vulcanite—with a metallic lining. This lining only comes to the surface at the edge of the window situated below the beak; elsewhere it is insulated from the urethra by the bakerlite covering (see fig. 2).

☛ The punch is introduced with an obturator in position, and, to facilitate its insertion in difficult cases, a pliable bougie guide may be attached to the beak by means of a screw. When in position the obturator is withdrawn, and a system consisting of a telescope, a lamp, and inlet and outlet tubes for irrigation is substituted, thus converting the instrument into a posterior urethroscope of the Gueringé type. Under guidance of the eye the punch is now withdrawn until the bar is seen to engage the window. Once it is in position the instrument is held firmly and pressure exerted on the bar by slightly raising the proximal end. Connexion is then made with the source of diathermy current. Immediately the current is turned on the

inner metallic layer of the sheath becomes a urethral electrode, but owing to the insulation conferred by the bakerlite covering, the only point of contact with the urethra is the edge of the window engaging the bar. From this edge a line of coagulation can be observed to spread within ten or fifteen seconds: this whitens the whole area of prostate engaged. By maintaining continuous irrigation the process of diathermy can be carried out under complete ocular control, the current being turned off when it is judged that a sufficient depth of tissue has been destroyed.

In order to remove the wedge of destroyed tissue and thus obtain immediate benefit without waiting for the separation of sloughs, the telescope and lighting system is withdrawn and an inner tube, resembling that used in the Young punch, is inserted. By pushing this home and giving it at the same time a slight rotatory movement, a core of destroyed tissue, about 2 cm. in length and 0.5 cm. in diameter, may be removed. This does not, however, constitute the whole amount of prostatic tissue destroyed by the operation, the full benefit of which will only be obtained at a later period when the sloughs have separated. The telescope and lighting system may then be re-introduced, the area of operation inspected, and a decision arrived at as to whether any further fulguration is required. If the operator is satisfied with the amount of tissue dealt with, the bladder is emptied, the obturator inserted, and the instrument withdrawn.

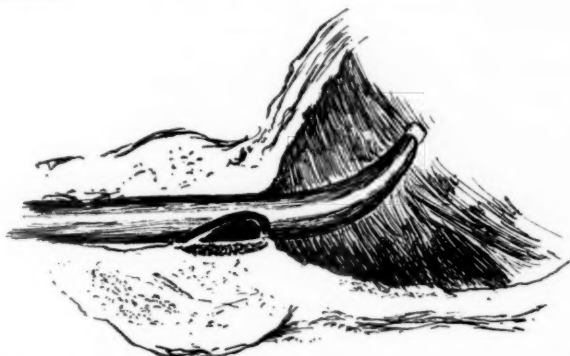


FIG. 2.—Punch in position. The black area shows the tissue removed directly by the punch, the dotted area that which subsequently comes away with the separation of the sloughs.

Very little after-treatment is necessary, and it is not my practice to tie in a catheter. A most remarkable point is the complete absence of pain or hæmorrhage, or indeed of any other complication, after this operation. What little bleeding there is at this early period is usually due to the trauma of instrumentation, and it ceases within a few hours. Most patients are able to get up and, if necessary, to leave the hospital or nursing home the day following the operation; their discomfort is not much greater than that commonly experienced after a cystoscopy. An alteration in the stream is not usually noted for four or six days, but as the sloughs begin to come away an improvement occurs. The separation of these sloughs may be encouraged by posterior irrigation according to Janet's method. In those cases in which more than one sitting is required, an interval of three to four weeks should be allowed between operations so as to allow time for the separation of the sloughs and the subsidence of the reaction.

Having described the instrument it will now be convenient to indicate the type of case in which it is of use. In the first place it must be understood that a punch operation is in no way a general substitute for prostatectomy. If it be employed indiscriminately the results obtained will be unsatisfactory and at the best temporary. The type of case suitable *par excellence* for a punch operation is that in which,

although all the signs of prostatic obstruction exist, little or no enlargement can be felt *per rectum*. These cases have been described variously as cases of prostatic bar, contracture of the bladder neck, "prostatisme sans prostate," and sclerosis of the bladder neck.

Randall, who has worked particularly on this subject, shows that there are four distinct types of prostatic bar, two of which are fibrous in nature and two glandular. In my experience the commonest of these is the fibrous bar that is found stretched across the posterior margin of the bladder neck and overhanging the vesical outlet. In this type of bar there is an increase in the distance between the posterior lip of the meatus and the perivesical tissues, and a reduction in that between the lip and the verumontanum, so that this latter structure appears drawn up towards the bladder. This condition is undoubtedly post-inflammatory, resulting from a previous prostatitis. It frequently occurs in men who have not yet reached the prostatic age, and is particularly amenable to treatment with the diathermy punch. Indeed I have had cases of this description in which a patient suffering from complete retention has passed urine a few hours after the operation and has left the hospital two days later without having suffered any material discomfort from this operation.

Although a punch operation is unsuitable for cases in which there is considerable enlargement, on account of the temporary nature of the relief afforded, I have nevertheless found it of use in those in which for some reason or other the performance of the major operation was impossible. In spite of the improvement in the technique of prostatectomy and the reduction in its mortality rate, it still remains a serious undertaking and there will always be some patients too feeble to be submitted to it. In such cases, should the obstruction be mainly in the so-called middle lobe, the diathermy punch may afford relief, and even should the operation have to be repeated subsequently on account of further enlargement it is such a mild proceeding that its repetition does not inflict any particular hardship.

For the sake of convenience, I will summarize the conclusions arrived at under the following headings:—

- (1) The diathermy punch has definite advantages over any similar instrument yet evolved. The operation is not a blind proceeding like Bottini's, but is done under full ocular control. It has an advantage over Young's operation in that it eliminates the risk of hæmorrhage, and it allows of the easy removal of greater quantities of prostatic tissue, the subsequent sloughing of the diathermy area widening still further the channel dug by the punch.

- (2) The type of case in which the diathermy punch can be most conveniently employed is that in which there is a "bar" obstructing the vesical outlet. It may also be used in cases of moderate enlargement in which prostatectomy is contra-indicated, and occasionally, in cases of post-operative obstruction and malignant disease.

Section of Urology.

President—Mr. CYRIL A. R. NITCH, M.S.

Foreign Body removed from the Bladder.

By C. A. R. NITCH, M.S. (President).

PATIENT, a female, aged 40, married.

History.—Hæmaturia and cystitis of two days' duration following insertion of a piece of slippery elm into vagina for amenorrhœa.

Cystoscopy.—Bladder not injured.

Treatment.—Removal by suprapubic cystotomy of a piece of slippery elm measuring 7 in. long by $\frac{3}{4}$ in. at its widest part. The foreign body was soft, swollen, and very slippery. When moist it apparently acts like a laminaria tent.

Large Renal Calculus.

By C. A. R. NITCH, M.S. (President).

PATIENT, a female, aged 47, single.

History.—Right loin ache for twenty-five years; intermittent hæmaturia first four years; nephroptosis. Wore belt. Small sarcoma of antrum removed twenty years ago. Severe hæmorrhages from ear since.

Symptoms.—Right loin ache.

Result of Examination (including "tests," &c.).—Large right kidney, hard; stones could be felt, crepitating on palpation. No efflux from right ureter. Urea concentration of left kidney, 3.8 per cent. in third hour. No cerebral symptoms.

Treatment.—Nephrectomy. Kidney one solid mass of stones and calcium salts, both oxalate and phosphate.

Pathology.—Death a week later from broncho-pneumonia. Left kidney healthy. Small sarcoma of petrous bone pressing on cerebrum and cerebellum.

Case of Congenital Abnormality of the Right Ureter.

By A. CLIFFORD MORSON, O.B.E., F.R.C.S.

PATIENT, a female, aged 18½.

History.—This case was submitted to me for examination by Mr. Comyns Berkeley on account of intermittent incontinence of urine. Since childhood incontinence of urine had occurred by day but not by night. There were no other urinary symptoms.

Cystoscopy.—Showed a healthy bladder. Right and left ureteric orifices normal in size and position. About $\frac{1}{2}$ in. from the internal meatus on the right side there was another opening which admitted a ureteric bougie. The latter was observed to pass the normal right ureteric orifice, thus demonstrating that the two orifices were in communication. An efflux was seen from both the normal right ureteric orifice and the abnormal one, but from the latter opening the escape of urine seemed slight.

From the cystoscopic examination it was clear that the right ureter had a normal opening at the inter-ureteric bar, but then was continued on to the region of the internal meatus. The daily incontinence of urine can be explained by the

stimulation of the urethral sphincter to relax when urine escaped from the abnormal ureteric opening while the patient was in a standing position and impinging against the internal meatus.

It has been decided that should the incontinence of urine persist the patient will submit to destruction of the portion of the ureter between the two openings by means of diathermy.

Cases have been described in which the ureter has opened into the urethra, thus causing incontinence of urine. The case now recorded, though rare, is due to a similar embryological defect.

Two Pyelograms.

Shown by RALPH THOMPSON, M.Ch.

I.

Mr. RALPH THOMPSON showed a pyelogram of the left kidney, which indicated a blurred outline of the renal pelvis, and the kidney was displaced downwards. Patient was a married woman, aged 31.

There was a tender lump in the left lumbar region of the abdomen, and it was thought that the left kidney was at fault. Mild pyuria.

When an operation was done on the left kidney, it was found that there was some evidence of the kidney being freely movable, and there was also evidence of chronic capsulitis of the organ. But the swelling was found to be due entirely to an aberrant spleen, which extended from the lower pole of the left kidney to the brim of the true pelvis. The peritoneum was seen to become wrinkled over its surface, and blood-vessels to be entering the surface of the organ in numerous places.

A blood-count revealed nothing abnormal.

The kidney was fixed in position, the patient made a good recovery, and has had no pain since the operation. The urine is clear.

II.

The exhibitor showed a pyelogram of the right kidney of a male aged about 25. It indicated aberrant blood-vessels. Mr. Thompson said he was by no means satisfied with the after-history of patients who were operated upon and the aberrant vessels divided after ligature. He was aware that the cases appeared to do well for about a year from the operation, but they sometimes had recurrence of trouble after this period. He had seen atrophy of the kidney occur after division of the aberrant vessels, and was disposed to think that an operation designed to close the blood-vessels was preferable to one in which they were divided.

Papilloma of Ureter.

Shown by F. MCG. LOUGHNANE, F.R.C.S.

J. W., AGED 70, admitted to All Saints' Hospital on November 12, 1923. Five years ago patient had an attack of hæmaturia which lasted for one week. He had another attack four years later, and again at the beginning of 1925. Since then he has had several attacks. He has had no other urinary symptoms.

On examination.—Patient seen to be very obese, hence palpation in kidney region of no account. Prostate slightly enlarged, but caused no obstruction.

Cystoscopy.—Left ureteric orifice normal; thick stream of tarry blood seen to issue from right orifice. Indigo-carmin injected intravenously appeared at the left orifice in four minutes, but none seen at right orifice even after twelve minutes. A pyelogram taken was a failure, the opaque fluid not having ascended above the crest of the ilium. X-rays showed nothing abnormal. Examination of urine only revealed presence of red blood-cells.

I made a diagnosis of renal neoplasm, and proceeded to operate. On eventrating the kidney I realized it was small, and could feel no tumour or stone. Therefore, doubting the diagnosis, I opened the ureter and passed a catheter down into the bladder in the event of there being a stone in the ureter not shown by the X-rays. I then removed the kidney, which was somewhat atrophied, the pelvis being slightly dilated. The wound healed well and the patient was discharged, though he still suffered from slight hæmaturia.

Two months later he was again admitted for hæmaturia, cystoscopy showing thick, tarry blood issuing from the right ureteric orifice. The ureter was exposed and completely removed. A fusiform swelling, 1 by $\frac{1}{2}$ in., was found in the upper part, about two inches below the ligature. No second growths were present, and under the microscope the growth is seen to be a simple papilloma.

Microscopic Report.—Section shows masses of spindle-shaped and ovoid epithelial cells, the arrangement of which suggests a papilloma.

X-ray Films illustrating Mobility of the Kidney.

Shown by F. MCG. LOUGHNANE, F.R.C.S.

AN X-ray of the kidney after perirenal injection of oxygen. The kidney is seen to be rotated round its antero-posterior axis, the lower pole being swung across the spine. The case was one of left-sided mobile kidney, with pain in the groin.

Massive Calculus Formation in an Ectopic Kidney.

By H. P. WINSBURY WHITE, F.R.C.S.

I REMOVED this specimen from a male patient, aged 43, who had complained of a pain in the left lumbar region over a period of ten years or more. It was on account of this persisting pain that he had been discharged from the Navy as physically unfit, in 1915.

On presenting himself to me the urine contained a considerable quantity of pus. A radiographic examination showed both renal regions to be free from stone, but in the hollow of the sacrum there was seen to be an irregular opacity occupying the whole of this area.

Cystoscopy excluded vesical calculus, and showed an inflamed and inactive left ureteric orifice. The right ureteric orifice was normal in appearance. The intravenous injection of indigo-carmin carried out during the cystoscopy indicated that the right kidney was functioning normally and that the left kidney was diseased. A ureteric catheter was passed up the right ureter for a distance representing the normal length of this structure, whereas the left ureter admitted the catheter for some centimetres only.

It was concluded from these observations that the right kidney was healthy and lying in its usual position, and that the left was situated below the pelvic brim and contained a mass of stones. These facts were verified by operation when the abdomen was opened in the mid-line below the umbilicus, and the diseased kidney removed from the presacral retroperitoneal region. The patient made a good recovery.

Some difficulty in removing the kidney arose from the presence of a considerable amount of fibro-fatty adhesions which surrounded it and obscured the course of the renal vessels, which were scattered and largely aberrant.

As the radiogram indicated, all the calyces and the whole pelvis were found to be completely occupied by the processes of a large branching calculus. There was but little secreting renal substance left. In addition to the ectopia the kidney was the seat of another congenital abnormality, the pelvis being of the trifid variety.

An interesting clinical point was the fact that the patient never referred his pain to the actual position of the diseased kidney but always to the normal situation of the left kidney.

Hydronephrosis due to a Squamous-celled Carcinoma blocking the Ureteropelvic Junction.

Shown by S. G. MACDONALD, F.R.C.S.

THE specimen was removed from a patient, aged 35 (referred by Dr. G. A. Hodgson), in June, 1924.

When 14 years of age he received an injury to the right kidney followed by hæmaturia for some days, for which he was admitted to St. Thomas's Hospital.

During the war he served in India and began to suffer from backache. In November, 1923, and again in June, 1924, he had attacks of hæmaturia lasting two days on each occasion.

Examination revealed the presence of a large hydronephrosis on the right side, producing compression of the base of right lung, and pushing the apex beat to $\frac{1}{2}$ in. outside the nipple line. A squamous-celled carcinoma blocked the communication of the pelvis with the ureter.

Cystic Lower End of the Right Ureter.

Shown by S. G. MACDONALD, F.R.C.S.

THE specimen was removed from a female patient, aged 39 (referred to the exhibitor by Dr. Tonking). No symptoms present until the cyst was extended through the urethra. The ureteric opening is a pin point and is demonstrated by the bristle through it. The cyst is apparently due to a congenital stricture of the orifice.

Pyelograms of Double Ureters.

By J. SWIFT JOLY, F.R.C.S.

Case I.—Male, aged 25. *History.*—Litholapaxy four years ago.

Persistent dull pain in the right loin. X-ray examination negative. Cystoscopy: Bladder normal. Left ureter normal in appearance and position. Right side, two ureteric orifices, about $\frac{1}{2}$ in. apart; both normal in appearance; clear urine coming down both. Pyelography: No. 6 catheters passed up both the ureters on the right side; 2 c.c. of 30 per cent. sodium iodide injected up the catheter in the lower orifice, and 5 c.c. up that in the upper. The pyelogram showed that the lower ureter, as viewed from the bladder, drained a single calyx in the upper pole of the kidney, while the upper drained a renal pelvis, communicating with the middle and lower calyces. No evidence of hydronephrosis, or of displacement of the kidney. The ureters crossed each other twice in their course, once in the true pelvis, and again just below the crest of the ilium.

Case II.—Male, aged 44, admitted to St. Peter's Hospital complaining of pain in the right side. X-ray examination showed a round shadow about the size of a shilling, midway between the tip of the last rib and the crest of the ilium. Neither kidney palpable. Cystoscopy: Normal bladder and ureters. Pyelography: 20 c.c. of sodium iodide solution injected. The ureter bifurcated at the level of the lower pole of the kidney. The upper branch drained a single calyx at the upper pole, while the lower could be traced into a large hydronephrotic sac involving the lower two-thirds of the kidney, in which the stone lay. There was a definite stricture at the junction of the ureter with the lower pelvis. Nephrectomy: The hydronephrosis was of the pelvic type, and the lower portion of the kidney was a mere shell.

Section of Urology.

President—Mr. CYRIL A. R. NITCH, M.S.

DISCUSSION ON BACTERIAL INFECTIONS OF THE URINARY TRACT.

Professor LEONARD S. DUDGEON

confined his remarks to the types of infection met with, which in his opinion were important. By the request of the Council of the Section of Urology, no reference was made to venereal or tuberculous infections.

(1) *Infection due to a special group of hæmolytic bacilli.*—He had seen up to the present time over two hundred of these cases, and in the great majority of instances this infection ran a very acute course; it was not infrequently mistaken for paratyphoid fever. This erroneous diagnosis might be explained to some extent, as the general clinical phenomena were often far in excess of the local symptoms due to inflammation of the urinary passages, at the onset of the illness. The course of the acute infection generally lasted from two to three weeks, in the majority of cases the patients made a complete recovery and the urine was free from the specific organism. Two fatal cases had occurred, and in one instance the infection became chronic.

The great differences between these infections and those due to the *Bacillus coli* were: (1) That they ran a more acute course as a rule, and were of longer duration; (2) that complete recovery usually occurred and the urine became free from the infective organism; and (3) that there was hypersensitiveness to specific vaccines.

This group of bacteria had a superficial resemblance to paratyphoid bacilli, as they were slow lactose fermenters, formed blue colonies on litmus lactose agar, and cane sugar was unaffected. But indol formation was rapidly produced and active hæmolysis occurred. A much smaller group of non-hæmolytic slow lactose fermenters was now under investigation but here we had indol formation and the serological reactions to decide the issue. In a small number of cases the same bacillus (hæmolytic type) as had produced the acute infection of the urinary passages was found in the fæces. In spite of the very high temperature which might occur during the course of this disease, the bacillus had not been isolated from the blood-stream.

Patients suffering from this infection were found to be hypersensitive to vaccines if administered during the acute stages of the disease, or even until the temperature had become normal for two or three days. A complete return of all the clinical manifestations had occurred from small doses of specific vaccines administered during this period. Vaccines should not be administered until the temperature had been normal for several days, and in his experience in the majority of instances vaccines were not required.

(2) *Infections due to Bacillus coli.*—The work of Dudgeon, Bawtree and Wordley showed that colon bacilli infecting the urinary tract could be grouped into hæmolytic and non-hæmolytic strains. The hæmolytic group accounted for about 70 per cent. of cases in the male sex and about 30 per cent. in the female, although the percentage of hæmolytic organisms was higher in women suffering from acute coli fever. With the non-hæmolytic group the percentages among men and women were reversed. Rabbits were readily immunized by the hæmolytic group, but often with great difficulty by the non-hæmolytic bacilli.

It had long been suggested that *Bacillus coli* infections of the urinary tract were due to the presence of these bacilli in the fæces, but the proof, however, of faecal infection was indefinite or absent. Many cases of *Bacillus coli* infection of the urinary tract had, however, been shown to be due to the same bacillus isolated from the urine and from the fæces. Herrold, in America, had confirmed these results of Dudgeon, Bawtree and Wordley; Bawtree in this country had also confirmed them, and, lately, similar results had been obtained on the Continent. Numerous observations had been made on the presence of coli agglutinins in the blood-serum of normal individuals and of those who had been, or were, subject to *Bacillus coli* infections. The antigens had been prepared from cultures of *Bacillus coli* isolated from urinary and faecal infections. Experience with these reactions suggested that a positive reaction of 1 in 100 indicated a *Bacillus coli* infection.

44 Dudgeon—Lepper: *Bacterial Infections of the Urinary Tract*

From the results of the researches of the above-mentioned workers it was considered that an autogenous vaccine was always necessary for the treatment of these infections, because, as already mentioned, the vaccine must be made from the infecting organism or otherwise no protection might be offered, and the same remarks applied to preventive inoculation. It was well to realize that it was difficult to immunize man and sometimes animals with dead cultures of *Bacillus coli* given subcutaneously. If during vaccine treatment the patient's symptoms were aggravated from the autogenous vaccine, even when given in small doses, it was advisable to ascertain whether the infective process was held up at some point along the urinary tract, more especially in cases of unilateral pyuria. In his (Prof. Dudgeon's) experience rabbits showed little effect from intravenous inoculation of colon bacilli even in enormous doses, but if the organism was localized in the tissues, or held up, then abscess formation or acute inflammation might occur. This was well shown when these bacteria were injected into the pleural cavity in rabbits.

For these reasons he advised that vaccine treatment should be withheld if it was found that the patient's temperature or symptoms were aggravated when the injections had been carried out with average doses. In his experience second or subsequent attacks of coli fever were due, with very few exceptions, to the same organism as incited the primary fever, in spite of the fact that the urine might have cleared. It was necessary to examine a few c.c. of urine before it was regarded as free from bacteria.

(3) *Bacillus proteus*.—Although from inexperience this organism had been regarded as non-pathogenic to man, it is well to realize that it may give rise to very acute infections, and the bacillus can then be recovered from the blood-stream. The temperature in some proteus infections reaches a very high level. The urine in proteus infections was alkaline, contains mucus and pus and often masses of triple phosphate with the mucus. Vaccine treatment has often been found to be of definite value when the acute stage is over.

(4) *Staphylococcus albus*.—The presence of this coccus in large numbers in the urine, together with pus, was in his experience invariably associated with the presence of a calculus.

THE EXPERIMENTAL PRODUCTION OF INFECTIONS OF THE URINARY TRACT BY THE COLON BACILLUS.

Dr. ELIZABETH LEPPER.

It is quite unnecessary for me to attempt to give this Section any complete account of the numerous investigations which have been made on the production of infections of the urinary tract. The classical work which has been done on this subject is well known to you. I will, therefore, deal only with a certain branch of this large subject of which I have had practical experience,—the production of infections of the kidney by means of the colon bacillus. It seemed to me that it might be of some interest to those of you who have not facilities for experimental work, to see lesions which had been produced by certain definite modes of attack.

When I looked through the literature before starting work on this subject, I came to the conclusion that in spite of apparently contradictory statements, the following two facts had been established: (1) That it is comparatively easy to produce lesions in a kidney by injections of organisms either into the blood-stream or into the ureter, provided that some obstruction of that ureter is produced at the same time; (2) that inflammatory changes of the substance of a kidney or its pelvis can be produced by the intravenous injection of bacilli alone, without the presence of obstruction to the outflow of urine. The occurrence of such lesions is, however, very uncertain, as is also the degree of severity and their localization. I could find no explanation as to why these two experimental methods should differ so much in their results. It seemed to me that if I could modify the technique in such a way as to set up only a temporary obstruction of the ureter and so produce only slight lesions in the kidney I might be able to find out why this factor of obstruction to the outflow of urine was so important. To this end I carried out some experiments in the following way.

A rabbit was anesthetized, and the left ureter exposed through a lumbar incision. It was freed from the surrounding tissues and an aneurysm needle passed under it by means of which it could be lifted up into the wound. A piece of elastic band $\frac{1}{8}$ in. wide was passed under the ureter and made to compress the lumen by being fixed tightly round the ureter by means of a pair of bull-dog forceps placed parallel with it. The wound was then covered with damp gauze.

An intravenous injection of bacilli was then given into an ear vein. The compression of

the ureter was continued for varying lengths of time. Then the ureter was carefully replaced and the wound closed. The animal was killed five days later.

The results were not what I had anticipated. Even such a short period of obstruction as thirty minutes was sufficient to produce very profound changes in the kidney (specimen shown). The kidney was greatly enlarged and was studded with multiple small abscesses. The shortest period of compression, fifteen minutes, resulted in a well marked cortical abscess. Several of these experiments were carried out and they resulted either in very well-marked, diffuse lesions, or none at all. I killed one animal at the end of the operation when the ureter had been compressed for one hour, hoping to be able to detect changes microscopically in the kidney, but I was disappointed. The explanation of these results was obtained from an animal which accidentally died during the operation. The injection had been given and compression of the ureter was in progress for fifteen minutes when the rabbit suddenly died. I clamped the renal vessels and tied the ureter below the obstruction and put the kidney in formalin. On cutting into the specimen some days later I was surprised to find considerable dilatation of the pelvis, which contained blood-stained fluid. This dilatation was so marked that the renal vessels could be seen running across the lateral walls as raised strands. The section of this kidney shows fairly well on the screen. You can see the vessels, on the way to the substance of the kidney, almost surrounded by the dilated pelvis. Microscopically, one can see that the thin-walled vein is much more compressed than the artery and that there is very intense congestion of the cortex.

This section affords a reasonable explanation as to the rôle played by obstruction of the ureter in the production of these lesions. If secretion of urine is taking place the pelvis becomes dilated and obstruction to the venous return is set up; this may result in the rupture of some of the vessels and in the extravasation of blood containing bacilli into the substance of the kidney; and diffuse acute inflammatory foci are produced in the kidney. When no secretion of urine is taking place, which sometimes happens in the case of animals under an anæsthetic, no dilatation of the pelvis occurs, there is no venous congestion or rupture of small vessels, and the result of the experiment is negative.

How far we can apply results obtained with rabbits' kidneys to the study of disease in man is problematical, but the experiments warn us against the production of any sort of obstruction to outflow of urine from the ureters.

Intravenous injections.—With regard to the experiments in which intravenous injections of bacteria were given, I obtained practically the same results as other observers, that is to say, a low percentage of positive results, the lesions generally slight, and confined to the papilla, but occasionally severe, numerous small cortical abscesses being present. Of fifty rabbits used, some of which received repeated injections, the results in twenty-eight were quite negative. Of the twenty-two which showed lesions only eight would have been detected by inspection alone. These showed cortical abscesses or congestion of the pelvis with a slight purulent exudate. It was the condition of the urine during life that made one investigate the slight cases microscopically, and sometimes serial sections had to be made in order to detect the lesion.

The most severe lesion of the papilla which I obtained shows fairly well on the screen. You will see that there is necrosis of the tip of the papilla, an exudation of leucocytes and an enormous number of bacilli, which are stained purple by thionin blue. This animal had been injected four days previously. These lesions are brought about by bacterial embolism of the vessels supplying the papilla. A kidney obtained twenty-four hours after the intravenous injection shows this well. A small vessel supplying the papilla can be seen full of organisms; it is cut longitudinally at the base of the papilla and transversely at the tip and is almost entirely blocked by the bacilli, though a few red corpuscles can be detected. Following the embolism an exudation of leucocytes occurs round the affected vessels, and small interstitial abscesses are produced. These may rupture into the straight tubules or upon the surface of the papilla, and lead to the appearance of pus and bacilli in the urine. The slides under the microscope show the various stages I have described.

The cortical lesions, which are much less frequent, are produced in the same way. I have not been able to find out why, in coliform infections, the vessels supplying the papilla are more liable to embolism than those supplying the cortex. I understand from Professor Starling that when injecting the arteries of kidneys he sometimes has a difficulty in getting good preparations showing the vessels of the cortex, so that there may be some anatomical explanation to account for the difference.

To sum up, therefore, these specimens show that, in the experimental production of lesions

in the kidney, obstruction of the ureter is important in bringing about a dilatation of the pelvis and pressure on the veins, which may result in extravasation of infected blood into the substance of the kidneys, and subsequently diffuse inflammatory changes throughout the kidney. Intravenous injection alone, however, acts by producing embolism of small arteries, especially those supplying the papilla; the lesions are localized, and much less severe in character than those produced by the extravasation of infected blood into the tissues, such as occurs after obstruction of the ureter.

(This communication was illustrated by microscopic preparations shown on the epidiascope.)

SYMPTOMS AND TREATMENT OF URINARY INFECTIONS.

Sir JOHN THOMSON-WALKER.

There are two classes of infection of the urinary tract.

(1) Infection in a urinary tract already the seat of disease, such as stricture, enlarged prostate, growths of the bladder, or stone.

(2) Infection in a previously healthy urinary tract.

For the purpose of this discussion I will deal only with infection occurring in a previously healthy urinary tract, for here the symptoms are entirely due to the infection and are not complicated by those which arise from the previously existing disease.

The cases may be broadly divided into (i) acute and (ii) chronic.

(I) ACUTE URINARY INFECTION.

I shall take the common type of case first, and then refer to unusual types.

Prodromal Symptoms.—There is usually a period of four or five days or a week before the acute symptoms develop, during which the patient feels ill, but these symptoms are not sufficiently severe for him to seek medical advice.

At this prodromal stage there is some loss of appetite, a little headache, want of energy, irritability, and the patient is hypersensitive to changes in the surrounding temperature. Almost always there is some bowel trouble. If the patient is inclined to constipation the bowels are more difficult to move; flatulent distension, in patients subject to it, is more pronounced and distressing; patients prone to irritability of the bowel have an attack of diarrhoea. There may also be a little irritability of the bladder and increased frequency of micturition although the urine still remains clear.

Symptoms.—The acute symptoms often come on suddenly:

(1) The patient has a sudden desire to pass water, sometimes so urgent that there is an involuntary escape. This is followed by intense bladder irritation and strangury.

(2) After a few hours of bladder irritation there is a rigor and the temperature runs up to 103° or 104° F. The rigor may be repeated.

(3) The urine, which is passed in small quantities at frequent intervals, is high coloured and cloudy and often it has an offensive, fishy smell. It may contain some blood and the patient may have noticed that a few drops of blood were expressed at the end of micturition. Evidence of bacilluria, in the shimmering appearance on swinging the glass so as to circulate the urine, may be present. Subsequent bacteriological examination of the urine shows an abundant growth of bacteria, most commonly *Bacillus coli*.

(4) Flatulent distension of the bowel is a frequent and distressing symptom, and during the course of the attack this is accompanied by obstinate constipation.

(5) Some symptoms may now develop which show where the main stress of the infection is likely to be focussed.

There is pain in the loin on one side, situated in the kidney region both in front and behind. It may be vague at first and felt in the abdomen or the back, but it becomes localized as a dull, heavy, aching pain in one loin. In addition there may be an attack of severe pain along the line of the ureter, amounting to renal colic. The pain here is a localizing symptom of extreme value and shows that there is pyelitis and descending ureteritis.

(6) On palpation of the abdomen there is tenderness of one loin. Tenderness may be present even when pain is absent. There is also some rigidity of the abdominal muscles on the affected side.

(7) The kidney is not enlarged in the early stage of the illness and may not be enlarged at any time. If there is no enlargement the diagnosis of pyelitis is made. Enlargement of the kidney indicates pyelonephritis or pyonephrosis.

In the earlier stage of enlargement of the kidney the enlargement is due to inflammation

of the kidney substance. Later, obstruction of the pelvic outlet may be superadded to this, and a pyonephrosis may develop. The time at which a pyelonephritis became a pyonephrosis is not easy to diagnose, but in the pyonephrosis the temperature, instead of running continually high, takes on a swinging character, the pain and tenderness are greater and the enlargement and rigidity are likely to be more pronounced. The urine may become clear, and this is an important diagnostic sign. Later, the escape of a quantity of pus, with relief of pain and subsiding symptoms, is characteristic of pyonephrosis. But the urine may have been clear throughout, and this, together with a swelling in the loin and symptoms of infection, points to an infection of the renal cortex, with perirenal inflammation and probably abscess.

(8) Instead of symptoms relating to renal infection a group of symptoms may develop which indicate prostatitis or seminal vesiculitis. There have been bladder irritability, high temperature and changes in the urine; now difficulty of micturition is superadded, and retention of urine may suddenly occur, necessitating the passage of the catheter for its relief.

Short of complete obstruction, there is difficult, frequent, painful micturition with a heavy aching pain in the rectum and perineum, a feeling of fullness in the rectum and pain on defecation. Aching in the testicles is usually present.

Rectal examination shows that the prostate is enlarged and very tender, and that the seminal vesicles are distended.

(9) There may be present neither renal pain or enlargement, nor symptoms of prostatic or vesicular inflammation.

Course.—The course of an attack of urinary infection with the *Bacillus coli* is very variable. Slight attacks of pyelitis or prostatitis or cystitis, lasting from ten to fourteen or twenty-one days, are common. It is no infrequent experience to see a patient in consultation in whom the acute symptoms already described have been present and some frequency of micturition still exists, and yet on examining the urine it is found to be sterile.

These severe infections may last for many weeks, with the temperature remaining high and the symptoms continuing. Recurrent exacerbations in the course of a severe renal infection are not uncommon. Relapse in urinary infection is very frequent and is especially common in infections with the *Bacillus coli*. It occurs usually when the patient gets up or begins to move about the house.

I will now describe the course of the more obscure clinical cases in which the symptoms do not present themselves so clearly as I have indicated.

(1) *Cases with high temperature as the chief symptom.*—There are puzzling cases of sudden high temperature usually of short duration without other symptoms. Occasionally the high temperature is recurrent and appears after exertion or exposure.

Urinary symptoms are either completely absent or so slight as to be overlooked. An examination of the urine may show an unsuspected bacilluria and reveal the cause of these attacks. The condition is specially puzzling in children and infants, for with the high temperature, nervous symptoms such as squinting, vomiting and convulsions may be present.

(2) *Cases with pain as the most prominent symptom.*—The aching pain of pyelitis may be mistaken for appendicitis, but it ought not to be so mistaken, for the pain is over the kidney, there is tenderness of the affected kidney, and bacilli are present in the urine.

Appendicitis is, of course, a likely disease to be present in the same cases as urinary infection, but I think that care in diagnosis would prevent the removal of a normal appendix in a fair number of cases of right-sided pyelitis.

Renal colic may lead to the diagnosis of stone in the renal pelvis or ureter, for the pain of acute pyelitis and ureteritis may be as acute as that of a descending calculus. The presence of fever and the infection of the urine is sufficient for the diagnosis of descending ureteritis.

A stone may be present when an infection of the urinary tract occurs, but it is unlikely that the first symptoms of both diseases will coincide, and the previous history of infection or of stone may help in the diagnosis. In case of doubt an X-ray examination will be made when the acute symptoms have subsided.

(3) *Hæmaturia.*—It comes as a surprise to find a large quantity of blood being passed in the urine in the early stage of a urinary infection. This may, in my experience, precede by some days any bladder or kidney symptom and may thus, in itself, be a very puzzling symptom.

Bacteria are found in great quantity in the urine if looked for, but their presence may not be suspected. I have known a very copious hæmaturia begin suddenly and continue for seven days without any other symptoms, and then be followed by very moderate symptoms of cystitis. In this case there was a mixed infection located primarily in the left renal pelvis.

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(4) *Cases of renal infection with sterile urine.*—There is a group of cases, fortunately not very large, in which the cortex of the kidney is infected by way of the blood-stream and the urine remains clear and sterile. The infection is wholly confined to the secreting portion of the kidney and does not invade the conducting portion. In such cases the infection is almost always due to the staphylococcus.

Suppurative nephritis results from the cortical infection and produces either wedge-shaped infarcts or small abscesses in the outer part of the cortex, which spread outwards and cause a perinephritic abscess. These cases frequently present a puzzling clinical picture. Renal pain is slight or absent in the early stages of the attack, and the urine is sterile. There is a high, continuous or swinging temperature. Later, there is some thickening and tenderness deep in the loin and a little rigidity of the muscles on the affected side. In one case that I saw recently, a swelling that appeared to be the greatly enlarged kidney developed in the loin and consisted of perinephric inflammation with a small pocket of pus. In another case nothing more than deep tenderness and thickening could be detected, but there was limitation of extension of the ilio-psoas muscle. In both these cases there was a very definite antecedent history of staphylococcal infection of the skin, although in one an interval of a year had elapsed since this was cured.

(II) CHRONIC URINARY INFECTION.

I shall refer to three common varieties. (1) Recurrent pyelitis and cystitis. (2) Chronic cystitis with quiescent pyelitis. (3) Bacilluria with toxæmia.

(1) *Recurrent pyelitis and cystitis.*—The initial attack of pyelitis or pyelocystitis has settled down and the patient may consider himself quite well and return to his business. After an interval of weeks or months, or even a year or more, he gets a second attack and this is succeeded by others. There are two types of case included under this category.

(a) In one there is a persistent infection of the renal pelvis or bladder, which flares up into an acute attack. The attack is excited by cold and damp, anxiety, unsuitable food, alcohol, periods of constipation or attacks of diarrhoea, tonsillitis and other causes. Between the attacks there may be little, excepting a slight irritability of the bladder, to draw attention to the urinary tract—but in these cases many signs of chronic toxæmia may exist.

(b) The second type of recurrent pyelocystitis is that in which the patient's urine is sterile between the attacks. In the male subject some of these cases are due to an infected seminal vesicle which from time to time re-infects the prostatic urethra and bladder, but the condition is not infrequent in the female subject and is due in most cases to re-infection of the urinary tract from some chronic condition of the bowel such as chronic stasis, chronic appendicitis, colitis, dysentery, piles, cholecystitis. Before each fresh attack of pyelitis or pyelocystitis there is an exacerbation of the bowel condition.

(2) *Chronic cystitis with quiescent pyelitis.*—Chronic cystitis, persisting in spite of long continued bladder washing and courses of urinary antiseptics and alkalies and of vaccine, is one of the most troublesome conditions that the practitioner has to treat.

There may be some local condition in the bladder, such as a diverticulum or growth, which will explain the persistence of the infection; or the prostate and seminal vesicles may be the seat of a chronic infection which constantly supplies bacteria to keep up the stock in the bladder. But these being excluded, the cause of the persistent cystitis is most frequently a chronic infection of the pelvis of the kidney.

This form of pyelitis may be termed "quiescent pyelitis," for it gives rise to no symptoms and is only discovered by the examination of a specimen of the urine of each kidney obtained by the urethral catheter. It is a mistake, however, to suppose that all cases of chronic urinary infection in women or in the male, where the prostates and vesicles are normal, are due to pyelitis. There are many cases in which the infection is confined to the bladder.

In 100 cases of urinary infection with the *Bacillus coli* in which the urine from each kidney was examined, the following results were obtained: in forty-two there was bladder infection, but both kidney urines were sterile. Among fifty-eight cases, thirty had one kidney, twenty-eight had both kidneys infected.

(3) *Bacilluria without local symptoms, but with toxæmia.*—Bacilluria is an infection of the urine, usually with the *Bacillus coli*, but sometimes with other bacteria, when there is either no inflammation or a minimal amount of inflammation of the urinary tract. The urine is cloudy from a bacillary emulsion and on swinging the specimen glass to circulate the urine a peculiar and characteristic shimmering is seen. On standing, the fluid remains cloudy and there is no deposit. The urine has a peculiar stale fish odour.

The cystoscope shows a normal bladder or, at most, a few areas of congestion. The ureteric catheter may show infection of the urine of one or both renal pelves, or the specimen from the kidneys may be sterile and the bladder urine infected. This is bacilluria in its pure state. In such a case it is possible, by the irritation caused by large doses of hexamine, to produce a reaction in the mucous membrane of the urinary tract which results in mucus and pus appearing in the urine, and the same may occur spontaneously after dietetic indiscretion, bowel disorders and other causes.

Bacilluria may be present with any of the other types of urinary infection. While there may be no local reaction with bacilluria, symptoms of absorption of toxin may be present. The toxæmia may be acute or chronic. Acute toxæmia in the form of obscure attacks of pyrexia has already been described. Chronic toxæmia may result from any chronic infection of the urinary tract, but it is more common in cases of bacilluria, and I have therefore placed it under this heading. It is sometimes doubtful whether the toxin is absorbed from the bowel, which is the original source of the infection, or from the urinary tract. Not infrequently the bowel disorder has long since disappeared, and only the bacilluria remains as the source of the toxæmia.

These patients show all the symptoms of chronic toxæmia. The complexion is sallow and muddy—the patient thin and never well. A lack of energy is shown, headaches and aching of muscles and joints are present. The skin is usually dry and scaly, but sometimes the patient complains of night sweats.

Fibrositis is a common complication; lumbago, stiffness of the neck muscles and fibrositis of the sacro-iliac ligaments may also occur. Stiffness of the joints is common, and in the worst cases there is extensive osteo-arthritis and partial or complete crippling of the patient.

In the slighter degrees of toxæmia the patients are often looked upon as neurasthenics.

PROPHYLAXIS.

In the great majority of infections occurring in a previously healthy urinary tract, apart from instrumental interference, the infecting bacterium is the *Bacillus coli* and the source is the bowel.

The *Bacillus coli* is indigenous in the bowel, and there are certain barriers which prevent its escape into the circulation and the subsequent implantation in the urinary tract. The intact epithelium of the intestine with its covering of mucus is the first of these. The lymphatic nodes of the intestine, the solitary glands, Peyer's patches and the appendix are the next, and the lymphatic glands along the ileocolic vessels constitute the final barrier.

Certain forms of treatment injudiciously carried out undoubtedly break down these barriers and permit the escape of the *Bacillus coli* from the intestine. I have seen a bacilluria appear after a course of drastic purging, and a bacilluria has developed in patients under my observation who have been subjected to a severe treatment by the Plombières method. I have witnessed the sudden disappearance of a bacilluria after a bismuth enema given for the purpose of radiography, only to reappear after a few days.

Mottram and others have shown that prolonged radiation of the abdomen, either by X-rays or radium, so damages the epithelium of the intestine that bacteria escape into the circulation.

The change in all these cases appears to be a reduction of, or disappearance of, the mucus which covers the interior of the intestine as a protective layer, and also damage to the epithelium itself. The effect of the bismuth enema was probably to form a protective coating on the surface of the mucous membrane. These observations should, I think, act as a warning to physicians against drastic purgation, over-distension by the Plombières treatment, and prolonged radiation without adequate intervals.

The indiscriminate removal of the normal appendix appears to me to be a factor in the production of urinary infections with the *Bacillus coli*. Operations on the bowel and on the pelvic organs are a frequent cause of bacilluria. The mere handling of the bowel may be the cause of a sharp attack of urinary infection.

It is now a number of years since I advocated the prophylactic use of urinary antiseptics before and after operations on the bowel, rectum and female pelvic organs. So far as I know this use of urinary antiseptics is still entirely neglected. It is not an uncommon experience to see a case of excision of the rectum which has been brilliantly successful, so far as the removal of the malignant growth is concerned, marred by the presence of chronic cystitis due to infection by the *Bacillus coli*. It appears to me that a urine fully charged with antiseptics at the time and immediately after the operation will probably turn the scale against a

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transitory bacilluria becoming a permanent infection of the urinary tract. Urinary antiseptics should also be administered before parturition, when the catheter will have to be passed, it may be under difficult conditions in regard to asepsis.

Similar precautions are taken in all operations on the urinary tract.

TREATMENT.

(1) *Symptomatic.*—In the treatment of acute urinary infections the symptoms which first call for treatment are pain, constipation, abdominal distension, and frequent micturition.

The question as to whether it is safe to give morphia will probably arise. The quantity of urine may be much reduced in the early stage but unless the kidney is felt to be enlarged (hyponephritis, pyonephrosis) no anxiety need be felt in regard to the renal function. And usually with an enlarged kidney on one side the function of the remaining kidney is adequate, and morphia may be administered. This should, however, be given sparingly, for the distension of the bowel is often very troublesome and will be rendered more so if morphia is freely administered. Another point is that saline purgatives are better avoided on account of the flatulent distension of the bowel they produce.

(2) *Of the urinary tract.*—This consists in the administration of diuretics, alkalies and urinary antiseptics.

It should be the rule that no instrumental interference such as catheterization, sounding, cystoscopy or catheterization of the ureters be permitted when the disease is in the acute stage. Only harm can result from such action at this stage. One exception may be made, namely, in the case of retention of urine whether in the bladder or in the renal pelvis. Retention of urine in the bladder, either partial or complete, may result from acute prostatitis or vesiculitis or even in cystitis alone. Another exception is in some cases of pyelitis of pregnancy in which the passage of a ureteral catheter and gentle lavage of the renal pelvis are followed by a drop of temperature and improvement in the symptoms.

(a) *Diuretics.*—These form an important part of the treatment in all cases. The diuretic waters (Contrexéville, Vittel, Vichy) are useful. Barley water, distilled water or weak tea may be preferred. Freshly made infusions from the dried leaves of buchu or *Uva ursi* are powerful diuretics. It should be pointed out, however, that there is a danger that the flatulent distension of the bowel may be increased if too much fluid is given.

Together with the diuretic treatment the alkaline or the antiseptic treatment may be employed.

The urine should be tested with the litmus, and if, as it will be in the majority of uncomplicated urinary infections, the reaction is acid, an alkaline treatment will usually be preferred in the acute stage. The citrate and acetate of potash and bicarbonate of soda are the drugs used; the latter is the most powerful alkalinizing agent. The dose of the alkali should be increased until an alkaline reaction of the urine is obtained. When improvement in or disappearance of the symptoms has followed this treatment the infection of the urine will still be present, and if the patient is allowed up and the treatment discontinued a relapse will very probably take place. The patient should be kept in bed at least a week or ten days after the temperature is normal.

(b) *Antiseptic.*—Salol and boracic acid exercise a mild antiseptic action in the urinary tract when administered by the mouth.

Hexamine or urotropin is the most powerful of the urinary antiseptics, but its antiseptic action is subject to many influences not all of which are under the control of the physician.

Hexamine is a combination of ammonia and formaldehyde, and it acts as an antiseptic only by the liberation of the formaldehyde in an acid medium. When in combination with ammonia, and the combination is maintained in all alkaline media, the drug has no antiseptic action whatever. It is therefore useless to prescribe hexamine with an alkali.

Formaldehyde is an irritating antiseptic; it is important that hexamine should be absorbed in the form of hexamine which is non-irritating and that the splitting with formaldehyde should first take place in the urine. Hexamine should therefore not be prescribed with an acid and if a drug such as sodium acid phosphate is required to acidify the urine it should be prescribed separately from the hexamine. For the same reason hexamine is administered between meals in a large draught of water in order to avoid as much as possible the action of the acid gastric juice.

I have suggested the administration of keratin-coated tablets of hexamine in hyperacidity owing to the indigestion produced by hexamine in these cases, but this is rarely required in practice.

Some physicians have treated acute and chronic infections of the urinary tract by intravenous infusion of urotropin.¹ In acute cases Cooke² administered $\frac{1}{4}$ gr. daily and in chronic cases the same dose, with intervals of two or three days. Ten or twelve injections were used on an average in the case of seventy-five patients.

Many factors affect the antiseptic action of hexamine: among these are the following:—

(1) The amount excreted; (2) the degree of acidity of the urine; (3) dilution by artificial or pathological diuresis; (4) the time during which the hexamine is in contact with the urine; (5) the amount of mucus in the urine.

It is not possible to discuss all these factors here, but a word may be said in regard to two of them.

The time during which hexamine is in contact with the acid urine is important, from the fact that time is required for the liberation of formaldehyde from hexamine. This time-factor destroys the utility of the drug in a large number of cases. Hexamine on this account is of little value in kidney diseases or in infection of the renal pelvis. In the bladder, the diseases in which it is most wanted are those in which polyuria and frequent micturition are common, so that the length of time during which the hexamine is in contact with the acid urine is too short for complete splitting. The same applies to cases in which the bladder is continuously drained by a catheter or where a fistula is present. After prostatectomy hexamine is practically valueless until the bladder is closed.

The second factor is the degree of acidity of the urine required to convert the hexamine into ammonia and formaldehyde. If the urine is not already sufficiently acid the acidity is increased by the administration of acidifying drugs such as sodium acid phosphate or sodium benzoate in increasing doses. But an obstacle is met with in the intolerance of the acutely inflamed bladder to a high degree of acidity of the urine. In acute urinary infections, therefore, the administration of acidifying drugs and hexamine may only increase the severity of the symptoms without producing any effect on the disease. There are a few cases in which the urine is highly alkaline and in which the administration of acidifying drugs, followed by that of hexamine, clears up the infection rapidly and completely.

In the great majority of cases of acute infection it is, however, better to leave aside the question of antiseptic treatment altogether and direct the attention to soothing the mucous membrane by means of alkalies, diuretics and sandal-wood oil.

In chronic infections the continuous administration of hexamine over long periods is disappointing in its results. I find it is better to alternate the drug with other antiseptics such as salol and boracic acid, or to give alternate courses, of a fortnight each, of antiseptics in an acid urine, and alkalies. This alternation is specially useful when the bladder is irritable and intolerant of acidity.

The recently introduced combination of hexamine with resorcin (hexyl-resorcinol) has received a large amount of attention from the lay press, and extravagant claims have been made in its favour. The cases in which I have tried the drug are still small in number. In some there was pronounced improvement where other urinary antiseptics had failed, but there were other cases in which the drug appeared to have no advantage over other urinary antiseptics already in use. In one patient suffering from chronic parenchymatous nephritis together with urinary infection of long standing all the symptoms increased after the administration of moderate doses of the drug, and the patient died in six or eight weeks. I would suggest therefore that this drug be given with the greatest caution where nephritis is present.

Urinary antiseptics of the formaldehyde series act only in acid medium and there are many cases in which this cannot be obtained. There is urgent need, therefore, for a urinary antiseptic which will act in an alkaline medium. Methylene blue has long had a reputation as a urinary antiseptic but its action is somewhat ill-defined. It is a useful drug, especially in coccal infections, and acts in an alkaline urine.

The intravenous administration of antiseptic drugs has recently been advocated in America. In urinary infections intravenous injections of mercurochrome or of methylviolet have been used. Some success has been claimed for this treatment but on studying the reported cases closely one gets the impression that the action of these drugs is very uncertain. I have had a few opportunities of trying this method in advanced cases of urinary septicæmia but in none of the cases did either of these drugs exercise any influence on the course of the disease.

(3) *By vaccines.*—Vaccine treatment appears to be irrational in the acute stage of

¹ Blanc, *La Presse Médicale*, No. 36, May 6, 1925.

² Cooke, *New York Med. Journ.*, 1922, cxvi, p. 542.

urinary infection, and in chronic urinary infection it is very frequently disappointing in its results. We know, as clinicians, that there are many cases in which vaccine therapy appears to be worthless and we believe that in few cases does improvement follow its use. But which are those cases that are likely to benefit by vaccine treatment we have not been able definitely to ascertain. I cannot, therefore, believe that all patients are equally suitable for vaccine treatment or that the somewhat rigid scheme of dosage that appears to be well-nigh universal can be the best method for its administration.

The cases in which I have seen the best results given have been those in which the vaccine was administered during the decline of the infection when the temperature had fallen and the symptoms had abated but the urine was still infected. In very chronic cases, and in the type we know as bacilluria, vaccine therapy has in my experience done no good at all. I have tried the method of infusion of blood from an immunized healthy individual (immuno-transfusion), but so far with disappointing results.

The fact that a chronic case of urinary infection of many years' standing may suddenly clear up completely after an acute exacerbation and show no tendency to recur, leads me to suppose that vaccine therapy, if properly handled, might give better results than it does at the present time.

(4) *Local*.—Two important local methods of treatment consist in washing out the bladder and the renal pelvis. I must leave the former, but will say a few words about lavage of the renal pelvis.

When proof has been obtained that the focus of infection lies in the renal pelvis, lavage is a very valuable method of treatment and may be the only method that will bring about a cure. Great care should be exercised in the selection of cases. Lavage of the renal pelvis is contra-indicated where there is infection of the prostate and vesicles, where there is incurable infection of the lower urinary tract such as that complicating malignant growth of the bladder, and where there is obstruction in the lower urinary tract. In acute infections the method should be avoided. The only exception to this rule is in the case of the pyelitis of pregnancy, when good results sometimes follow the passing of the ureteral catheter and careful lavage. The method has been employed for young children, but I do not consider that it is wise to use it at so early an age.

Renal lavage is most useful in cases of persistent pyelitis where there are constant symptoms or recurrent attacks, and in cases of chronic cystitis secondary to pyelitis. In many cases the infection is cured by this method, but not in all.

At the end of a course of washing, the state of the urine from the renal pelvis may be much improved in condition, but still be infected. The distressing bladder symptoms may have completely disappeared, but they will probably recur at a future date. In these cases, however, the relief of symptoms is such that the patient is satisfied with the treatment and will return for a further course if the bladder becomes troublesome.

Persistence of infection in spite of lavage may be due to: (1) The use of too weak an antiseptic; (2) too long intervals between treatment; (3) dilatation of the renal pelvis; (4) stone in the renal pelvis.

Recurrence of the infection of the renal pelvis after the urine has been found sterile is due to infection from the bowel.

(5) *Operative*.—There is not time to discuss the indications for operation in urinary infections. Such operations may affect the kidney, bladder, prostate or vesicles and may involve drainage or excision. In the pyelitis of pregnancy emptying the uterus may have to be discussed.

(6) *Treatment of the bowel*.—The bowel is the chief source of the bacteria, causing urinary infection, and the infection may be repeated time after time. It is therefore necessary in all urinary infections to examine and treat the bowel. The method will consist in the administration of bowel antiseptics, in the treatment of atony of the bowel wall and other causes of chronic constipation. The question of operation for piles, for chronic appendicitis and for cholecystitis will also arise for discussion.

DR. OLIVER HEATH,

founding his ideas on a personal experience of acute cystitis due to a staphylococcus and a diphtheroid bacillus, suggested that the symptoms of frequency of micturition and of strangury should be considered to indicate an abnormal need to micturate frequently, first in order to increase the inflow of fresh blood by muscular action of the bladder wall, secondly to evacuate the irritant pus.

In the course of his own attack he had noticed that abstinence from all liquid increased

frequency and pain up to the point of strangury in four to five hours; whereas copious draughts of water enabled him to hold his urine in comfort for two or three hours. This he said, supplied a method of controlling the symptoms during the acute stage.

After suffering day and night for a whole month, he applied treatment on these lines to himself. Using a vaccine, he drank copiously until twenty-four hours after the first dose, when he withheld drink on each of two successive evenings until strangury occurred; a pint and a half of water was then taken as quickly as possible and gave relief in twenty minutes. This was repeated forty-eight and seventy-two hours after a second dose of vaccine, but caused less discomfort; and eight days from initiation of treatment cure was complete and permanent, as shown by the absence of symptoms and freedom of the urine from pus or albumin.

Mr. S. G. MACDONALD

inquired of Professor Dudgeon as to the significance of the hæmolytic organism and what practical bearing it had on treatment. Professor Dudgeon said he had not found this organism in the intestinal tract except on rare occasions—where then was the source of infection? In the recurring cases, with clear intervals, why did Professor Dudgeon state so dogmatically that the source of re-infection was some focus in the urinary tract? It might equally, and possibly more likely, be a re-infection from the original source. The value of vaccines was not in recent acute infections, but in cases of recurring infection and chronic cases with exacerbations. In his (Mr. MacDonald's) experience acute infections of the *Bacillus proteus* were rare, the chronic *proteus* cases were more commonly terminal infections and often associated with carcinoma. He (Mr. MacDonald) agreed as to the association of staphylococcal infections and stone; in chronic staphylococcal infections it was doubtful whether nephrolithotomy was worth while; the stone was almost certain to recur.

With reference to Sir John Thomson-Walker's remarks as to acute bacterial infections, in which the urine was sterile, he (the speaker) said this also occurred with renal block, the continuance of which would be an indication for nephrotomy. He asked Sir John's opinion as to where hexamine splitting took place. Certainly the damaged kidneys of urinary obstruction seemed incapable of producing free formalin. He (Mr. MacDonald) said he was sceptical as to the value of renal lavage except in isolated cases; he had tried it extensively and could only record one persisting bacteriological cure—this was a case of ureteral obstruction by a pelvic tumour in which pyelitis continued after the pelvic tumour had been removed.

Professor DUDGEON (in reply)

stated that by his methods infection of the urinary from the intestinal tract had been bacteriologically proved. Previous workers had suggested that the intestinal tract was the source of infection, but they failed to establish proof-positive evidence.

The reason for his dogmatic view that the bacillus isolated from the urine in subsequent attacks of coli infection, which he had proved to be the same as gave rise to the primary infection, must be lying dormant, was due to the fact that it was the same strain of bacillus. If it was a re-infection from the intestinal tract it would be more than unlikely that the same strain would appear on each occasion. Further, in cases which were regarded as cured with clear urine the bacillus could not infrequently be obtained if several c.c. were employed for the examination.

Sir JOHN THOMSON-WALKER (in reply).

The view has been expressed that in the cases of recurrent infection in which the urine is sterile between the attacks, there is some pocket in the kidney in which the bacteria lie dormant, and that this is supported by the fact that the same type of bacteria is found in each of the recurrent attacks.

This view does not appear to me to be tenable. No pockets exist naturally and no pathological sacculi have ever been found. There is no reason why the same type of bacteria should not re-infect the urinary tract from the bowel on succeeding occasions.

In the male, retained infections of the prostate and seminal vesicles with a sterile urine, are of course well recognized. In regard to the acute cases of infection in which the urine is sterile, there are two types of case. There is the case with infection of the kidney and renal pelvis where the outlet of the pelvis suddenly becomes blocked, and the urine, which previously contained pus and bacteria, clears up and becomes sterile while the symptoms of renal infection become more acute; and, secondly, there is the case in which a hæmatogenous infection has affected the cortex of the kidney alone and eventually leads to the formation of perinephric abscess.

The type of urinary infection in stone has been mentioned. It is an interesting fact, for which I have found no explanation, that a pure infection with the *Staphylococcus albus* is very frequently found where stone is present. The infection in these cases is spontaneous.

Other bacteria are, of course common in calculus of the urinary tract. I have seen a temporary bacilluria due to the *Bacillus coli*, which lasted a few hours and then passed off, in a case of stone in the ureter. In another case of stone in the ureter there was a persistent bacilluria due to the typhoid bacillus which cleared up completely when the stone was removed. The mixed infections of old-standing stone cases in which the *Bacillus coli* and the *Bacillus proteus* combine to give the urine a peculiar penetrating, offensive odour are well known.

It has been stated that renal lavage does not cure cases of pyelitis, or only rarely does so. With this I do not agree; I see many cases in which a complete bacteriological cure is obtained by this means. Recurrence of infection may take place in these cases, but it is, I believe, due to re-infection from the bowel. There are other cases in which improvement is observed and the most distressing bladder symptoms become less after the treatment; and there are cases where, for reasons I have already set out, no progress is made under renal lavage. It is difficult to lay down any rule as to the number of times renal lavage should be carried out before accepting failure of the method. I certainly think that if no improvement is seen after five or six washings, the method is not likely to be successful. The case must, of course, be fully examined under the X-rays and by pyelography, to find out the cause of failure.

It has been stated that there is a direct communication between the lymphatics of the ascending colon and those of the right kidney. I do not know of any scientific proof of this connexion. There are, however, many clinical facts that point very strongly to a lymphatic communication not only between the bowel and the right kidney but also between the bowel and the ureters and between the rectum and the bladder. I had hoped that more definite proof of these would be forthcoming.

[May 28, 1925.]

Demonstration of a New (?) Mycotic Parasite Found in the Bladder In a Case Resembling Malakoplakia.

By W. E. CARNEGIE DICKSON, M.D.

(With CHARLES GRAY, M.D., FRANK KIDD, M.Ch., and JOHN RAMSBOTTOM, O.B.E. (Mycologist to the British Museum).)

PATIENT, a lady aged 37, has suffered from a urinary infection for many years. In 1916, when I first examined the urine, there was evidence of pyelocystitis, with the presence of Friedländer-like bacilli, a few small Gram-positive streptobacilli and streptococci of *Streptococcus faecalis* type, together with some larger spore-like bodies suggestive of one of the *Fungi Imperfecti*, possibly a mould. These were found, not only in catheter specimens from the bladder, but also from the pelvis of the kidney. Corresponding with this fungus-like organism, a very "pleomorphic" organism was sometimes found in the cultures, but was extremely difficult to grow, and soon died out.

Similar findings were obtained from time to time, and on May 6, 1925, a small shred or clump of large, irregular cells, containing numerous vacuoles was found. When examined wet, and faintly stained with methylene blue and other stains, these were seen to contain apparently structureless, clear, rounded or slightly oval bodies or inclusions, which strongly suggested to my mind the so-called "Michaelis-Gutmann bodies" of malakoplakia. I examined a case of this extremely rare condition for Sir John Thomson-Walker in 1915; and, on comparing the present specimen with a drawing I had made of the malakoplakia case, the resemblance seemed to be more than superficial.

The only case fully recorded in the literature of this country is that described by Stuart McDonald and W. T. Sewell in their paper "Malakoplakia of the Bladder and Kidneys," in the *Journal of Pathology and Bacteriology*, 1914, xviii, p. 306. In this

they review the previously recorded twelve cases, in most of which *Bacillus coli* or coliform bacilli were found in the urine: and various hypotheses as to the nature of the large "malakoplakia-cells" and the "Michaelis-Gutmann bodies" are suggested, but in none were the latter supposed to be parasitic. M'Donald and Sewell think that

"*Bacillus coli* may, after all, only act by paving the way for the entrance of the specific causal germ, if there is one . . . We are left with the impression that the real cause must be sought for in a specific infective agent, or in some local predisposing factor on which some other exciting cause acts. Like other observers, we have entirely failed to demonstrate any such specific agent, protozoal, bacterial, or blastomycetic."

They therefore think that the explanation of the condition may lie

"In the presence of certain cell-rests of developmental origin, which are stimulated to activity with associated specific degenerative changes by some other factor which may possibly be the *Bacillus coli* itself."

Whilst investigating the present case I found that the only way of demonstrating the curious mycotic organism which I am showing at this meeting was in fresh wet films, either with or without a faint trace of some such stain as methylene blue, thionin, methyl violet, Sudan III, scarlet red, or the like. In these conditions, and in the original urine or other exudate, in vaseline-ring preparations made on sterile slides with sterile cover-slips, such mycotic organisms can not only be seen more readily, but will continue to live and grow, and develop into other stages and forms of their life history—whilst in dried films they either disappear entirely, or shrivel into unrecognizable "blobs" which do not in any way suggest their true nature. Even in preparations fixed wet, and still more in fixed sections, they are also so much altered that, in most cases, one would pass them by as something of no importance or as mere artefacts.

Such fresh wet preparations of the centrifugalized deposit of the urine of this case showed considerable numbers of large cells containing the cell-inclusions already mentioned. These inclusions varied much in size, from 3 or 4 up to 10μ or even more. When still quite fresh, they appeared absolutely structureless, homogeneous, rounded or slightly oval, and occasionally sausage-shaped, refractile and slightly greenish, and with a very definite, clear-cut outline. In a few hours little mycelial filaments began to sprout, and soon, apparently, every cell showed an appearance which may best be likened to germinating peas, the thickness of the filaments varying very much, from 1 or 2 up to 4 or 5μ , or even more. The growth was so rapid that there was occasionally a slow, snake-like, writhing movement, seen distinctly on two occasions. Yeast-like budding was also present. As the original cells of the exudate gradually disintegrated, very numerous spherical bodies were set free and passed into the fluid; these varied in size up to $10, 12\mu$ or even more in diameter. I shall delay giving a more detailed account of the morphology and cultural characters of the organism until Mr. Ramsbottom, Mycologist to the British Museum, has investigated it further.

Mr. Frank Kidd cystoscoped the patient ten days ago (May 18, 1925). From several small snippings he made from the surface of the patches, I prepared teased-out, fresh wet preparations and sections, which I show you under the microscope. The former exhibits the same curious organism in many of its forms which I have already described. Most numerous are the perfectly spherical, hyaline, structureless bodies, which can best be described as like little droplets of catheter oil. In fact, many of them might pass as such had oil been used in obtaining a catheter specimen, and did not some of them show budding, or the formation of filaments.

Meanwhile, I record the presence of this very pleomorphic fungus-like organism as occurring in the cells of the mucous membrane of the bladder, in which there

are patches of a lesion resembling, allied to, or possibly identical with malakoplakia: although paraffin sections of the very small portions of bladder mucous membrane available for examination do not show the classical histological appearances of that rare condition, but more those of a leukoplakia, with irregular thickening of the mucous membrane, oedematous swelling of the thickened layer of surface cells, and some reversion to stratified squamous prickle-cell epithelium. The submucous coat shows some irregular, patchy, small-celled infiltration, but in the minute portions of the tissue available for examination I have not found any of the typical malakoplakia cells. Many of the deeper surface epithelial cells show numerous vacuoles, but it is extremely hard to say, without a little imagination, that they have any definite contents which correspond with the globular bodies so easily demonstrable in the fresh, wet, teased preparations. If one had not already found that they practically disappeared in dried films, one would have been inclined to doubt their presence in the paraffin sections. Wet preparations, examined, if possible, with a binocular stereoscopic microscope, are essential for their demonstration.

[June 25, 1925.]

DISCUSSION ON STERILITY AND IMPOTENCE IN THE MALE.

MR. KENNETH M. WALKER.

I am glad that the Section of Urology has selected the subject of sterility and impotence in the male for discussion to-night, for although these conditions may cause very little damage to health they are responsible for a vast amount of unhappiness. All that I can do in this introductory address is to touch on certain salient points, since a full review of the subject, including, as it should, a consideration of the physiology of reproduction, the various pathological conditions that may affect the genital tract, and the psychology of the sexual function, would not only demand more time than you are prepared to allow me, but also more knowledge than I claim to possess.

For the sake of convenience I shall first discuss male sterility and then deal with the subject of impotence.

STERILITY.

It is important to remember that sterility is a relative term. Lode has estimated that in a single ejaculation some two hundred million spermatozoa are emptied into the vagina, of which vast number only one will in favourable circumstances reach the descending ovum. It is easy to see that if this number is reduced, or if the health and vigour of the spermatozoa are impaired, the chances of a successful impregnation are diminished.

By investigation of a specimen of semen it is possible to form some rough estimate of the number and vitality of the spermatozoa present, but it must always be borne in mind that our examination is far from complete. The microscope and the test-tube can only reveal gross abnormalities, and it is quite possible that some of the males whom we pass as normal are nevertheless responsible for the childlessness of their wives. It is indeed probable that certain cases of repeated miscarriages in the female are due to the death of ova that have been fertilized by imperfect or unhealthy spermatozoa, although microscopic examination of the semen may have failed to discover any abnormality. But even if we confine our attention to abnormalities sufficiently gross to be detected by the microscope or the test-tube, relative sterility will be found to be very much commoner amongst males than was believed some fifty years ago.

Of thirty-two husbands referred to me by gynaecologists, thirteen were absolutely normal in all respects and nineteen showed varying degrees of impaired fertility, ranging from complete azoospermia to a mild degree of oligospermia. However, as these cases were only sent to me as the result of the gynaecologist having found no gross abnormality in their wives, these figures cannot be taken as an expression of the frequency with which the male is responsible for an infertile union.

Looking through my case sheets I find that by far the commonest cause of sterility in the male is gonorrhœa. In most of these there was a history of epididymitis, but in some the

obstructive lesion responsible for the azoospermia was a prostatitis or vesiculitis blocking the ejaculatory ducts. Benzer's observations on the after-history of German soldiers who had suffered from gonorrhoea and had subsequently married, are of special interest in this connexion. Of those men who had escaped epididymitis altogether 10.5 remained childless; of those who had suffered from a unilateral epididymitis 23.4 per cent. were sterile, and of the bilateral cases as great a number as 41.7 per cent.

Gonorrhoea causes azoospermia by mechanical blockage in the genital tract, the blockage as a rule being permanent.

I have been interested in finding that sometimes a healthy young man, free from venereal history, shows a complete absence of spermatozoa in the semen. In two of such cases I have been unable to find anything abnormal in the patients' condition or in their past history beyond the fact that they had previously suffered from shell shock and neurasthenia. It has, of course, long been recognized that the activity of the tubules varies in different states of health, and it would not be surprising to find that spermatogenesis is to some extent under the control of the nervous system. However, even if this is the case it is difficult to believe that any permanent inhibition of spermatogenesis can be brought about by functional disease of the nervous system.

Age is not such an important factor in the fertility of the male as it is in that of the female. An examination of testes removed from elderly subjects shows great discrepancies, some specimens revealing great activity and others signs of regression. There is, however, a gradual diminution in spermatogenesis after the age of 50, although I have examined a testicle removed from a nonagenarian in which the tubules were full of spermatozoa.

The well-known sterility of caged wild animals shows that alterations in diet and in habit also have their effect on the testes, and for this reason the preliminary examination of a patient should include within its scope an investigation of his habits, diet and general physical state. Considerable variations in the fertility of rats can be brought about by alterations in diet, and a deficiency of vitamins definitely impairs fertility. Thus Eckstein (*Archiv f. d. ges. Physiol.*, 1923, cci, p. 18) has shown that a diet deficient in fat, vitamin A and phosphorus leads to atrophy of the testes and failure of spermatogenesis in male rats. It is equally true that a change from a sedentary to an outdoor life will, in a human subject, sometimes result in an increase in the spermatozoa found in the semen.

Drugs, so far as I know, have only an indirect action on spermatogenesis, although quinine is regarded by many Anglo-Indians as a cause of sterility if it be taken over long periods. Changes of temperature are more important in this connexion. It has been shown experimentally in rabbits that moderate heating of the testes, either by sunlight or by warm baths, stops spermatogenesis. Why the testes of mammals should be so susceptible to heat is a difficult question to answer, but there is much to support the theory that the migration of the testes from the lumbar region to the scrotum is not unconnected with this question of heat. It is at any rate certain that the scrotum is so constructed as to form an excellent heat-regulating device.

Lack of time prevents me from going into the practical details of an examination of a patient for sterility. It may, however, be said that an investigation consists of three parts:—

(1) A careful scrutiny of the patient's history, paying special regard to such illnesses as mumps, typhoid fever, endocrine disorders, venereal disease and genital complaints. It should also include an investigation of the patient's habits, diet, consumption of alcohol and tobacco; (2) a careful examination of the genito-urinary apparatus for the existence of stricture, nodules in the epididymis, signs of disease in the prostate and vesicles, atrophy of the testes or any other gross lesion of the genital tract; (3) a microscopic examination of three specimens of semen, obtained after a week's abstinence from coitus.

It must, however, be remembered that at the best a microscopic examination of a condom specimen furnishes only a rough estimate of the vitality of the spermatozoa. More accurate information may be obtained by searching the female passages at different intervals after coitus and determining the presence of active spermatozoa in the vagina, cervical canal and the fundus uteri respectively. This examination, however, is so tedious and unpopular that it can seldom be realized.

IMPOTENCE.

In order to save time in discussing the question of impotence I shall not deal with cases that are secondary to an organic lesion in the genitalia, or in some other part of the body but only with those of primary impotence where no gross physical lesion can be found. These cases form the bulk of those patients who come to the consulting rooms for help, and

their treatment furnishes a problem of the greatest difficulty. We examine them carefully and find nothing abnormal except perhaps a hyperæsthesia of the posterior urethra and a verumontanum which when seen through the posterior urethroscope appears swollen and œdematous. Not infrequently these patients state that they have suffered in the past from excessive nocturnal pollutions. Sometimes they complain of a clear, mucoid discharge at the end of the penis. On inspection the genitalia appear limp and flaccid and the cremasteric reflex is absent or sluggish. In making a rectal examination the external sphincter feels atonic and no hardening of the prostate can be obtained on massaging that gland. The patient complains that erections are imperfect and that the ejaculation occurs prematurely. These cases may be described under the heading of "atonic." As a rule they are functional in origin although sometimes a minor lesion of the genitalia will be found which may or may not be a factor in its causation. It may also be said in parentheses that a functional impotence may occur as a sequel to an organic lesion of the genitalia or neighbouring organs. For example, I have seen patients in whom the original exciting cause has been an operation on the bladder or prostate, the temporary disability produced by the operation "suggesting" to the patient an impotence that was therefore psychological in nature. A transient or a very mild organic lesion may therefore be the starting point of a stubborn functional impotence. It is obvious, therefore, that in treatment due attention must be paid to any organic lesion that may be found, however trivial it may seem. At the same time such psychological measures as reassurance, persuasion and suggestion are carried out.

Since many cases of functional impotence are "atonic" in nature it is obvious that attention directed towards the improvement of the general health and muscular tone of the patient is important. Hence the value of cold douches, Swedish exercises, out-of-door sports, massage and tonics. But at the same time it must be remembered that such measures constitute only a part of the treatment and that in the absence of experienced psychological handling a case of functional impotence will rarely recover by such means alone.

Sometimes the incapacity to perform the sexual act is complete, and sometimes it is only partial. Sometimes the impotence is one of many other neurotic symptoms and its psychical origin is apparent to any observer; but sometimes, on the other hand, the patient would outwardly appear to be a normal healthy being and his disability comes as a surprise to himself and his medical advisers.

Careful searching may be required before the underlying psychical factor is found. Generally it takes the form of fear, for fear is the greatest of all paralysing forces. It is indeed the Medusa head that turns us into stone, and its action on the sexual function is especially marked. It may take many forms and of these I can only mention a few. Perhaps the commonest is the fear of the consequence of masturbation. Ninety-eight per cent. of males have masturbated at some time or another, and it is a common belief that masturbation may inflict permanent damage on the sexual apparatus. The patient often zealously guards the secret that he has masturbated, and it may only be at the second or third visit (when confidence in his medical adviser has been established) that he will confess his guilt and express the conviction that to this is attributable all his present trouble. Another example of the action of fear is furnished by the man who has learnt to look upon the sexual act as something to which a stigma of guilt is inevitably attached. From time to time sexual thoughts may have troubled his boyhood, but with fear in his heart he has turned from these temptations of the evil one and hurried along the narrow path of chastity. Eventually he marries late in the thirties and finds that he is impotent. How bitter is the discovery to one who feels that impotence is a punishment that should have been meted out to the rake rather than to the man who has led a life of virtue.

In another type of case the inhibitory cause is the fear of contracting venereal disease, a fear that may continue to act long after such a danger as venereal disease has ceased to exist.

Finally, there is the fear that comes from perusing books dealing with aberration of sex. At the present moment I have on my hands two patients whose troubles are almost entirely due to an indiscreet excursion into medical literature followed by a review of their own feelings or symptoms. Coitus, like digestion, should be an automatic act. Once the higher centres begin to interest themselves in either of these functions impotence and functional dyspepsia are not far ahead. These intimate affairs should be left to the discretion of the solar plexus and the lumbar enlargement of the spinal cord.

Although fear is the most important factor in the production of impotence it is not the only one. In certain cases active sexual desire has never been experienced. Sometimes this is associated with a physical defect such as testicular deficiency or eunuchoidism, but as often

as not no physical explanation can be found for the weakness of the sexual appetite. The patient may state that pictures and statues of an erotic nature have never aroused any excitement in him and that he is only slightly conscious of the attraction of sex. Occasionally this deficiency is due to an error in the direction the sex urge has taken, and then the patient will be found to be suffering from some peculiar form of perversion. In other cases what the psychologists term "sublimation" has occurred and the libido of the patient has found an outlet in, for example, excessive devotion to his business or profession.

But the subject on which I have embarked is beyond the scope of this brief address, and I must content myself with having emphasized the importance of the psychological factor in the production of impotence. It is the master key that unlocks the door to the right understanding of the case and to its treatment. Indeed, it may be said that compared with the psychological handling of the case, the physical treatment is of minor importance. Drugs, electricity, urethral instillation, baths and exercises, all have their place in treatment, but if the psychological side of the case has not received attention, these measures will have little or no effect. Sometimes the problem is so intricate that the help of an expert psychologist will be required. Psychotherapists are, not without some reason, looked upon with suspicion by the average surgeon, for unscrupulous practitioners, and ill-balanced exponents of psychological medicine, have combined to bring this branch of the healing art into disrepute. Yet I have no hesitation in affirming that the treatment of impotence generally lies along psychological lines, and that the best results will be obtained by the co-operation of a urologist with a psychotherapist. Your surgeon is, as a rule, a poor psychologist. He is concerned with the aberration of the body and is prone to under-rate the mental factor in disease. He will perform an exploratory laparotomy on a nervous patient without for a moment considering the effect of the operation on the patient's mentality. Yet the scar he leaves on that patient's abdomen may be as nothing compared with the scar he leaves on his mind. No! Surgery is a bad training ground for a psychologist, and for this reason a closer co-operation will be required between the urologist and the psychotherapist if these difficult cases of functional impotence are to be brought to a successful conclusion.

Mr. FRANK KIDD: Sterility in the male is most commonly due to blockage of the vas deferens or the epididymis, from gonococcal or other infections. This may prove a matter of grave concern to those who wish for an heir to an estate or to those who wish to satisfy the parental instincts. Operations have been devised with the object of anastomosing the vas to the head of the epididymis so as to permit the spermatozoa in the testicle to reach the vesicles. I have only had the opportunity of performing this operation on three occasions. None of these cases proved successful, though live spermatozoa were found in the head of the epididymis at the time of the operation.

It is difficult to find out whether this operation has been successful in the hands of others. Martin states he has had success in a few cases, but I have not been able to find other reports of successful results. The excuse usually made for non-success is that there is a block in the vas deep in the pelvis. Such a block was not present in any of my cases, as I was able to pass a fine piece of silkworm gut down the vas for its full length in each case.

In future I shall be inclined to try a puncture of the testicle and artificial insemination, as German authors state they have been successful with this method. In the first case in which I proposed to try this I punctured the testicle and found it was cystic and contained no live spermatozoa. I am hoping to carry out this procedure in a second case shortly, and I exhibit a special needle and syringe which I have designed for the purpose.

As regards sterility on the part of the woman, a pig breeder of my acquaintance states that he finds infertile sows have a too acid vaginal secretion. He has employed sodium bicarbonate douches and by this means has rendered the infertile sows fertile. Sodium bicarbonate douches are worth recommending for sterile women.

It should not be lost sight of that suggestion may play a rôle in the cure of sterility in women. In a book called "Van Zanten's Happy Days" an account is given of a girl who thought she was bewitched, and remained sterile. She was taken to a witch doctor, who apparently did nothing but practise very violent suggestion upon her and she immediately became fertile.

I will now turn to an observation of considerable interest that I was able to make. In 1910 I was consulted by a young man with double inguinal hernia, each testicle being at the top of the scrotum. The testicles were always getting bruised. He wanted the

ruptures cured, as he was an athlete; he asked that the testicles should be put back in the abdomen to make sure of a radical cure, as he was about to get married. The marriage of his fiancée's uncle had turned out a failure because he was sterile, and her guardian came to me and said she must be certain this would not happen in this case. The testicles were tested and found to be full of living spermatozoa. Radical cure was performed on each side, each testicle being replaced in the abdomen. The marriage took place eighteen months later, but there were no children of the marriage, and the secretion of the testicles tested on several occasions was found to have become sterile. Separation ensued and the woman married again and had a child by the second husband.

We are still in the dark as to the meaning of the descent of the testicle. In certain mammals the testicles appear only to descend at the rutting season. In man they have descended for good and all and there is no periodicity of sex. That this is only a recently acquired condition is suggested by the fact that a large number of undescended testicles are seen in the male. I had thought the sterility of non-descended testicles was due to a congenital defect in anatomy and physiology closely correlated. I imagined that if they were partly descended, as in this case, and were found to be fertile, the mere putting back of them in the abdomen would not affect their fertility. The result shows that this idea is not correct.

Here is an instance in which the environment appears to have reacted directly on the germ cells, in contradiction to the teaching of Weismann. The origin of species and the origin of varieties and mutations still remain a profound mystery. There is some key we have still to find which will unlock the door. The longer I live and observe living matter the more convinced I become that the need for function determines structure; anatomy seems to follow physiology. Here, by reversing the anatomy we reversed the function. The need for a new function seems to call into being a new organ, or else seems to modify an old organ in such a way as to satisfy the function.

Turning from these more general considerations and coming to the testicle itself, the latest idea put forward is that the testes need to be kept at a very even temperature and one below the temperature of the abdomen in order to remain fertile. If the testes were present in the abdomen they would be too hot. By descent of the testicle and the action of the cremaster muscle the temperature of the testicle is supposed to be exactly regulated so as to keep the organ fertile. This is an ingenious idea but I doubt whether it expresses the whole truth. Anyhow there appears to be something inimical to the fertility of the testicles if they remain in the abdomen or if they are replaced in the abdomen, as we see here that fertile testicles replaced in the abdomen become sterile. It suggests that testicles should not be put back in the abdomen at operations for radical cure, but that they should be replaced in the scrotum by being unwound.

I examined thirty cases operated on by Mr. Jonathan Hutchinson, Junr., in which the testicles were replaced in the scrotum. Seventeen of these patients had normal testicles in good position which appeared to be actively fertile.

Dr. MICHAEL LEAHY said that with regard to psychical impotence, he had had the opportunity of treating some cases, and would give the results attained in some of them. He argued that as anxiety was the cause in psychical impotence in most cases, and as this was a mental state, it was essential to get at the mind of the patient, and to endeavour to inspire in him confidence that he was capable of performing the sexual act—in other words to work on what psychologists called the subconscious mind, and what he described to his patients as their imagination. He told them they thought they could not perform the act, and that that impression was the only reason they could not—here he was not referring to organic cases. Often the reply to that was that the patient had been trying to persuade himself to the same effect for a long time, but with no result. He then told a patient he would inoculate his mind with the idea that he was capable of coitus, and then it would be done without any special effort of the will. There was a sound reason why it should be done automatically, namely, that the function itself was an automatic one. His aim was, accordingly, to get the patient into a suggestible condition. When a man said it was impossible for him to get a firm, rigid erection he (the speaker) set out to show him that by an act of suggestion another part of the body could be made rigid, and then it was obvious that this could equally apply to the penis. He told the patient, whose right arm was resting on the arm of the speaker's chair, that this arm would begin to rise up towards the ceiling and become firm and rigid, and that this

could be taken as a proof that the suggestion could be made to apply to some parts of the body, and equally to the penis. Usually this statement bewildered the patient, but he was willing to listen. Generally he (the speaker) succeeded in inducing rigidity of arm or leg in this way—for reasons of delicacy the penis was not included in the demonstration. This induced confidence in the patient, subsequently a firm penile erection resulted, and the psychical impotence was overcome.

A man, aged 49, was a schoolmaster for seven years, and for a year was a planter in Australia. Practically all his life he had had financial worries; before his marriage he had never had connexion with a woman, and had experienced no desire to do so. He had masturbated from his thirteenth year, and he had a brooding disposition. His complaint when he applied for advice was that he was unable to have a normal healthy connexion with his wife, a lady aged 28, to whom he had then been married eleven months. He produced an erection, but attempts at intromission resulted in a penile telescoping. He (the speaker) suggested to the patient that he would soon be having a firm erection normally, that his anxiety on the matter would then disappear, and his physical power would develop. Between September 28, 1923, and December 3 in the same year he gave the man ten treatments. Half-way through the treatment the patient was attacked with influenza; he then developed boils and a sore throat, and discontinued the treatment. He (Dr. Leahy) thereupon wrote to him, and said that if only he would believe what had been told him in the consultations all would be right. In the following April a letter came reporting that he had "got there six times in the past four days."

Another case was that of a man aged 53, a journalist, who was, physically, remarkably "soft" and flabby, with low virility, though apparently very intelligent. All his life he prided himself on having cut out of his life all ideas of sex, and evidently he was a virtuous man. At the age of 53 he married a woman of 28, and since that event he had had a great desire for connexion, but found he could not gratify it. He came for a consultation on January 21, 1924. He was very "fussy" and pedantic. He (the speaker) treated him by suggestion, and rigidity of the arm occurred at once. This made a great impression on the patient, and he talked about it for five minutes, saying how wonderful it all was. Later, a letter was received from him saying that on the night following the interview, instead of the production of an erection taking the usual fifteen minutes he had had a very satisfactory erection in two minutes. Later he reported successful coitus, and said he was quite happy about it. Six months later (i.e., in July, 1924), he reported he was having successful cohabitation two to three times weekly since treatment.

A man, aged 33, shy and hypersensitive, depressed and slow of speech, had been married three years, when he came for consultation. He had never had connexion with his wife at that date, nor with any other woman; the attempt always resulted in the penis being telescoped. When he came to him (Dr. Leahy) he was suffering from colitis, and had been so suffering for about twelve months. When a boy, he had indulged in self-abuse; all his life he had had a dread of venereal disease and avoided women for this reason. He was, however, an athlete, and represented his university at lacrosse. He was, indeed, a very good type of man, though *very* depressed. He consulted the speaker on November 17, 1919, and between then and December 18 he was treated sixteen times. Treatment was then discontinued but the patient was told he would succeed in time. He was well off financially, but had no occupation. He had married a charming wife, who, however, when advice was sought, was threatening to leave him. This man said that between January, 1917, and April, 1919, he had had no erection at all, and his failure made him very nervous and exceedingly depressed. Soon after the treatment commenced the note he made was that the erections were now frequent. As showing the perseverance he practised, he said that on January 22, after two hours in the morning, he succeeded! On the twenty-ninth of the same month he managed it in a few minutes. On September 28, 1923, he (Dr. Leahy) met the patient in the street and the patient informed him he was the father of three children.

A great deal was said about mental complexes and pre-marital experiences, but the plain English of psychical impotence was that the man was *obsessed with the idea "I CANNOT perform,"* and what the doctor had to do was to give him *THE IDEA* that he could. But the medical man must himself believe he could do what he said he could; it was wrong, and

a mistake, to try to "bluff" the patient. Nor was it wise to start reasoning with the patient, and trying to find out *why* he was impotent. Patients would often ask whether their disability was due to heredity, or whether it was a frightful punishment for indulgence in masturbation. But such searchings for causes should be discouraged, and every effort concentrated on accomplishing the act. That was to say: One must impress on the patient that it was useless and even *waste of energy to search for "the reason why."* The patient must be persuaded to concentrate on the idea that he could succeed. In the ordinary conscious state it was not generally possible to persuade the patient to do this. If, however, the doctor in charge was able to induce in the patient the subconscious or suggestible state he would be able to fill the mind of the patient with the idea "he could" and from the idea the reality would develop.

Mr. J. SWIFT JOLY commended the Council of the Section for having arranged this discussion, as the subject was not only important, but also difficult, and often it was either not properly handled, or was treated in an off-hand manner.

With regard to the actual obstruction in the vasa, he had done the operation of short-circuiting the vas on three occasions that he remembered, possibly on a fourth, but he had never had any good result from it. The most promising of the four failed through a peculiar set of circumstances.

The case was that of a man aged 39, who was sent to him by a doctor in South Africa, with the statement that he had been married nine years and was very anxious to have children, but was unable to bring this about. At the age of 6 he had scarlet fever, and then had double testicular inflammation; distinct nodules formed at the lower end of both epididymes, and they suppurated. In 1912—when he was 31—he went to Berlin, and Posner needled both testicles and extracted motile spermatozoa from one side, but not from the other. Nothing further was done at the time. He saw the speaker in 1920, and then he had had obstruction thirty-three years. His doctor had assured him that his wife was healthy and normal, and that the failure to have children must be debited to him. He (Mr. Joly) therefore advised an operation, but warned him he did not think it would enable him to beget children. He (the speaker) did an anastomosis on both sides, and at the operation motile spermatozoa were obtained from the testicle opposite to that from which Posner obtained them. After the operation, a few motile spermatozoa were found in the material expressed from the seminal vesicles, though there had been none before. Only when he returned four years after the operation did the man tell him that his wife was more than ten years his senior; therefore she had passed the menopause, and this accounted for the failure to procreate.

He believed his other cases had been uniformly unsuccessful.

Mr. FOWLER WARD said a cause of impotence which he did not believe was uncommon was the great affection of a husband for his wife, which made him sensitive about performing the sexual act. To such his advice was to go home and have a good meal and some alcohol, and "be a brute." It always worked successfully.

Mr. R. H. JOCELYN SWAN said Mr. Kidd asked about results in cases in which there had been carried out an anastomosis between the vas and the epididymis in cases of patients who had previously had gonorrhoeal epididymitis. He (the speaker) had performed this operation in two cases, but without any result at all. In each case it was a bilateral anastomosis for block, and he investigated the lumen of the vas with silkworm gut, which was passed easily.

When he was at Woolwich, an officer in the Army Veterinary Corps was admitted with tuberculous disease of one testicle, the removal of which was advised. That officer told him a curious thing. He said he had been married twelve years, and had had perfectly normal and natural connexion, but had never had any children. He added that it was known in the veterinary world that a horse which had had one testicle removed was more fertile than was the ordinary stallion with two active testicles. He (Mr. Swan) removed this officer's tuberculous testicle, and eighteen months later he came to tell him that his wife had had a child, and two years later still that she had twins.

Mr. CYRIL NITCH (President) said that Mr. Kenneth Walker had referred to the importance of pre-existing disease, such as gonorrhoea and epididymitis; he had also laid stress on the importance of diet and had pointed out the place of drugs. Mr. Walker agreed that there was a large psychical influence operating in these cases.

Mr. Kidd had restricted his attention to the treatment of sterility by operative means. He (the speaker), speaking as a general surgeon, could not see how the operation of anastomosis between the vas and the epididymis could ever be successful, and he would like to hear whether Mr. Kidd knew of any cases so treated which were successful. The vas had a thick wall, and was a narrow channel; and the operation of anastomosing that to the epididymis was, it seemed to him, doomed to failure. Also, he thought that a silver wire in the vas deferens for ten days would destroy the epithelium and convert the deferential canal into a fibrous track.

Mr. Kidd had also referred to replacement of testes in the abdomen leading to sterility. It was known that imperfectly descended testes were usually accompanied by sterility. He (the speaker) knew one patient both of whose testicles were undescended, but he had a large family. Perhaps that was contrary to the general rule. There was one objection to replacing an imperfectly descended testicle into the abdomen, namely, that of late years observers had noted that a large proportion of such testicles had eventually become malignant.

Dr. Leahy's address was very interesting, and he was evidently on the right lines. Many of the cases were purely mental, and, as Dr. Leahy said, fear was a great cause. One of his (the speaker's) patients had been well and virile; suddenly he became nervous about himself, then frightened, and finally impotent. Separation from his wife for a time and other measures were tried, but attempts at coitus resulted in telescoping, and that was still his condition. Examination with the posterior urethroscope revealed a large, congested verumontanum, which he (Mr. Nitch) seared laterally with a diathermy electrode. He did not think this would have any marked effect, but it was possible that the feeling that he had had something done for him might act beneficially in a psychical way. He would like to hear from Mr. Kidd whether he knew of successful cases following Martin's operation.

Mr. FRANK KIDD (in reply) said he was hoping for some light to be thrown on the results of operation, as mentioned by the President. Posner said he was unable to find a single success recorded. He (the speaker) could not find Martin's original paper. Morton quoted Martin as having a few successful results; but it was always difficult to be certain of these results, because there was always the factor of the interloper.

He was sure one could get the vas to join to the epididymis with an opening, for a time at least, but the spermatozoa did not seem to get through. He used very fine silver wire, and he did not think that would injure the vas at all. He intended this as an improvement to the operation, but it did not succeed.

The President mentioned sending patients to a psycho-analyst; but he (the speaker) found that the psycho-analyst did not benefit the patients, and for some years he had been sending his patients to Dr. Leahy, who had brought about some wonderful cures and in patients whom one would have regarded as hopeless. He was sure these cures resulted because Dr. Leahy did not go into the depths of their sexual complexes, but tackled them in a manly and direct way.

Mr. KENNETH WALKER (in reply) said that, in Mr. Kidd's experiment of returning testicles into the abdomen, it must be remembered that the testicles were normal ones. Conditions were different, therefore, from those that obtained in the congenital cryptorchid. Imperfectly descended testicles were almost invariably imperfectly developed ones. He did not know that such an experiment had been done on human beings before, but it had been proved in the case of rabbits that normal testicles when returned into the abdomen underwent regressive changes, with cessation of spermatogenesis. If these testicles were again drawn down into the scrotum the activity of the tubules was renewed, with formation of spermatozoa.

Mr. Fowler Ward's advice to husbands was excellent. He (the speaker) discovered sometimes that the cause of a man's impotence was to be found in his wife. Not infrequently one came across a poor, sensitive husband overcome with shyness and confusion, and married to a big, physically robust wife of a domineering demeanour. Sex reversal, whatever its success might be in the poultry yard, was a failure as regards human life. Emancipated woman could not have it every way. If she desired to rule the household she must be

prepared to rule an impotent husband. He had frequently told men who consulted him to take the "cave-man" attitude towards their wives, and a little alcohol would often assist in the assumption of such an attitude.

Mr. Jocelyn Swan's narration of the case in which removal of a tuberculous testicle had been followed by the arrival of twins was interesting. It was true that a horse with only one testicle descended was usually more fiery than was the ordinary stallion; but in the case mentioned he thought the explanation was that the removal of a tuberculous testicle had resulted in the improvement in the condition of the prostate and the vesicle on the same side. This would materially increase the fertility of the patient. He agreed that enforced separation of husband and wife for a time was often a benefit.

Some had spoken hopefully of artificial insemination, but he was surprised how unsuccessful artificial insemination had been in the case of the human being as compared with what happened in the case of animals. It was one of the puzzles on which he would like further light.

War Section.

President—Surgeon Vice-Admiral JOSEPH CHAMBERS,
C.B., C.M.G., R.N.

Hospital Ships in Peace and War :

PRESIDENT'S ADDRESS.

By Surgeon Vice-Admiral JOSEPH CHAMBERS, C.B., C.M.G., R.N.

HISTORICAL.

Old French and British records show that so-called sea-going hospital ships were attached to fleets even as early as the end of the seventeenth century, and a French report about this time recommended the use of a ship entirely devoted to the sick and completely staffed and equipped. The difficulties in trans-shipment, however, inherent in sail power, seem to have led to a comparative failure of the principle when tried at the Battle of Beachy Head in 1690.

The first *modern* vessels to be used for the purpose were two converted iron steamships, for the China operations of 1860. These were in attendance upon the Army and their employment was followed at intervals during last century by that of others fitted out for similar purposes. One of the first purely *naval* hospital ships was provided by Japan for the war with China in 1894-5; and the *Malacca*, in 1897, was equipped by our own Navy to serve as a base hospital for the Benin expedition.

Considerable numbers of such vessels served during the Spanish-American and South African wars, the Boxer troubles, and the Russo-Japanese war, mainly in attendance upon the respective armies as sick transports. Since early in the present century permanent naval hospital ships have been a feature of the American Navy, as well as of our own, and are of interest to us to-night, from the fact that such vessels provide the only examples of sea-going floating hospitals utilized in peace time.

Finally, during the Great War of 1914-18, no fewer than twenty-one naval and eighty-four military *hospital ships* were provided by this country alone; these ranged from the gigantic *Mauretania* (30,784 gross tonnage) down to the small "drifter" *Queen Alexandra* (268 gross tonnage).

Owing to the enormous proportion of the overseas campaigns undertaken by the Empire, these numbers far exceeded the total employed for the purpose by the Allies and enemy combined.

THE EVOLUTION OF THE HOSPITAL SHIP.

Naturally it is a far cry from the old time floating hospital to the ship of to-day and in most directions improvement has been continuous. The substitution of metal for wood, the application of steam to water-supply, pumping, heating, and disinfection, and of electricity to lighting, ventilation,

heating and laundry, and the cold storage system for food have all played a notable part in this development. Advances in medical and surgical science have at the same time emphasized the necessity for surgical cleanliness and all it implies, cubic space, fresh air and sunlight, in the treatment of disease and injury.

Nevertheless, in some ways the provision of ventilation, and more particularly of equability of temperature, is more difficult in the metal steamship than in the old wooden vessel, and it is in the direction of lessening movement, vibration and noise, the improvement of ventilation, and the control of "wild heat" (heat given off in ships by main and auxiliary engines, steam pipes, &c., which produces unequal and irregular heating in the living parts), that further improvement can mainly be expected.

SOME ESSENTIALS IN CONSTRUCTION AND EQUIPMENT.

The one outstanding advantage of a ship originally designed for hospital purposes is that it would be possible so to arrange the details of construction as to economize space in the highest degree and, at the same time, to ensure that every section occupies the most convenient and appropriate position.

Unfortunately in the present state of world finance only the United States (which has provided such a vessel) can afford the necessary expenditure, and less fortunate countries either have to depend on vessels adapted for the purpose, or follow the Japanese plan of designing certain mercantile craft with a view to such conversion.

At present, therefore, hospital ships must be improvised from already existent vessels, and the first question to be considered is that of size, which has already been shown to have varied in the recent war from that of the *Mauretania* downwards. In spite of the proved usefulness of these leviathans in rapidly conveying large numbers from Gallipoli and elsewhere and their steadiness at sea, their use for the purpose is nevertheless attended by disadvantages quite apart from possible administrative difficulties. For example, the *Aquitania* and *Britannic* were too large to enter the harbours at Alexandria and Malta and the number of cases for passage to England at Mudros was not always sufficient to fill them, fitted as they were for 3,500 and 4,000 men respectively. Consequently special arrangements had to be made to avoid demurrage at Mudros. All their coal and water had to be transported there and the daily consumption of these big ships was considerable even when at anchor.

In general, the combination of a draught shallow enough to enter ordinary harbours with sufficient tonnage to accommodate 300 or 400 patients on decks at least 7½ ft. in height, with the principal wards situated above the water line, and providing 500 cubic feet of space per head, will meet requirements.

Nor is there under ordinary circumstances any very great advantage in high speed, as this implies greater cost, absorption of space, production of "wild" heat, more frequent replenishment, larger crews, and increase of discomfort in bad weather. A cruising speed of about 10-12 knots, with something extra for emergencies, is probably sufficient, and as regards purely naval hospital ships subsequent remarks on their method of employment will also be found to have a bearing on this point.

From their increased cleanliness and the ease of replenishment oil-burners are, of course, advantageous.

Steadiness at sea and maximum freedom from vibration are of importance and, if possible, the principal wards should be located where movement is least felt and the ship is least "wet." Promenade decks assist in the latter and help ventilation in bad weather.

It stands to reason that steam power must suffice for the subsidiary purposes already mentioned in addition to motive power, and that the electrical plant should be capable of providing for lighting, ventilation, hoists and so on. It is decidedly an advantage if the ship be already fitted with wireless, submarine signalling and refrigerating rooms and other stores; while her storage capacity for water and the yield from her distilling plant should be very large. It is also of extreme importance that her general construction should facilitate the fitting of easy approaches to the principal wards and of lifts in convenient positions.

Usually it is desirable to locate aft and on the upper deck those departments which are apt to produce unpleasant odours or from which infective material may be derived, such as infectious wards, mortuary, laundry and disinfecter. The possibilities of traffic have also an important bearing on the positions of such departments as the disinfecter, where the disinfection of hospital parties has to be constantly carried out, and also the dental department. Proximity to a gangway, and to a bathroom in the case of the disinfecter, is consequently indicated.

With regard to equipment it is impossible to make more than a passing reference. All that can be said is that it includes everything that is requisite for medical, surgical, zymotic and mental wards and their annexes, operating, dental, bacteriological and post-mortem departments and laundries, galleys, pantries, latrines and wash-places attached to the hospital, in addition to the requirements of the crew and staff.

It is to be noted that as a result of a meeting held in the Transport Department in November, 1922, it was agreed by representatives of the Naval and Army Medical, and the Admiralty and War Office Transport Departments that the specification for naval and military hospital ships should, in future, be practically identical. The meeting also decided that military hospital carriers should be provided in future on the same lines as the naval vessels of the kind. The main differences that remained were that a dental department and a steam laundry were confined to naval ships, only a hand-laundry being provided in the others.

THE USES OF HOSPITAL SHIPS IN PEACE TIME.

The only vessels of the sort which have been utilized in time of peace are the Naval Hospital Ship *Maine* in our own Service and a few similar craft in other navies, such as the *Solace* and *Relief* in the United States Service. Their main uses have to some extent varied. Not infrequently, for example, the *Maine* has accompanied fleets and squadrons for cruises, and the *Solace* accompanied the American fleet around the world. On the other hand, for a long time, while Mediterranean fever held sway, the *Maine* was chiefly concerned with the transport of invalided men to England, whilst at other times she acted as a base hospital for destroyers in Sliema harbour.

Whatever the particular duty the *Maine* has been engaged upon, she has, when attached to a fleet or squadron, provided, in addition to the ordinary medical and surgical work, accommodation for isolation and equipment for disinfection of bedding and effects, together with laboratory facilities for the diagnosis of infectious disease. Of late years, too, she or her successor has provided for the dental work of all or part of the Fleet, X-ray, throat, nose, ear and eye examinations for the smaller ships, for which also the treatment of venereal disease has been undertaken, while, finally, her mortuary has been available in cases where inquiry is necessary. All this is leaving out of account the value of the extra training-ground she has provided for medical officers and sick-berth staff.

HOSPITAL SHIPS IN WAR-TIME.

Most of the objects to which reference has just been made become of even greater importance in time of war, and this especially applies to measures such as isolation, disinfection and laboratory work in connexion with zymotic disease, which tend to preserve the health of the personnel, and also to those which minimize the effects of disease, such as the continuous treatment of venereal ailments. Needless to say, it is a vital point in war to keep the personnel as physically fit as possible.

The one outstanding difference, however, between the two forms of hospital ship is that the war-time hospital ship must always be prepared for admissions or discharges in mass. It is not meant that this never occurs in peace-time, but admission or discharge in mass is then decidedly exceptional, whereas in war it is one of the main objects for which the ship is employed, and speed in dealing with the emergency is frequently of the first importance.

For example, at Gallipoli the naval and military hospital ships admitted literally thousands of cases in a comparatively short time and these crowds of cases usually came in rushes. Up to the time of the evacuation the *Rewa* had dealt with over 20,000 cases and another naval ship, the *Soudan*, within fourteen days received over 2,300 wounded, rescued and cared for 440 survivors from a wrecked ship, and, finally, removed 106 sick and wounded from a ship with her coal bunkers on fire.

Naval engagements of any magnitude were infrequent, but after the battle of Jutland the *Plassy* received at one time 192 cases, many suffering from severe wounds and scalds, and, as the result of the blowing up of the *Natal*, 131 survivors were received on board the *Drina*.

Now in order to deal effectively with such occurrences as these, it is not only necessary to considerably augment the peace-time medical and nursing staff, a measure which is provided for by the instructions, but it is also essential carefully to organize beforehand the routine for the embarkation and disembarkation of the incapacitated with their effects, and for the provision of the necessary documents which must accompany them. It is also necessary to prepare an organization for providing emergency beds or billets for occasions such as the rescue of a shipwrecked crew, when the ordinary accommodation is entirely occupied.

THE ADMIRALTY SCHEME FOR THE ORGANIZATION OF HOSPITAL SHIP ASSISTANCE IN WAR.

This was designed after the South African War by the Transport Department, who were responsible for the military as well as the naval vessels. It was put into action during the Great War, and the whole organization worked with distinguished success.

Briefly, it consisted in the compilation of instructions which specified in detail the various structural alterations and additions required, while the necessary fittings, equipment, stores, and so on were constantly kept ready for use at any moment. To facilitate the taking up of several vessels plans were made of such as would serve as types, and this planning was kept up to date.

For immediate requirements, what were known as hospital carriers, capable of being sent to sea in forty-eight hours, which would embark artisans to continue the work after sailing, were provided for the Navy, and these were replaced after three or four weeks by the more elaborately fitted hospital ships proper. These carriers will, in future, be provided for military purposes also if required. They are essentially stop-gaps, and were taken over

"all standing," being provided with a specified equipment, kept in a central depot to minimize delays in supplying it wherever required.

POLICY GOVERNING THE EMPLOYMENT OF HOSPITAL SHIPS.

With regard to the military ships this is simple enough, as they merely act either as hospital transports, as at Gallipoli and during the South African War, or as base hospitals, as in some of our minor expeditions abroad. With the naval vessels it is, however, somewhat different, and it was pretty generally realized, even before the Great War, that they would operate at a considerable distance from the scene of hostilities. Their comparatively low speed and consequent risk of hampering the Fleet or revealing its presence, the development of heavy gun, torpedo and mine, together with the possibilities of wireless communication, all argued in favour of this probability.

Their main spheres of usefulness were considered to be the preliminary clearing of the ships and nearest hospitals preceding hostilities, and, naturally, after action, the reception of the injured and incapacitated. In order to perform this latter function it was thought that they would probably lie at some neighbouring base till after action, when the Commander-in-Chief might call them up to the Fleet in order to avoid the detachment of any of his fighting units. As a matter of fact this never occurred, and the wounded were always conveyed to them at the base, although it is possible that if the Germans had risked a general action early in the war when the Fleets were more evenly matched, there might have been another story. On the whole, however, even this seems improbable on account of the fact that the area of hostilities was so limited that the enemy's coast line was never more than twenty-four hours' steaming from the base, and with the enormous numbers engaged in this narrow area and their high speed the hospital ship would almost certainly hamper operations. Moreover, in modern naval warfare the fighting ships exhaust their fuel and munitions so rapidly that a return for replenishment is soon necessary, as at Jutland, and their speed is so much greater than that of the hospital ship that retention of the wounded on board is likely to be preferred. But, in addition to this, the indiscriminate sowing of mines and the torpedoing of ships by submarines practically eliminated the possibility of any trans-shipment except in a protected area.

Probably these considerations will operate in any future war, as, except for the possibility of operations in a less confined area and an international limitation of the uses of mine and torpedo, the deterrent influences are likely to increase rather than to diminish. In this connexion the great developments in aerial warfare may be referred to. But the comparatively limited experience of the *Plassy* and similar craft has also shown that they are not at all well adapted for the *prolonged* treatment of the scalds and severe injuries resulting from a naval action, owing to the lack of cubic space, limitations of ship ventilation, and difficulties of moving and manipulating patients.

It follows that their chief functions after a naval action will probably be those of acting as a casualty clearing station, of giving the severely shocked time to recover before being moved, of dealing with emergency cases, of weeding out all capable of movement, and finally of acting as base hospitals for the remnant whose condition compels their retention. This is leaving out of account the possibility of a hospital ship accompanying a fleet during a prolonged cruise, similar to that of *Rojdesvensky* to the Far East. The occurrences of this war do not negative such a possibility, but the chances are perhaps against it. It is noteworthy that no such vessel accompanied Admiral Sturdee's squadron, probably on account of the speed and speeded

required, although here he was proceeding to a part of the world ill-supplied with hospitals, and in search of the notorious German gunnery fleet. The prospects of a heavy casualty list were by no means inconsiderable.

RULES GOVERNING THE EMPLOYMENT OF HOSPITAL SHIPS.

These are laid down in the articles of the Geneva Convention which were signed and ratified by all the representative powers at the Hague Convention in 1899. This Convention was later amended by the Hague Conference of October, 1907, and was signed by the representatives of thirty-two states; but this amendment has never been ratified by the British Government. The British Government, however, at the beginning of the recent war, declared that it would abide by the Hague Conventions if enemy Governments would do the same, and there the matter has rested.

The Articles of the Amended Convention deal mainly with the immunity of those hospital ships and their personnel which have been duly notified to the belligerents, with the distinguishing marks which should be used to identify them, and with rules prohibiting their use for any combatant purpose.

For gross breaches of the Convention the present clauses are sufficiently clear. Thus, during the late war, the German hospital ship, *Ophelia*, was captured in 1915, and condemned by the London Prize Court. It was held that her equipment was much better adapted for acting as a signalling ship than for her proper purpose and that, in fact, she had never attempted to render service to the sick and wounded, whereas she was under grave suspicion of having served as a signalling unit.

Nevertheless, there is a very general agreement that certain clauses present ambiguities and require very much more precise definition. But the elaboration of a formula to which all parties can subscribe and which is sufficiently binding to prevent any wriggling out of its provisions is a matter that is beset with difficulties. These remarks apply more particularly to the articles which specify the classes of material and personnel which it is permissible to convey in these vessels in war-time.

During the war the right of search was seldom exercised: by the Germans because their surface craft had few opportunities and their submarines probably preferred not to betray their presence unnecessarily, and by the Allies because enemy hospital ships were seldom far from their own bases. In a future war, however, things might be quite different and the present lack of clarity on these points would very likely lead to disputes as to the meaning of various articles. To an unscrupulous enemy, too, it offers opportunities of profiting by these ambiguities. This was illustrated when the Germans made allegations of the conveyance of combatants in hospital ships a pretext for sinking them without warning.

A first step towards remedying this state of affairs was taken by a Sub-committee of the Admiralty Reconstruction Committee in 1919. Article 4 lays down that hospital ships shall afford "relief and assistance to the wounded, sick and shipwrecked of the belligerents." In the opinion of the Sub-committee there could be no difficulty about the interpretation of the word "shipwrecked" but the case was different with regard to the "sick and wounded," owing to the absence of any satisfactory line of demarcation between those that are, and those that are not, entitled to the benefits of the Convention.

They proposed the following definition of the sick and wounded persons who are thus entitled :—

- (a) Officers and men of the naval, military, or air forces.

(b) Persons (male and female) employed by the Government in connexion with the fighting services. (This definition they quite recognized was lacking in absolute precision.)

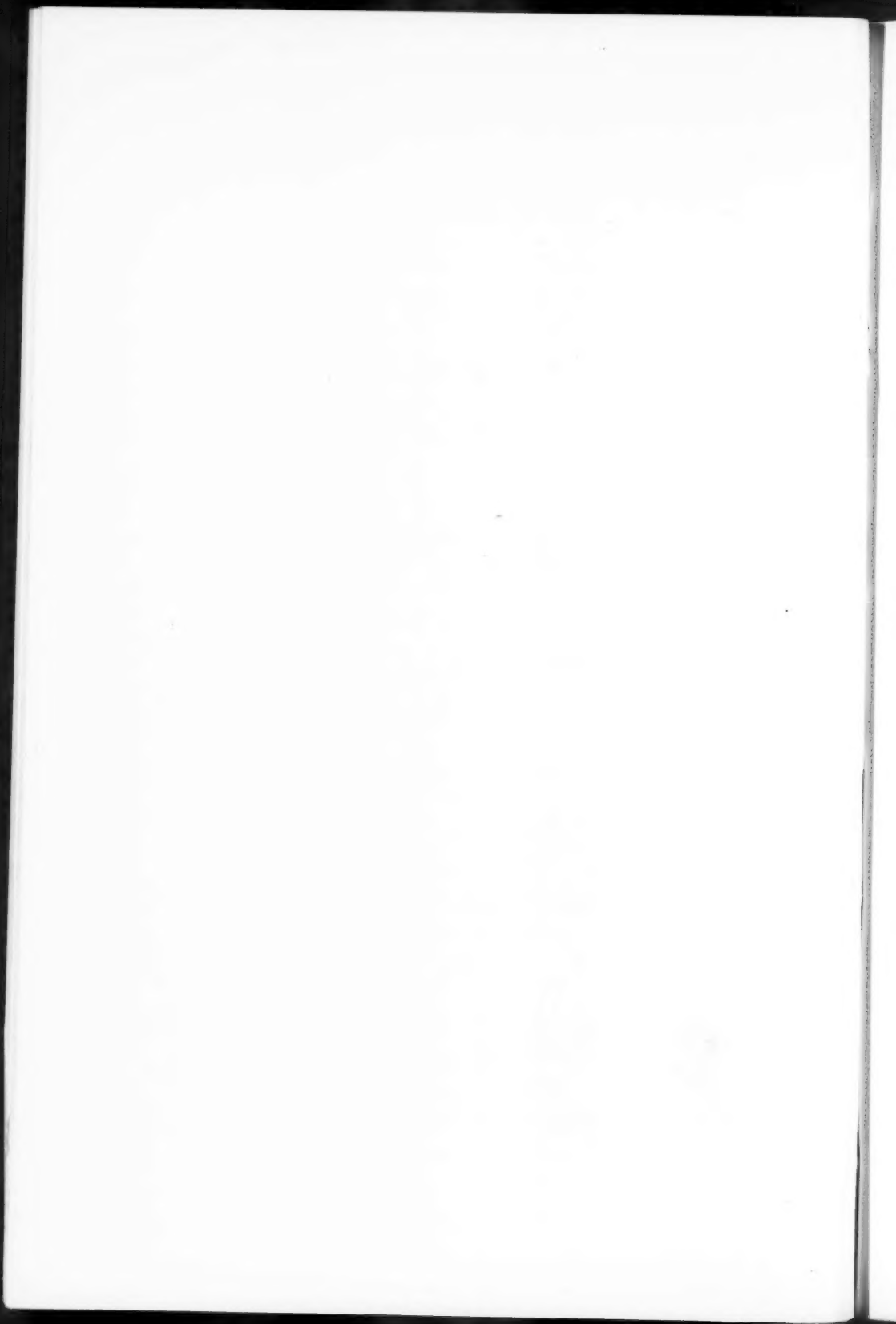
(c) Officers and men of the mercantile marine.

(d) Personnel belonging to the Red Cross Organization.

With regard to the carriage of healthy medical personnel and surplus stores, other than those required for the ships in which they were embarked, it was considered that on the whole it was better that this should be entirely prohibited. Here humanitarian considerations conflict with the military requirements of blockade. For instance, in the late war, rubber, cotton, and fats thus carried might have been turned to military uses by the enemy, and although the Foreign Office agreed to pass medical stores to American Red Cross Units in enemy countries, this was coupled with an insistence on the rationing of fats and rubber and the destruction of all old stores. Even this concession was opposed by naval and military authorities.

A final point dealt with the German interpretation of the article which gives belligerents the right to order hospital ships to take a certain course. The Germans made use of this to issue a notice, warning off all such vessels from a zone in the Channel and North Sea, and the Committee considered that it should be made quite clear that this clause can only be exercised by officers of the belligerent forces who actually intercept the hospital ship.

Here the whole of these matters remain at the moment.



War Section.

President—Surgeon Vice-Admiral JOSEPH CHAMBERS, C.B., C.M.G., R.N.

Remarks on the Incidence, *Æ*tiology and Prevention of Rheumatic Fever in the Navy.

By Surgeon Commander THOMAS BROWN SHAW, R.N.,

R.N. College, Greenwich.

RHEUMATIC fever, which causes such a large amount of disability not only in the Services but in the civil community, is one of those diseases, gradually becoming fewer in number, the *æ*tiology of which still remains obscure. Some time ago, when considering further measures for the prevention of diseases in the Navy, I was struck by the sudden fall in the incidence of this disease which occurred in 1921, and this short paper, based on some observations which were made to try to elucidate the cause of this fall, and so ascertain definitely the methods for prevention which we should use, was written in the hope that it would lead to a useful discussion, and also that other medical officers in close touch with cases of the disease in our hospitals might make further investigations as to its *æ*tiology.

In the statistical reports of the Health of the Navy up to and including the year 1906, cases of rheumatic fever were entered under "rheumatism," a heading which was used in the widest sense and included also chronic affections of the joints and many indefinite cases of muscular rheumatism. From 1907 onwards the disease is shown under a separate heading. In that year the case ratio (number of cases per 1,000 men) was 4.77. The year following the incidence was 6.5. This apparent rise in incidence was probably due to the fact that with the introduction of the new nomenclature in 1907 all cases of the disease were not accurately recorded as such, and this assumption is borne out by the fact that during the following years the incidence, although showing slight changes, remained at a fairly constant level until 1921. Table I¹ shows that the incidence of the disease increased slightly up to and including the year 1910, after which there is a very gradual fall until 1915. But the most striking fact in this table is the marked fall in the incidence of the disease in 1921. In 1915 the number of cases per 1,000 men was 6.3; in 1921 it was 2.28.

When this marked fall in the incidence of rheumatic fever was noted, the incidence of chronic rheumatic diseases for several years was examined in order to see whether these affections also were diminishing in the same manner. In the Health of the Navy Reports cases of chronic rheumatism are entered under "Diseases of the Organs of Locomotion," and as the large majority of these diseases are made up from year to year by affections of a rheumatic nature, such as lumbago and muscular, fascial and joint rheumatism, the incidence for all diseases in the group is a useful though not accurate guide to the incidence of chronic rheumatism. The incidence of diseases of the organs of locomotion during the years 1911 to 1921 is shown in Table II. The average incidence from 1911 to 1915 was 14.69 with a tendency to rise

¹ Tables I, II and IV are not reproduced.

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rather than fall. In 1915 the incidence was 20·75; in 1921 it was 9·32, representing a reduction of 54·9 per cent. We see, therefore, that with the fall in the rheumatic fever incidence of 63·8 per cent. we have during the same period a corresponding fall in chronic rheumatic infections of 54·9 per cent. The increase in incidence in 1915 as compared with 1914 is probably due to the large increase in the proportion of older men serving in the Navy during the war period.

As has been indicated, the information available in the Health of the Navy is not sufficient to separate accurately chronic rheumatism from various other affections entered under diseases of organs of locomotion; but that the fall in incidence of rheumatic infections is a real one is shown further by noting the prevalence of diseases of muscles, fasciæ, tendons and bursæ, which form one group under the general heading and of which practically all are cases of lumbago and muscular rheumatism. The case ratio per 1,000 for this group for 1914, 1915 and 1921 were respectively 9·7, 15·0 and 5·2.

Rheumatic fever is met with on all stations, but as will be seen in Table III, in which the average case ratios per 1,000 men on the various stations for the years 1909-15 are given, the incidence is highest on the Home station. Those stations such as the Mediterranean, China and East Indies, where the climate generally is equable and warm, show the lowest incidence. Next to the Home station the disease is most frequent on the African station, and that this should be the case is readily understood when we consider that the greater number of days' service on this station are spent at the Cape, where we have a sub-tropical summer followed by a fairly severe winter, and rheumatic diseases are comparatively frequent amongst the shore population.

TABLE III.—RHEUMATIC FEVER. *Average case ratio per 1,000 on the stations noted between the years 1909-1915.*

Home Station	10·8
Home Fleet	6·0
Mediterranean	5·4
North American West Indies	7·4
China	4·0
East Indies	5·1
Africa	8·8

The greater prevalence of the disease on the Home station bears out the belief which is commonly held that rheumatic fever is more common in the British Isles than elsewhere. But in this connexion another factor has to be taken into consideration. The returns from the Home station include those from the boys' and youths' training establishments, which contain a large number of subjects apparently especially susceptible to the disease. When serving in charge of one of the medical sections recently at the R.N. Hospital, Plymouth, I was impressed by the large number of cases of rheumatic fever received from the training establishments of that port; and the figures for the year 1921 show that of the 139 cases of the disease returned from the Home station, sixty-five, or nearly 50 per cent., were returned by two of these training establishments.

Rheumatic fever is important not so much from the actual number of cases and loss in days of sickness to the Service which it entails, but from the heart lesions caused by it and the final invalidings which result. The disease is believed by some to be due to a streptococcus, by others to a micrococcus; but although there can be little doubt that it is caused by infection, the causal organism has so far not been definitely determined, and our preventive measures are directed largely to the protection of the body against dampness and cold.

The relationship of rheumatic fever to endocarditis is too well known to need discussion. In a large percentage of cases the heart is affected, and the prevention of permanent damage to the valves is our most important problem in the treatment of the disease. In Table IV the incidence of rheumatic fever is compared with the

numbers invalided out of the Service with organic disease of the heart (M.C.O.) during corresponding years. The table shows that, generally speaking, the figures run on parallel lines; a rise in the incidence of rheumatic fever is accompanied by an increase in the number invalided with M.C.O., and vice versa, but we do not see the same striking fall in incidence of the latter between 1915 and 1921 which we find in the case of rheumatic fever.

There can be no question that the virus of rheumatic fever may affect the body and cause endocarditis without producing the signs and symptoms of the disease which we recognize clinically as rheumatic fever. In this connexion also we have to bear in mind those indefinite muscular and joint pains in children and adolescents which we have been accustomed hitherto to regard as "growing pains," and as having no very definite significance. Further investigation may show that these symptoms are frequently an indication of a mild form of rheumatic affection, and as such will require greater attention.

Some connexion between rheumatic fever and tonsillitis and sore throat has long been suspected. Many clinicians believe that a large proportion of these latter affections are "rheumatic" in origin, and it is customary now when investigating heart cases to inquire not only as to a previous history of rheumatic fever but also of sore throat and tonsillitis. Mouth infection generally is probably an important link in the chain of rheumatic infection, and in our measures for the prevention of the disease, this, as we shall see, must be borne in mind. Table V shows the incidence of sore throat and tonsillitis in the Navy during the years 1907-21.

TABLE V.

Rheumatic fever					Tonsillitis and sore throat				
Case ratio per 1,000					Case ratio per 1,000				
1907	4.7	46
1908	6.5	48
1909	7.2	49
1910	8.5	50
1911	7.6	54
1912	7.5	57
1913	7.0	55
1914	6.1	47
1915	6.3	46
1921	2.2	40

It will be noted that during the years 1909 to 1913, which show the highest incidence in rheumatic fever, we also find the highest incidence in cases of sore throat and tonsillitis; but there is not the same marked fall in incidence of the latter diseases between 1915 and 1921.

If we analyse the figures further and separate cases of tonsillitis from those of sore throat we obtain the figures which are given in Table VI.

TABLE VI.

Diseases of the teeth and gums, mouth, palate, fauces, and pharynx.					Tonsillitis.				
Case ratio per 1,000					Case ratio per 1,000				
Year
1913	23.3	37.3
1914	18.7	33.7
1915	23.3	30.6
1921	10.5	33.7

In this table the figures in the first column include not only sore throat (inflammation of the palate, fauces and pharynx) but also diseases of the mouth, teeth, and gums, and they bring out the striking fact that whilst, accompanying the marked drop in the incidence of rheumatic fever which we have noted in 1921 we have a sudden fall in diseases of the teeth and gums and sore throat, the incidence of tonsillitis during the same period has actually increased. Although one must be careful in drawing inferences from the figures for one year only, the important fact remains that with a fall in the incidence of rheumatic fever between 1915 and 1921

of 63·8 per cent. we have during the same period a fall in the incidence of "mouth diseases" of 56 per cent., and one may be justified in hazarding the suggestion that mouth infections have an important bearing in the causation of the former disease. In the Navy during recent years increasing attention has been paid to oral hygiene. The care of the mouth and teeth has been emphasized in health lectures, and the men now seek the dental treatment which formerly was only carried out after much persuasion on the part of the medical officer. Facilities for dental treatment have also increased and the result is seen in the marked fall in the number of cases of mouth infection quoted above.

Infection of the tonsils has been so frequently noted as a factor in the aetiology of rheumatic fever that one hesitates to draw any conclusions from our figures until the incidences of the two diseases are noted further in future years; we have seen, however, that with the steep fall in the incidence of rheumatic fever in 1921 we find no reduction in the incidence of tonsillitis but an actual increase in the disease. Has the diminution in the disease which we recognize clinically as rheumatic fever been reflected partly in an increased incidence in rheumatic tonsillitis?

In a report on the prevalence of rheumatic diseases amongst insured persons in England and Wales during the year 1921, issued by the Ministry of Health, support is given to the view that tonsillar sepsis is an important aetiological factor in the incidence of acute rheumatism. Of patients with acute rheumatism enlarged or septic tonsils occurred in 50 per cent. and in only 2 per cent. of the cases were the tonsils removed. From these findings it would appear that together with a rise in the incidence of cases of infected tonsils we should also find a rise in the incidence of rheumatic fever. But as we have seen, the Naval statistics indicate that the incidence of rheumatic fever is much more influenced by mouth infection than by tonsillitis. As some of the medical officers present may not have had an opportunity of reading the Ministry of Health report referred to above, a few of the more important points contained in it which bear on the subject which we have been considering may be usefully mentioned: Nearly one-sixth of the industrial invalidity of this country is due to diseases classed as "rheumatic," the term being used in its widest sense to include: (1) acute rheumatism (rheumatic fever) and sub-acute rheumatism, (2) muscular rheumatism, lumbago, and fibrositis, (3) diseases characterized by chronic joint changes. The general attack rate for "rheumatism" for the year under consideration (1922) was 27·6 per 1,000 insured persons of both sexes. Amongst the males alone the incidence was 30·5. In group (1) corresponding to our entries in the Navy for rheumatic fever, the incidence per 1,000 insured males was about four. Fifty per cent. of the cases in this group had enlarged or septic tonsils; and 75 per cent. of all patients with "rheumatism" over 20 years of age suffered from dental sepsis in some form or other. The conclusion arrived at is that dental sepsis, whilst by no means the only causal factor, is a factor of importance in the causation of those forms of rheumatism included in group (2). The opinion is expressed that the traditional association of dampness of houses with all varieties of rheumatism probably rests on a substantial basis. It will be noted that the incidence of rheumatic fever amongst the insured male population on shore during 1922 is considerably less than the average incidence of the disease in the Navy during the years 1909 to 1915 but is much greater than the incidence in the Navy in 1921.

PREVENTION.

Although in dealing with rheumatic fever we are handicapped by the fact that the causal agent of the disease has not yet been determined, it is evident, I think, that oral hygiene must play an important part in its prevention. We must also pay due attention to environmental conditions such as dampness of living spaces, exposures to sudden changes of temperature and so on, which probably act as

predisposing causes. In the days of our old wooden battleships when artificial ventilation had not been thought of and leakages with accumulation of bilge water in the ships were of common occurrence, dampness was a serious problem, as we realize also when we read of the various methods employed by the medical officer of the time to alleviate it. In our modern ships the problem is much simpler. Art. 554, K. R. & A. I., which deals with cleanliness and ventilation, directs that the captain is to use his best endeavours to keep all compartments of the ship dry. Dampness and excessive air humidity can be readily prevented provided full use is made of the means for ventilation which are now provided and care is taken to clean mess decks and spaces between decks generally, not by flooding with large amounts of water but by washing down with a cloth and soap and a bucket of water as is done in living spaces on shore.

Exposures to sudden changes in temperature can be largely prevented by regulating the temperature of the mess-decks. During the winter months in home waters the temperature of the air in living spaces should not be allowed to exceed 62° F. If care is not taken the mess-decks in winter, crowded with men and heated by air which has passed through steam heaters, will soon become oppressively close and warm; with the result that liability to chills will be much increased when the occupants go on deck and are exposed to the cold external air. The heating of the air therefore supplied to living spaces must be properly supervised and high air temperature prevented by suitable adjustment of the regulating levers fitted to the steam heaters. In this connexion we may note that in certain occupations where the workers are exposed to marked changes of temperature, as in foundries, the incidence of rheumatic fever is high.

Chills must also be guarded against by the use of clothes suited to the season and the work which is being done. Men must be properly protected against cold winds and rain, and they should be taught in health lectures the dangers of wearing damp clothes. Drying rooms are now available in many of our ships and are an important asset in the prevention of chills. In this connexion, too, we may note amongst general preventive measures "the airing of bedding," a most important hygienic measure, especially on board ship, where the blankets and mattresses lashed up in hammocks are stowed away immediately after use. Under such conditions a certain degree of dampness in the bedding is inevitable. The dangers of wet clothes and the importance of airing the bedding are very rigidly emphasized and provided for in the K. R. & A. I.: Art. 552 directs that the captain is to see that (1) men are never allowed to remain in wet clothes nor sleep in wet bedding when it can possibly be avoided; (2) that the bedding is aired once a week when the weather will permit it, each article being exposed separately to the air. The latter is a most important hygienic measure to remember on board ship not only in the prevention of rheumatism but in the prevention of infectious diseases generally.

We have noted the importance of oral hygiene in the prevention of rheumatic fever; all sources of infection from the mouth must be carefully guarded against. As already stated, the proper care of the teeth is emphasized in health lectures, and medical officers should take every opportunity of inspecting the mouths of the men under their care to see that their instructions are being intelligently carried out. Every rating on board must have a toothbrush and we must see that it is used. Lectures on oral hygiene are given now by the naval dental officers attached to naval depots, and all new entries are required to attend these lectures as part of their general instruction.

As it would appear to be established that "rheumatic" infection of the tonsils may occur, every effort should be made, especially in the case of boys and youths in the training establishments, to maintain the tonsils in a healthy condition by frequent medical inspections; chronically enlarged tonsils which have resisted treatment should be removed, and septic foci in others dealt with as required. Sore

throat or infections of the palate and fauces must also receive suitable treatment and the mouth cavity generally maintained as far as possible in a healthy condition.

In the case of boys and youths I think we should treat affections of the throat and tonsils more seriously, and in cases where inflammation and pain are present, whether there is a rise of temperature or not, the patient should be put in bed instead of being placed on the attending list, and given a gargle or mouth-wash until the acute signs subside. Boys and youths, also, who come to us complaining of symptoms suggesting growing pains, should not be considered too lightly; such cases should certainly be excused all drills and exercises, and in view of the fact that the symptoms may be rheumatic in origin, we should, perhaps, be acting in the best interests of the patients by giving such cases complete rest in bed, and so perhaps prevent rheumatic infection of the heart.

As we have seen there can be no doubt that chill is a most important factor in the causation of rheumatic fever, and, in the case of boys and youths especially, we must guard against the occurrence of such on the lines already suggested, and which are evident, by every means in our power.

DISCUSSION.

Colonel KENNEDY, in discussing the paper from the Army Medical point of view, said he regretted that, owing to short notice, he had not been able to make as detailed a statistical investigation as he could have wished. His figures were abstracted from the Army Medical Department Reports of 1913 and 1921, the latter being the only one so far published since the war. The available figures for 1914 dealt only with the first seven months of that year. He was unable, therefore, to give a complete set of figures for the Army for comparison with those for the Navy given by Surgeon Commander Shaw in his excellent paper. His remarks would therefore be limited to rheumatic fever, tonsillitis and valvular disease of the heart.

It was a striking fact that the incidence of rheumatic fever in the Army showed a definite fall in 1921 comparable to that noted in the Navy. The figures for the whole Army at home and abroad were:—

For the quinquennial period 1908-12	5.9 per 1,000 strength
In 1913	4.3 "
In 1921	2.1 "

This decline in incidence became more manifest when the figures for the United Kingdom alone were taken, e.g.:—

In 1913	5.0 per 1,000 strength
In 1921	1.6 "

The comparison was still further borne out by the figures for tonsillitis. The incidence for this disease in the whole Army at home and abroad was:—

For the period 1908-12	23.4 per 1,000 strength
1913	22.7 "
1921	32.1 "

Commenting on the suggested explanation of the decline in rheumatic fever as being the result of oral hygiene, Colonel Kennedy said he was unable to reconcile the deduction that oral sepsis was an aetiological factor of importance in the incidence of rheumatic fever with the well-known prevalence of the disease in childhood, and proceeded to give further analysis of figures which brought out other interesting points in epidemiology.

The incidence of rheumatic fever in the United Kingdom compared with the stations abroad, omitting places with small garrisons:—

In 1913 South Africa	...	7.3	In 1921 Rhine and Silesia	...	3.6
United Kingdom	...	5.0	India	...	3.1
India	...	3.8	Egypt	...	1.9
Egypt	...	2.8	Iraq	...	1.9
			United Kingdom	...	1.6
			Constantinople	...	1.0

During the period 1904-7 the incidence in South Africa was two to three times higher than that prevailing in the United Kingdom at that time; in Egypt it was just a little higher, and was still relatively so; and in India it was as low as 0.8 per 1,000 as compared with 3.8 and 3.1 in 1913 and 1921 respectively.

These figures threw some doubt on the ætiological significance of dampness, and favoured the opinion held by some, that bright sunshine, high temperature and easterly winds, were important factors, though when the disease had once been established its persistence or relapse was favoured by humidity.

It was pointed out that in assessing the significance of figures for any one year the variability of the disease from year to year must be remembered, and the fact that it tended to appear in modified epidemic form every three or four years.

The next series of figures compared the incidence in the various Commands at home for the year 1921 :—

London District	5.4 per 1,000
Eastern Command	2.4 "
Southern	1.7 "
Scottish	1.4 "
Aldershot	1.2 "
Irish	1.0 "
Western	1.0 "
Northern	0.8 "
Channel Islands	Nil

The high incidence in the London district was a striking feature, and was difficult to account for, except by recognizing the infectious nature of the disease, and that it bore relation to the well-known conditions under which troops in the London district were accommodated. It was possible that this high incidence in London had some relation to the facts that had been brought out by a study of valvular disease of the heart. Granted that the major cause of valvular disease of the heart in childhood was rheumatic fever, a census of children suffering from the former would give an approximate figure for the incidence of the latter disease. The Board of Education figures for 1912 for the whole country showed that 10.9 girls and 8.2 boys per 1,000 had valvular disease of the heart, while a series of London school children examined by Langmead gave the high figure of 29.3 per 1,000.

The figures for rejection of recruits could be applied in the same way, and brought out interesting points. For the decade 1901-10 the annual number of recruits rejected for valvular disease of the heart varied from 7.3 to 10.9 per 1,000—average 9—closely approximating the Board of Education figures quoted above. Incidentally the rejections rose in 1912-13 to 12.89 and 1920-21 to 14.46 per 1,000. By analysing the recruiting figures for the years 1901-10 it was shown that the rejections varied very considerably in different districts, e.g., lowest, 1 per 1,000, in the Channel Islands; highest, 32.4 per 1,000 in the East Riding of Yorkshire; and it was evident that valvular disease of the heart (and presumably rheumatic fever) tended to be more prevalent in towns, and especially in industrial towns.

Colonel Kennedy, for lack of time, was unable to enlarge on these points, but he said their significance was obvious, and he closed with a reference to the work of the late Colonel W. S. Harrison, from which he had quoted, and urged that his observations—so unfortunately curtailed—in the bacteriology of rheumatic fever, should be followed up.

Squadron-Leader E. W. CRAIG (Royal Air Force) quoted figures which showed that the case incidence rate of acute rheumatism in the Royal Air Force had remained practically unchanged during the years 1921, 1922 and 1923. The annual attack rate during the period was 0.9 per 1,000 of strength; this figure closely approximated to the rate found amongst the insured male population by the Ministry of Health in their recent investigation. The figure also closely agreed with statistics from Germany and Norway. During the same period, the case incidence of both tonsillitis and diseases of the teeth and mouth showed a progressive decrease in the Royal Air Force.

As regards dental sepsis as a causal factor, he drew attention to two significant findings in the Ministry of Health report: (1) That dental sepsis appeared to be very frequently associated with all rheumatic diseases except acute rheumatism, and (2) that in acute rheumatism, records showed a noteworthy absence of dental sepsis in those cases in which, in the absence of tonsillar sepsis or its history, search was made for some alternative focus of infection. He suggested that as regards (1) the figures only showed that dental sepsis was relatively less frequently found in association with acute rheumatism than with other rheumatic conditions.

Chills, dampness of dwellings and oral sepsis were recognized as causal factors and were all preventable. He emphasized the importance of: (1) The avoidance of septic conditions of the mouth which arose from mouth-breathing, enlarged tonsils and adenoids, stomatitis,

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inefficient mastication and the presence of constitutional disease which predisposed to the bad formation of teeth. (2) The early detection and prompt conservative treatment of decayed teeth. (3) The practice of cleanliness of the mouth by the proper use of the toothbrush.

He quoted figures to show that amongst recruits for the R.A.F. the proportion of "dirty" mouths was high, lower amongst men under training, and lowest amongst fully trained men. Once a man was taught the necessity and importance of using a toothbrush he paid far more attention to the cleanliness of his mouth. He drew attention to the well-known fact that the risk of endocarditis was very greatly increased in subsequent attacks of acute rheumatism, and he emphasized the necessity of rest and prolonged convalescence during treatment.

Surgeon Commander T. B. SHAW, R.N. (in reply) said that he should perhaps have mentioned at the conclusion of his paper that the statistics for the Health of the Navy for 1922 were now being worked out, and that he had been able to ascertain a few days ago that the incidence of rheumatic fever for the year was 1.95 per 1,000. The incidence of the disease in 1922, therefore, was even lower than in 1921, so that the cause or causes which brought about the reduction in this disease in 1921 were still at work in 1922.

Colonel Kennedy in his remarks, had given a very full account of the incidence of rheumatic fever in the Army, and the figures supplied by him and also by Squadron-Leader Craig, who had discussed the incidence of the disease in the Royal Air Force, should prove most useful in our endeavours to elucidate further the causes of this disabling disease. Of course it was impossible for him (Commander Shaw) to comment upon those figures without closer examination. Although rheumatic fever was now recognized as an infection, text-books on preventive medicine and hygiene gave very little information as to its prevention. This discussion had shown that in our measures for the prevention of the disease oral hygiene must play an important part.

The very conflicting opinions which existed regarding rheumatic fever, many of which were indicated by Colonel Kennedy, showed how greatly the subject needed discussion. The Army statistics brought out the interesting fact that the incidence of the disease was high in London and in large industrial towns. This was difficult to explain. In such areas we knew that the incidence of respiratory diseases and also of sore throat was high, and it was believed by some that rheumatic infection might be respiratory in origin, and that outbreaks of catarrh were often associated with an increased incidence in rheumatic fever. Although this point was not referred to in his (Commander Shaw's) paper it had been considered, and it was found that this supposed connexion of the disease with catarrh was not supported by the Naval statistics. A large percentage of the cases entered under the heading "Respiratory Diseases" was due to catarrh. The average incidence of this disease from 1913 to 1915 was 61 per 1,000. In 1921 the incidence was 68.24, so that with the fall in rheumatic fever in 1921 there was a rise in the incidence of catarrh.

War Section.

President—Surgeon Vice-Admiral JOSEPH CHAMBERS, C.B., C.M.G., R.N.

A Further Communication on the Treatment of Gonorrhœa by Kataphoresis.

By Major A. T. FROST, R.A.M.C.

(*Royal Herbert Hospital, Woolwich.*)

[Proof not reviewed by author.]

IN November, 1923,¹ I reported to this Section the trend of a line of research on a new arm in medicine for the treatment of gonorrhœa, that is, making use of the pressure of an electric current known as kataphoresis, to bring the gonococci, when deep in the tissues, within the sphere of action of colloidal antiseptics. This is in contradiction to the use of the rate of flow of a current of electricity to convey chemical antiseptics to the tissues.

The preliminary results were encouraging and were sufficiently definite to bring before you. At the first report some hundred cases were in hospital for an average of thirty days whilst treated by a negatively charged colloidal silver made electrically. It was anticipated that chemically prepared colloidal silver would act as well as the electrically made colloid. This expectation has been verified during the last year.

As practically no literature exists dealing with the use of kataphoresis in medicine, a difficult and unlighted path has been travelled during the course of work of the last year. Many side-tracks were traversed, but these only led us into further darkness.

Gradually the average period spent by patients under treatment for gonorrhœa in hospital increased till at length the time of stay in hospital approached fifty days for the ordinary case.

It was only in September of last year (1924) that the advice given by Sir William Leishman² at the meeting of this Section in November, 1923, could be carried out effectively. He advised the use of the microscope to elucidate the action of the treatment on the tissue cells and organisms. And we who have been working at the subject beg to acknowledge that any light which has come to us has been through following that advice.

The only change in the treatment up to the end of the year 1923 was the substitution of chemically made for electrically made silver colloid. In April, 1924, it was noted that the number of closed urethral follicles was more marked in those cases treated by kataphoresis than in cases in which this method of treatment had not been used. This condition was due to the use of a negatively charged colloid in contact with tissues which are themselves negatively charged; and by the driving of gonococci more deeply into the urethral mucous membrane the process of increased exudation and reaction in the follicles was set up. This was due to the necessary experimental variations which it was thought might improve the treatment. The colloid was changed by giving it a positive instead of a negative charge, so that the electric pressure would act towards the lumen of the urethra instead of driving organisms deeper into the tissues. This appeared to have been the means of reducing the number of closed follicles, but did not reduce the stay in hospital. At the same time an attempt was made to reduce the number of applications and also to diminish the time during which the colloid under electric pressure was applied to the urethra.

The method adopted by the middle of September, 1924, was that of applying a positive colloid for twenty minutes and of repeating it on four successive days. The results were not good. At this point a series of patients was taken and they were placed in parallel with a microscope, and the movement of the colloid which was being applied

¹ *Proceedings*, 1923-24, xvii (War Section), pp. 1-6.

² *Ibid.*, 1923-24, xvii (War Section), p. 6

to the patient was watched under dark-ground illumination, under exactly similar electrical conditions on each occasion. As the current acted on the colloidal silver it was soon seen that the action was not a simple one but the resultant of a number of physical and electrical phenomena.

One of the earliest of these phenomena to be noted was a deposit on the end of the positive electrode in the urethral nozzle. This was found to be a mixture of precipitated colloid, pus-cells, and gonococci. Next it was demonstrated that, after a period of time depending on the amount of current and voltage employed, the route of the colloidal particles which began at the positive pole within the nozzle and progressed towards the tissues of the urethra, as was intended, actually changed its direction, and that the stream of colloidal particles passed from the mucous membrane towards the positive pole and led to a deposit of cells and gonococci on the positive wire above-mentioned. By the addition of suitable colloidal indicators, such as phenol red, to the silver colloid, the change in the amount of concentration of H and OH set free in the colloid by the action of the current on the water in which the silver was suspended, was accurately measured.

Further information was obtained during the experiments with the use of 15 per cent. gelatin to represent the tissues. The silver colloid was precipitated on the surface or edge of the gelatin at definite concentrations of H and OH-ions in the fluid, and the particles were redispersed at a higher concentration of these ions, following the known physical laws of colloidal chemistry.

These experiments were carried out under dark-ground illumination with gelatin 15 per cent. to represent the urethra in section, and were done to ascertain whether it was possible to cause the colloidal silver particle to penetrate into the tissues. The results were both interesting and suggestive of the possibilities of this form of treatment, for the silver could be seen first to be deposited on the surface of the gelatin and then from this deposit a clear layer of undeposited colloid could be seen penetrating for a depth of two millimetres into the gelatin. It was carefully noted at the time what were the conditions both in the colloid, in the time, and in the amperage and voltage necessary to bring about these results.

Patients were put under as nearly identical conditions in the circuit as the experiment under the microscope, and the clinical results were noted. These experiments were carried on till the best results were obtained with the particular method under trial. It was considered that the colloidal metal used was of secondary importance compared with the conditions under which the electric pressure and current were applied, and with the acidity or alkalinity of the fluid in which the colloid was suspended.

The gonococcus was next put under test as to how it reacted under these conditions. It had been noted that a high degree of alkalinity resulted from the passage of the current through the urethra. The gonococci could be seen actually moving to the positive pole, which accounted for the concentration of cells and organisms on the tip of the wire in the urethral nozzle, and gonococci were seen dissolving in the fluid. Further, when the direction of the current was reversed and the movement was directed towards the surface of the gelatin, the organism could be traced to the surface of the gelatin. Similarly the pus-cells became swollen and lysed with their contained gonococci. The leucocytes became twice or treble their original size before breaking up, their polymorphic nuclei changing into a round or oval contracted mass at the same time.

Owing to the brief time at disposal I propose to omit any record of the experimental work and confirmatory tests of the observed phenomena, also of any detailed account of the stages which led up to our present position with regard to kataphoresis; but it is of interest to show some of the actual slides made during the early stages of the inquiry, which have formed the basis upon which the further work developed.

The last point in the scheme laid out for investigation in 1923 was as to whether immunity could be obtained during the disease by adding the toxins of the gonococcus to the colloid with the hope of getting them conveyed into the tissues when adsorbed to the colloid. In the gelatin experiments solutions of the gonococcus protein were definitely seen to penetrate and become precipitated by the oppositely charged ions in the substance of the gelatin. For a year it had been the custom of Major Lambkin, the officer-in-charge of the treatment of gonorrhœa at the Royal Herbert Hospital, Woolwich, to use a solution of gonococci in distilled water—250,000,000 in each c.c.—as a provocative test of cure. He finds that this method is the most active test, much more reliable than the injection of irritating chemicals such as silver nitrate or magnesium chloride.

At the laboratory various strengths of the proteids obtained from the gonococcus were used to protect the colloid, and also various fractions of the proteid in an attempt to find one with the least toxic and highest immunity value. The first proteid used was the endotoxin, ranging in strength from 160,000,000 to 30,000,000 organisms per c.c. This was excellent when used electrically as a provocative of pus and gonococci in an uncured case which had become dry. Other fractions tried were the alkali soluble fraction, the alcohol insoluble fraction, the mixture of the alkali soluble fractions of the gonococcus, staphylococcus, and of *Bacillus coli*, the two latter being added owing to the poor antigenic value of the gonococcus alone.

An increase of infiltration of the urethra was noted at this period (December, 1924, to January, 1925). This could be directly attributed to the introduction of the toxin into the urethra during the acute stage of the disease. There was also seen to be an increase in the number of cases suffering from posterior urethritis and epididymitis.

The method of application of the colloid and of the colloid protected with toxin was reviewed, and the whole method has been investigated with the aid of charts made for each case, showing the dose of toxin, type of toxin, time of application, and strength and pressure of the current used. These charts showed that in those cases in which the patients received the weaker dose of toxin, and underwent a shorter time of application, they did much better than those undergoing more active and stronger applications. The urethral infiltrations and closed follicles practically ceased and the urethra began to look normal after a few applications of the colloidal silver.

At the time of writing this paper the method of use was first to wash out the urethra with lime water of a strength of 0.2 per cent., and afterwards to remove salts with distilled water. Next, the bladder was filled with silver colloid of a strength of 1 in 32,000 silver, with N/300 NaOH added, equal to 0.13 gm. of NaOH per litre of colloid. Then the patient sits on a pad of copper gauze well wrapped in lint, the pad fitting into the hollow of the perineum, and the penis, up to the tip, is bandaged with a 1-in. bandage, the whole being soaked in water. The negative pole is attached to this pad by soldering the wire to the copper gauze. The positive pole is led into the colloid by means of a wire which ends just at the tip of the special flanged nozzle which carries the colloid to the urethra. Means are adopted in the circuit to vary the above polarity when required.

The colloid itself in the glass funnel consists of a 1 in 32,000 silver colloid, chemically made by means of placing in a litre measure 80 c.c. of a 1 per cent. solution of silver nitrate, and adding N/1 ammonia solution until just a faint haze is left. The litre is made up with triply distilled water. Another litre measure is filled with triply distilled water to which 100 c.c. of tannic acid is added of a strength of 1.3 gm. to the litre. The two are mixed with constant rotation of the bottles used. The colloid is made positive by means of positively charged H-ions—many methods of effecting this are available—which will not produce precipitation

of the colloid. Lastly, to the colloid a small quantity of gonococcal endotoxin is added, of an equivalent strength of half a million organisms per c.c.

The patient (whom we have left seated on the negative pad with his bladder filled with negative colloid) now inserts the nozzle into his meatus, and when his urethra is full of colloid, the current (100 volts direct current) is turned on and the voltage slowly increased up to 100 volts, and allowed to act for ten minutes. The current is then reversed and the direction of the colloid particles for the next ten minutes is from the inside of the urethra carrying in colloid and endotoxin towards the pad, through the tissues. In the first instance, for the first ten minutes, the negatively charged gonococci are attracted out of the tissues into the lumen of the urethra, and, as has been mentioned, can be recovered from the tip of the positive wire inside the nozzle.

This process is repeated for two or three days, and if successful the case dries up and the treatment is not repeated unless failure to clear up the case is indicated by continuance of gonococci in daily smears from the urethra. When the patient is reported as having a dry urethra for five or six days, the next stage of the treatment consists in the form of a test of cure. If the urethroscopic examination of the urethra shows it to be clear of inflammation, and the vesicles and prostate are found normal, and if the case is clear for six days after what is really producing a negative phase in the urethra—namely, the injection of endotoxin-protected colloid electrically applied—the case is declared cured. This test is carried out as follows: The preliminary preparation is the same as before, and the kataphoresis stage comes next. Then a small 10 c.c. funnel, with a similar electrical attachment to that used in kataphoresis, is filled with endotoxin-protected colloid, each c.c. of which contains the equivalent of twenty or thirty million gonococci. This is introduced into the urethra by means of the special flanged nozzle, and the current slowly applied. The present factors used have to be watched and not exceeded, otherwise too big a reaction may occur, especially if the case is not cured. If the case is cured no reaction occurs. The amperage should not exceed $\frac{2}{3}$ of a milliampere; the voltage should be kept below 20. The time of application which is found to be best is one and a half minutes, as application for a longer time than this is liable to produce more reaction, with infiltration in uncured cases, and delay in final cure.

The foregoing record is a brief account of the position of this research up to the present date.

Owing to the innumerable factors involved in this work, and the difficulty of judging by immediate observation on a few cases the effects of small changes in the method of application—for example, the difference of half a minute in applying the test of cure—it may take years of work to obtain what is essential, a fixed method of treatment. The basal fact with regard to the method consists in a definite control of the disease being possible in so far: that gonococci can be taken out of the tissues and ducts of the urethra; that the proteid of the gonococcus can be driven into the tissues, and probably, by analogy, deposited within the tissues as a precipitate.

In discussing the results, it is here to be emphasized that disease external to the urethral canal is at present outside the scope of kataphoresis. It is only in the acute cases and in those in which the patients suffer from chronic anterior urethritis that hope of success may be predicted. The figures might be divided into three groups for comparison. The first group comprises 107 cases treated without any knowledge of the principles which govern kataphoresis, with an average time of thirty days in hospital, reported to this Section in November, 1923. The results were good. But the conditions under which the colloid was made were constantly changing and the resultant colloid was variable in strength and in size of colloidal particle. It also tended to produce chronic folliculitis towards the beginning of the second period, and therefore it was abandoned.

The second period, from May to December, 1924, was a period of retrogression owing to the number of cases which failed to clear up under an average of fifty-three days in hospital. This period was one of frequent change in the method of application. The principal method consisted in the use of a positively instead of a negatively charged colloid. It is now recognized that the principal cause of failure was too long application of the current, which has been stopped in the third period. The number of cases treated in the second series was 191.

Since September 30, 156 cases have been treated up to date and the patients were 39'8 days in hospital. The first two months were devoted to experimental work, as has been briefly detailed. Definite lengthening of the period in hospital was necessitated by using for immunity the strong toxin-protected colloid which is now only used as a test of cure. These cases developed infiltrations which took a long time to clear up, and the method of treatment has not been continued. However within the last few weeks a modification has been tried by adding the equivalent of 500,000 gonococci per c.c. of silver colloid in treatment. It has been observed by this method, and first noted when using the stronger toxin-protected colloid, that on the fourth to the sixth day after treatment had been given, all pus-cells were degenerated and appeared to be lysed. As this was too long for any direct action of the treatment, it is suggested that an autolysin is developed in the tissues which is either a direct or indirect result of the introduction of the toxin by kataphoresis. This is shown by the charts, and is indicated by an interrupted line in the curve of epithelium to pus ratio. During this cell-disintegration no gonococci can be detected, but on the return of gonococci in the pus, the unchanged cells reappear, while in cured cases there is a decline in pus to the stage in which epithelial cells only were found. The cases under this period were thirty in number, and the patients have been in hospital 31'2 days for each case.

This paper is not intended to be a report of a completed method, but a report of progress with its findings, brought up to date, showing the possibilities of a new method. There are numerous facts which have yet to be elucidated, and not the least of these is the maximum dose of the various factors. Selected typical examples of the various results obtained are in your hands, each chart showing the factors used in the treatment.

It is suggested that the future line of work should consist in fixing the voltage, amperage, and time of application to their precise limits; in finding the best type of antigen and its dose, and, as part of this latter, in determining a method of growing the gonococcus so that a fixed strength of toxin may be used in this form of treatment, which is of much more importance than in the preparation of vaccines.

Control of the electric supply of direct current has been very little changed since the publication of my previous paper on this subject. The change has consisted in an attempt to render cheaper the means of regulating the current. The direct current of 106 volts is brought on to a board through a plug and a 5-ampere fuse to a switch, which allows the current to pass through a resistance to a potentiometer and back to the main. From each end of the potentiometer the current is tapped, so that the patient receives a gradient of voltage regulated from zero to 100 volts. A millimeter is fixed in this circuit to show the rate of flow. This is the simplest form of switchboard. It has been working, without giving any trouble, for the past six or eight months. All the wiring is on the front of the board and none at the back, so that there is no difficulty in tracing the circuit. Neither has the method of wiring the room for treatment been altered.

The test of cure by introducing gonotoxin-protected silver colloid electrically into the urethra requires no elaborate apparatus. All that is required is a 4-6-cell secondary battery with a milliammeter in the circuit, and the small funnel and rubber tubing ending in a nozzle in which there is a wire connexion to the positive pole of

the battery. The pad is connected to the negative pole. The current loss is inappreciable, and it is only 10 to 12 volts at $\frac{2}{5}$ of a milliampere for 90 seconds per case. This, as I have mentioned, only fails in complicated cases with infection of the prostate, or vesicles, or post-urethra. A vaccine test is combined with the urethral test to limit these failures. The relapses amounted to 16 per cent., however, in spite of the tests during the past year, in which an unusually high number of complicated cases occurred. But they should not occur from this time onwards.

In conclusion, I wish to acknowledge the careful and very laborious research work of Captain J. Lyn Dimond by means of which the biochemical and electrical facts were elucidated and repeatedly confirmed, and his research forms the subject of this paper. Also, I have to thank Major E. C. Lambkin, D.S.O., for his clinical insight and control of all the cases under this form of treatment, which kept it from straying too far into wrong paths of inquiry.

Discussion.—Surgeon Commander R. J. G. PARNELL said that in his opinion gonorrhœa remained a disease which, despite treatment, ran a variable course, might or might not be associated with metastatic complications, and often proved very obstinate in resolution. The time-factor in the public services, where gonorrhœa was largely treated in hospital, was all-important. The reduction of days in hospital referred to by Major Frost was highly commendable. He (Surgeon Commander Parnell) did not think the relapse percentage too high. He would like to know more about the use of gonococcal antigen in the test for cure, and as to whether kataphoresis had been used as abortive treatment in the earliest cases.

Captain F. CARMINOW DOBLE, R.A.M.C., raised the question as to whether this treatment had been tried in the case of women. Diathermy, the great rival of this new method, had recently had excellent results in the treatment of gonorrhœal infections of the cervix in women and of the prostate in men. It would be interesting to know whether the amount of penetration of the colloid particles had been worked out by experiment on raw meat, such as a beef-steak.

Squadron-Leader MONTGOMERY said that the treatment and cure of gonorrhœa had always been so unsatisfactory that any new method was much to be appreciated. In his later results Major Frost had reduced the number of days under treatment to twenty-one. This was a great advance. At the R.A.F. Hospital, Halton, the treatment was that of the old Rochester Row routine method of irrigations by potassium permanganate, and it had resulted in the last thirty-four cases in an average of fifty-five days under treatment. The number of relapses was not as high as twelve per cent. Would the number of relapses (sixteen per cent.) in Major Frost's series be accounted for by the fact that the colloid silver failed to penetrate the crypts and follicles, or the patches of infiltrated mucous membrane where the infection lurked?

Sir WILLIAM LEISHMAN asked (1) whether the use of an antiserum had been considered; (2) whether the effects of the possible destruction had been considered.

War Section.

President—Surgeon Vice-Admiral JOSEPH CHAMBERS, C.B., C.M.G., R.N.

The Treatment of Malaria by Novarsenobillon.

By Wing Commander HAROLD E. WHITTINGHAM, M.B.,
Ch.B.(Glasgow).

(R.A.F. Medical Service.)

QUININE has been used as a specific for malaria for the past hundred years. To mankind it has been a great blessing, and yet, perhaps, a curse; a blessing for its value in saving life, a curse because of the over-reliance placed on its power to prevent and cure malaria. By many writers its protective and curative powers have been so much vaunted as to make the medical profession and the laity believe that if a man swallowed enough quinine no harm would come to him in a malarial district. Such a belief has been the cause of needless waste of an expensive drug and of damage to the human body. It has also hindered the adoption of other more secure methods of preventing malaria.

There is no rapid cure for most of the cases of malaria that report to the medical man for treatment. The disease is already established, and the causative protozoon has a long life cycle to run. This cycle may be considerably shortened and the damage to the tissues of the host rendered minimal by the judicious administration of drugs. As yet we cannot lay claim to any one drug being a cure for malaria. The disease cures itself most quickly when the doctor acts most wisely, and that is not when he saturates the patient with quinine.

Quinine still remains the most valuable drug we possess in order to combat malaria, but it has several well-known defects. Most people do not like taking it, and, consequently, avoid doing so whenever possible. To watch every dose of the drug administered would entail a heavy task on the medical profession. It is a gastric irritant and only aggravates the vomiting so commonly associated with acute malarial attacks. In addition it sets up gastritis, and consequently it is liable to weaken the resistance of the alimentary tract to such infections as typhoid, cholera, and dysentery. Its action as a vaso-depressant renders it unsuitable for prolonged administration in a chronic ailment like malaria, in which there is a tendency to strain certain endocrine glands, such as the adrenals. Some patients manifest an idiosyncrasy to quinine, and it is inadvisable to administer it to those who have had middle-ear disease.

It was on account of the objections to its use just stated, and because I saw many patients invalidated home suffering from over-medication with quinine, that I was induced in the early part of 1918 to undertake research on the treatment of malaria. The organic arsenic compound called novarsenobillon was the drug chosen for this research, chiefly because it had proved so efficient in the treatment of spirochætosis. In addition, the drug was easy to administer, relatively non-toxic, and not a vaso-depressant. Other workers, at home and abroad, have investigated along similar lines, but, as this work was carried out quite independently, little reference will be made to their results.

The present paper deals entirely with cases of malaria treated in this country, so that the question of re-infection does not arise. The patients had contracted malaria in various places—Italy, Macedonia, Palestine, Mesopotamia, India, Persia and South Russia. The total number of cases investigated during the years 1918 to 1924, was not large—119 in all. But against this small number may be set the fact that each case was very thoroughly observed.

PRELIMINARY INVESTIGATIONS.

It was necessary at first to ascertain whether novarsenobillon had the power of killing off malarial parasites in the blood-stream, and, if so, how long it would take to rid the peripheral blood of parasites. Secondly, we had to discover what length of time would elapse before malarial parasites could reappear in the peripheral blood; that is, the relapse period.

In connexion with these points it is interesting to compare fifteen cases of relapsing benign tertian malaria treated between August and November, 1918. Each case had been under daily observation for some time in order to establish the type of parasite, intensity of infection, and frequency of relapse when untreated.

These cases were divided up into three series.

Series I.—Five cases were given a single injection of 0.45 grm. of novarsenobillon intravenously. Afterwards the blood was examined hourly until the parasites had disappeared, then daily between 9 and 10 a.m. until parasites reappeared. The length of time between the disappearance and reappearance of parasites was estimated as the period of relapse.

Series II.—Five cases were given a single intravenous injection of 10 gr. of quinine hydrochloride. The blood was examined as above.

Series III.—Five cases were given quinine hydrochloride gr. 10, t.d.s. (gr. 30 daily) by the mouth for seven consecutive days. Their blood was checked for parasites in a manner similar to that adopted in the other cases.

The results are shown in Table No. I.

Treatment	Number of cases	Average number of hours before peripheral blood was negative to parasites	Number of cases relapsing	Average number of days before relapse
(1) Novarsenobillon 0.45 grm. intravenously, one injection ...	5	26	5	22.8
(2) Quinine hydrochloride 10 gr. intravenously, one injection ...	5	48	5	12.4
(3) Quinine hydrochloride 30 gr. daily for seven days by mouth ...	5	67	4	15.2

By this experiment it was demonstrated that novarsenobillon cleared the peripheral circulation of malarial parasites in the minimum of time, and that, on an average, the blood remained free from parasites for a longer period than under quinine treatment.

Passing reference may be made to the researches of Stephens, Yorke, Blacklock, and Macfie (1919) [1] in Liverpool. These workers found that 0.9 grm. of novarsenobillon given intravenously caused the disappearance of the benign tertian parasites in all its stages from the peripheral blood within twenty-four hours of administration. They also found that its action on benign tertian malaria was more rapid and efficient than that of quinine. Further experiments tended to prove that when used in conjunction with quinine the therapeutic results were more satisfactory than when either drug was used alone.

The results of the present series of experiments must not be taken as an indication of a definite decision on the matter. Due allowance must be made for a small number of cases treated, and for the relative vitality of parasite and patient. The value of recognizing these points will become more apparent when the results of the routine novarsenobillon treatment are considered. The figures, however, supplied a satisfactory basis upon which to draw up a plan for routine treatment. It was obvious that a small dose of novarsenobillon was sufficient to cause the disappearance of the parasites which had gained entrance to the blood-stream, and that the three-week interval before a relapse was a guide to the frequency with which further doses of the drug should be administered in order to prevent relapses.

The five cases which had already been used in the first series of experiments were allowed to relapse, so that repeated tests could be made to ascertain whether novarsenobillon would continue to cause the disappearance of the parasites from the peripheral blood of the patients. It was found that the drug was just as efficient

in dealing with these relapses, or, in other words, the parasite did not become novarsenobillon-resistant.

The next point was to determine the power of the drug in preventing relapses. For this purpose the interval between the doses was shortened so that an injection was given two days before a relapse was expected. This mode of treatment was kept up for three months, and during this period no relapses occurred.

Shortly after this, when treating routine cases, it became evident that these results over-estimated the power of novarsenobillon. The parasites in every case of benign tertian malaria could not be kept in check for three weeks by one injection of novarsenobillon. The date of relapse was governed mainly by the natural relapse interval, that is to say the period between the disappearance and reappearance of parasites in the peripheral blood in untreated cases. In some cases this period was as short as nine days. Accordingly, the original plan of giving an intravenous injection once every three weeks had to be replaced by one of giving weekly injections. After four to six weekly injections the relapse period tended to lengthen, and in most cases the interval between injections could then be extended to three weeks.

ROUTINE TREATMENT.

Before treatment was begun each case was kept under clinical and laboratory examination daily until the type of disease, presence of malarial parasites, and freedom, or otherwise, from other diseases were ascertained.

The routine treatment varied somewhat as experience was gained. Large doses of novarsenobillon (0.75 and 0.9 gm.) were given to the earlier cases treated, but later this was found to be unnecessary and 0.45 gm. was administered as the standard dose.

Course A.—Most of the cases received an intravenous injection of 0.45 gm. novarsenobillon weekly for six weeks, afterwards one every third week for three months. During the last three months a tonic pill was prescribed. The pill contained iron, arsenic, strychnine, and cascara; it was to be taken for the first three weeks of each month.

Course B.—Patients who had a definite history of fortnightly or three-weekly relapses were put on a modified course. They were given injections weekly for two weeks, then fortnightly or three-weekly, until eight injections had been given.

In those cases which relapsed after this treatment the patients were given a further course, usually Course B, or else such a course combined with the oral administration of quinine. The two courses of treatment, with a break of six weeks in between, took about a year to complete.

TABULAR STATEMENT OF COURSE A.

Day of treatment	No. of injection of novarsenobillon 0.45 gm.			Remarks
1st day	...	1st	...	In bed, in hospital
8th "	...	2nd	...	Up, in hospital
15th "	...	3rd	...	Convalescing on leave or light duty
22nd "	...	4th	...	} On light duty
29th "	...	5th	...	
36th "	...	6th	...	
57th "	...	7th	...	} Light duty and on tonic pill
78th "	...	8th	...	
99th "	...	9th	...	
120th "	...	10th	...	

During the period of treatment, so long as the cases were in hospital or near enough to attend, the peripheral blood was examined daily for parasites. After the patients had left the hospital they were instructed to report again for treatment every two to three weeks, according to the nature of the case; on these occasions, the blood was examined for parasites and then an intravenous injection of novarsenobillon was given.

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When the course of treatment was completed the patient was requested to report monthly for examination. In this manner these malaria cases were kept under observation for a year, some for a much longer period.

NUMBER OF CASES TREATED.

The total number of cases investigated was 119, comprised as follows:—

(a) Benign tertian malaria, 93; (b) malignant tertian malaria, 18; (c) quartan malaria, 2; (d) mixed types (M.T. and B.T.), 6.

Of the benign tertian cases, seventy-eight were treated with novarsenobillon, five with quinine, and ten with a combination of quinine and novarsenobillon. The malignant tertian, quartan, and mixed types were treated with quinine in conjunction with novarsenobillon.

The novarsenobillon treatment was as described above. The quinine treatment consisted of the daily administration of 30 gr. of the hydrochloride orally for a week, 20 gr. for four weeks, and 10 gr. for the following eight weeks. In the combined form of treatment 0.45 grm. of novarsenobillon was injected intravenously at the same time intervals as in the Course A, but, in addition, quinine gr. 10 was given orally thrice daily on the day of each injection and for two days subsequently.

The percentage of relapses was estimated from the number of blood-film relapses that occurred within eight weeks of the stopping of the treatment.

TABULAR STATEMENT OF CASES TREATED.

Type of Malaria	No. of cases treated	Treatment given	Percentage of cases showing a relapse within eight weeks of last treatment.
Benign tertian	93	78 were given novarsenobillon	41
		5 were given quinine	60
		10 were given quinine and novarsenobillon	30
Malignant tertian	18	novarsenobillon and quinine	68
Quartan	2	" " "	100
Mixed (B.T. and M.T.)	6	" " "	100

GENERAL RESULTS OF NOVARSENOBILLON TREATMENT.

The number of cases of malignant tertian, quartan, and mixed types of malaria treated was not sufficient to justify an opinion as to results.

As regards the effect of novarsenobillon on benign tertian malaria numerous general observations were made.

The drug, when given intravenously in small doses, rapidly cleared the peripheral blood-stream of parasites, usually within twenty-four hours, and, if the injections were repeated at weekly intervals, relapses were prevented. In addition, a small dose (0.45 grm.) appeared to be as efficient as a larger one (0.9 grm.).

When the records were investigated, it was found that the relapses could be classified into two types, according to the length of time that the patient had been infected with malaria. Infections of less than one year's duration were very liable to relapse, at times within nine days; but, older infections were more amenable to treatment. Of the seventy-eight cases of benign tertian malaria treated with novarsenobillon, twenty-five had been infected for less than a year, and, of the latter, 80 per cent. relapsed within eight weeks of last treatment; whereas, of the fifty-three patients who had contracted malaria more than a year previously only 22.6 per cent. relapsed.

TABLE SHOWING THE PERCENTAGE OF RELAPSES IN BENIGN TERTIAN MALARIA WHEN TREATED WITH NOVARSENOBILLON.

Type of Benign Tertian case treated	Number of cases that relapsed	Percentage of cases that relapsed within eight weeks of last treatment
53 cases of over twelve months' infection	12	22.6
25 cases of under twelve months' infection	20	80.0
78 cases of both of above types	32	41.0

The view arrived at was that so-called acute or primary malaria was more difficult to eradicate from the system than was the chronic or secondary type.

The *tonic effect* of the drug was very noticeable. Within a few days of the first injection, patients looked brighter and said they were feeling much fitter. In fact, notwithstanding the necessity of being pricked by a needle, many patients came back after the treatment was completed and asked to be given further injections, as they said they felt much better afterwards. The improvement in general health was more rapid and marked than when quinine was given.

In a few cases, within twenty-four hours of an injection of novarsenobillon, parasites were found in the peripheral blood, which previously had been reported as negative. It did not necessarily follow that the drug had done harm. Apparently the arsenical compound had acted as a vaso-constrictor and a stimulant of visceral muscle generally. In this manner there was a toning up of the circulatory system, including such organs as the spleen, and some parasites that had been lurking in a back-water were forced out into the fairway, where they were exposed to the toxic effects of the circulating drug. It was thought that if a dose of quinine was given on the day following the intravenous injection, those parasites which had been dislodged would be destroyed. On this line of argument the combined novarsenobillon and quinine treatment was instituted for the more resistant cases of malaria.

Most of the patients stood the drug well. Only in four of them were toxic symptoms encountered. In one case, suffering from both malaria and syphilis, cardiac and respiratory distress occurred during the sixth injection. This man was taking very large doses of potassium iodide at the time, and this may have accounted for the reaction. In the three remaining cases there was a history of periodic attacks of urticaria, so that a toxic reaction was not unexpected.

Malignant tertian, quartan and mixed types of malaria did not respond so readily to novarsenobillon. The injections had to be given more frequently, and, as there was the danger of arsenical poisoning to contend with, it was considered advisable to treat the more resistant cases with the combined therapy.

COMPARISON OF THE ANTIMALARIAL POWER OF NOVARSENOBILLON AND QUININE.

When considering the results of any form of antimalarial treatment there are certain difficulties to contend with. First of all, there is no agreement amongst various workers as to what shall be deemed a relapse; the reappearance of parasites in the peripheral blood would be a satisfactory test, provided a uniform time limit was agreed upon. The longer the period of observation, the more liable are relapses to occur. By many workers the term refers to a period of four weeks after the cessation of treatment. The next difficulty is to determine the length of time the patient has had malaria; this affects the vitality of the parasite. Lastly, it is impossible to estimate the vitality of the patient.

The results obtained in the short series of preliminary tests, after due allowance had been made for the above-mentioned difficulties, were sufficient to show that the action of novarsenobillon on malaria was of such merit as to warrant an extended trial. Unfortunately, there were not enough cases available to enable one to do more than test the antimalarial power of one drug properly. On this account, attention was concentrated on novarsenobillon.

For the purpose of general comparison, the results obtained with this novarsenobillon treatment will be compared with the findings of other writers who have carried out special researches dealing with the action of quinine on malaria.

(a) *Disappearance of Parasites from Peripheral Circulation.*

Regarding the length of time taken by quinine to effect the disappearance of parasites from the peripheral blood, there is agreement of opinion. With daily doses

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of 20 to 30 gr. of quinine given orally, this disappearance occurs within two to four days [2], [3], [4]. In the case of novarsenobillon, the present figures and those of Stephens show that a single dose of the drug will produce the same result within twenty-six hours [1].

TABULAR STATEMENT OF THE DISAPPEARANCE RATE OF BENIGN TERTIAN PARASITES FROM PERIPHERAL BLOOD AFTER ADMINISTRATION OF QUININE AND NOVARSENOBILLON.

Authority	Drug and dose	Number of days elapsing from the beginning of treatment until parasites had disappeared from the peripheral blood
Johnson and Gilchrist ...	Quinine 20-30 gr. daily ...	3-4
Fletcher ...	" 20 " ...	2-4
	" 10 " ...	2-4
	Quinidine 10 gr. daily ...	3
War Office, 1917 ...	Quinine 20-30 gr. daily ...	2-4
	" 30 " ...	3
Present paper ...	" 10 gr. intravenously ...	2
	Novarsenobillon 0.45 grm. intravenously ...	1
Stephens ...	Novarsenobillon 0.9 grm. intravenously ...	1

This rapid disappearance of all stages of the benign tertian parasites from the peripheral blood is of great value in connexion with the sterilization of malarial carriers in Anopheline areas.

(b) *Prevention of Relapses.*

It is very difficult to sift the figures relating to relapses. Acton [5] gives the percentage of relapses that occur in benign tertian malaria cases, after a full course of quinine, as 75, Stephens [6] as 54, and Johnson and Gilchrist [2] as 23.8; the relapses took place within three weeks of the cessation of treatment.

Fletcher [3] reports that 1.7 per cent. of benign tertian cases relapsed whilst receiving 20 grains of quinine daily under strict supervision: this treatment only lasted for a month.

In the instructions for the treatment of malaria issued by the War Office in 1917 [4], it is stated that "men who have been given antirelapse quinine prophylaxis of only 5 grains daily have relapsed while taking it to the extent of 30 per cent. of the cases." Other authors claim that they have had only isolated cases of relapses. Manson [7] admits the common liability to relapse and recommends that malarial patients continue to take quinine periodically over a period of two years.

Of the present series of seventy-eight cases of benign tertian malaria treated with novarsenobillon, 29 per cent. relapsed within three weeks, and 41 per cent. within eight weeks of cessation of treatment. During the treatment, four of the cases had a relapse, that is 5.1 per cent. It should be noted that the length of this treatment was four months. These figures compare very favourably with those for the quinine treatment.

TABLE SHOWING PERCENTAGE OF RELAPSES WHICH OCCURRED IN CASES OF BENIGN TERTIAN MALARIA WITHIN THREE WEEKS OF CESSATION OF TREATMENT.

Authority	Drug administered	Percentage of cases relapsing within three weeks of the cessation of treatment
Acton ...	Quinine ...	75
Stephens ...	" ...	54
Johnson ...	" ...	23.8
Present paper ...	Novarsenobillon ...	29

There is no uniformity in the results of the quinine treatment. The most probable explanation of this is that all the quinine prescribed was not taken by every patient. This is the weak point in oral quinine medication. It could be overcome by giving intravenous injections of the drug, but, as will be discussed later, it is doubtful whether such treatment is as efficient as the giving of novarsenobillon.

(c) Duration of Disease.

This depends on the stage of the disease; very early cases can be cured quickly, acute malaria is more difficult to eradicate, while chronic malaria occupies a mid-way position. Whichever drug is used, the disease, in the majority of cases, is only kept in control while the treatment is being administered. Over- or under-medication may prolong the disease.

It is important to remember that two organisms—the malarial parasite and man—are intimately associated in this treatment. The task is to kill off the protozoon of malaria without injuring the tissues of man, as damaged or devitalized tissues may form a nidus for the parasites and aid them in their struggle for existence. The parasites appear to be most vulnerable when circulating in the peripheral blood-stream. The tissues and body fluids of man possess the power of overcoming this parasite and thus of either eradicating the disease or of holding it in check.

Quinine is a tissue poison; this is made evident by the necrosis following intramuscular medication, rectal lavage and, at times, intravenous injection with the drug. Therefore, there is nothing to be gained by giving injections of quinine in place of novarsenobillon, for the latter is less toxic to man and is a vaso-stimulant.

The beneficial action of novarsenobillon is apparently due on the one hand to its power to destroy the parasites circulating in the peripheral blood, and on the other hand to the stimulation of the body-tissues to overcome the parasite. Those parasites to which belong the greatest powers of proliferation are most liable to gain entrance to the blood-stream and, by the killing off of these, a less prolific strain is left in the body. As a result there is a lengthening of the relapse period and also, on this account, the interval between injections can be increased. The great value of novarsenobillon lies in its use for the treatment of chronic benign tertian malaria, for it is in such cases that its tonic effect is most evident.

There are several general points in favour of intravenous medication with novarsenobillon in malaria.

- (1) The patient has no opportunity of evading the administration of the drug.
- (2) The exact dosage is known.
- (3) The maximum amount of the drug comes in contact with the blood-parasites in the shortest interval of time. By this means the dose can be accurately adjusted, so as not to damage the tissues of man.
- (4) Treatment is given weekly, fortnightly or every third week, instead of daily as is the case with quinine. This procedure renders the treatment less tedious to all concerned.
- (5) The drug can be given at the height of the fever—the time when the parasites are most vulnerable. With oral administration of quinine it is necessary to wait until the fever has abated before giving the drug, as its absorption by the congested gastric and duodenal mucosa is delayed during the pyrexia.
- (6) The drug can be administered to patients suffering from vomiting or unconsciousness.
- (7) The pain caused by the veno-puncture is trifling; and the danger of abscess formation and discomfort due to tension, which are liable to follow subcutaneous or intramuscular injections of quinine, is reduced to a minimum.
- (8) Other diseases such as relapsing fever, blackwater fever, syphilis, Veldt sore, Vincent's disease, yaws, and frambæsia, any of which may complicate malaria, can be treated by this drug.

It is only fair to state that there are some recognized disadvantages attaching to the intravenous medication with novarsenobillon. For example those suffering from cardiac or renal disease are excluded from undergoing its administration. It must

also be remembered that a certain amount of skill and experience is required to master the technique of intravenous therapy.

RELATIVE COST OF NOVARSENOBILLON AND QUININE.

An important point regarding any form of treatment is the cost. There is a marked difference in the relative prices of quinine and novarsenobillon.

Three types of quinine treatment may be cited: (1) Short course, 20 gr. daily for four weeks, that is, 560 gr. spread over four weeks; (2) medium course, 20 gr. daily for three weeks followed by 60 gr. weekly for nine more weeks, that is, 960 gr. spread over twelve weeks; (3) long course, 30 gr. daily for three weeks, 20 gr. daily for four weeks, and 10 gr. daily for eight weeks, giving a total of 1,750 gr. spread over fifteen weeks.

The cost price of quinine sulphate is 3s. 7d. per oz. and, if the figures are worked out, it will be found that in each of the three courses about 1s. per week per case will be spent on quinine. Whereas, a dose of 0.45 gm. novarsenobillon costs 2s. 3d. Thus, if weekly injections are given, the relative cost of the novarsenobillon treatment is double that of the quinine. The average case of malaria, however, would require treatment for about a year, and, as eighteen doses of novarsenobillon would be the amount administered in that time, the cost of the drug per case for one year would be £2 0s. 6d. With the quinine treatment, probably two long or three medium courses would be needed, giving a cost per head of about £1 10s. in either case. The longer the course of treatment the less is the difference in the cost.

As regards the cost of the drug, the quinine treatment is the cheaper, but in the reckoning of the cost of any treatment the time spent in hospital must be taken into consideration. With the novarsenobillon treatment a week in hospital is sufficient to overcome the acute attack; following that the patient simply reports to the medical officer weekly, or at longer intervals, for injections. The duration of hospital treatment when quinine is used is certainly as long as a week, if not even longer.

There remains to be considered the treatment of malaria from the standpoint of administration. It is obvious that the control of the treatment will lie in the hands of either the doctor or the patient. In the case of giving quinine by the mouth, this control is left chiefly to the patient. No matter how honest the man may be, he is apt to be forgetful at times. As Fletcher has remarked, "Quinine resistance lies, not in the parasite but in the patient." On the contrary, the novarsenobillon treatment is entirely in the hands of the doctor who gets a firm hold of the case. This hold is of the greatest importance to both people concerned. The doctor knows what treatment has been received and can watch the progress of the case, and there is with this mode of treatment a more beneficial psychical effect on the patient.

It is not stating the case too forcibly to say that, in the treatment of benign tertian malaria, novarsenobillon compares very favourably with quinine. The drug is not recommended in the treatment of malaria to the exclusion of quinine, but should be used in conjunction with it to ensure that the patient receives enough treatment to prevent a relapse. The two drugs act on the human body in a somewhat different manner, and at times the one is not well tolerated, at times the other. A combined therapy offers a greater scope of usefulness, for one drug will save the use of too large a dose of the other. The position is perhaps better illustrated by analogy. Organic arsenic compounds have been found to be of the utmost value in the treatment of syphilis, yet mercury has not been replaced by them. To carry the analogy further, the improvement in the antisyphilitic treatment since the widespread introduction of injections is possibly not due to any superiority of the new drug over the old, but rather to the fact that the drug has been actually given, and not avoided.

In conclusion, I wish to express my deep gratitude to the many patients who have so heartily co-operated in this work, and also to my assistant, Miss M. Sage, for the great care and time spent in examining the bloods for malarial parasites.

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Discussion.—Dr. ARTHUR POWELL said he regretted that his experience had been by no means so satisfactory as that of the author. When salvarsan first came into use he had had great hopes that it would destroy the malarial parasites. He employed it, counting the parasites before injection and from day to day afterwards. He found no reduction in fever or number of relapses and little diminution in the number of parasites. In one case of quartan fever there were three relapses after an intravenous dose of 0.5 grm. salvarsan (Meister, Lucius und Brüning), with increase in the number of parasites. During the third of these relapses two 10-gr. doses of quinine were given. No further relapse occurred, and next day parasites were scant; three days later they were absent. In several cases of benign and malignant tertian there was a continuance of relapses until quinine was given.

Malarial attacks frequently began in patients who were receiving salvarsan in full doses. Most of these were undoubtedly relapses, but five patients, in his opinion, were infected in the ten-day interval of full doses of salvarsan or neosalvarsan. All five were policemen in the Service for from two to seven years; during that time, they said, they had had no fever and certainly had never been off duty for fever. He (Dr. Powell) did not know of any biological method of proving that a benign tertian attack was a new infection, but our knowledge of the life history of *Laverania* enabled us to recognize a new infection. In the first of these malignant tertian cases, the fever reappeared five days after the second dose of 606, fifteen days after the first dose. On the first day of fever the blood showed numerous rings, on the second few rings, on the third very many rings, and again rings on the fourth, fifth and sixth days. On the seventh day there were numerous rings and, for the first time, a few crescents. From that day onwards crescents increased rapidly. On the tenth day quinine was given.

The second case was very similar, crescents first appearing on the eighth day of fever. Quinine was then given as the fever was rather serious. There could be no doubt that these two cases were infected between the doses of salvarsan. The second dose of salvarsan was given at a very favourable time, when the parasites were young and when one would expect a proper dose of quinine to kill them all.

In cases of cachexia and chronic malaria, he (Dr. Powell) found arsenic of great value when given in large doses, but it was not necessary to use the salvarsan compounds, which had to be given intravenously. He had good results from cacodylate of soda, which he injected in intramuscular doses of five to eight grains on alternate days. These doses were perfectly safe, perfectly painless, and needed no skill in administration. He had given them daily, in one case for fifteen consecutive days. He knew a colleague who gave gramme doses daily.

Lieut.-Colonel MACARTHUR said he considered the salvarsan preparations had little direct action on the malaria parasites. He knew from experience that malaria would develop in persons undergoing active salvarsan treatment for other conditions, and that such injections could not be relied upon to check the malaria infection. He cited a dozen cases of persons who were actually infected with malaria—several of them fatally—by an injection of neosalvarsan given through tubing contaminated with blood from a malaria carrier who had received an injection previously. A freshly sterilized needle had been used for each person, but the same tubing had been employed throughout. He could not imagine such infections occurring through a similar accident during intravenous administration of quinine.

At one time he had had several hundred cases of severe relapsing malaria—mostly of Macedonian origin—under treatment, and considered them a good test series, as they were the residuum left after the more amenable cases had been sorted out. He obtained the best results by giving quinine on two days each week, with arsenic, up to the limit of toleration.

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on the quinine-free days. He was not satisfied that neosalvarsan was any more certain to prevent relapse than arsenic given by the mouth. He believed the beneficial effects of arsenical treatment were due to the tonic properties of these preparations, and not to any direct parasitocidal action.

Wing Commander Whittingham had mentioned necrosis following intramuscular injections of quinine. He would like to hear from him, or from others in the audience, what proportion of their cases showed appreciable trouble following intramuscular injections. He believed that a useful method of treatment was falling into undeserved disrepute. Personally he had given as many as eighty injections in one week, and had never had any ill results.

One patient, unable to absorb quinine taken by the mouth, had received twenty-five injections into the deltoid muscles. Supposing each injection to have caused some degree of necrosis the patient should have had no muscle tissue left at the end of the injections, but the condition of his deltoids when the course was finished appeared as normal as it was before the injections were begun. He considered that intramuscular quinine was the treatment of choice in very severe cerebral cases which showed evidence of serious cardiac involvement. He had treated six such cases thus, with complete recovery in them all. But owing to the attacks on intramuscular quinine he now felt himself in a difficult position; if he were to treat a case of severe cerebral malaria and cardiac involvement by intramuscular quinine, and save the patient's life, yet if the person should be the 1 in 1,000—or whatever the figure might be—who developed an abscess, he would never hear the end of it.

War Section.

President—Surgeon Vice-Admiral JOSEPH CHAMBERS, C.B., C.M.G., R.N.

DISCUSSION ON TUBERCULOSIS IN THE SERVICES.

I.—TUBERCULOSIS IN THE ARMY.

Colonel J. C. KENNEDY pointed out that there were many aspects of the subject that might be considered with profit, but under the circumstances he would limit himself to a brief statistical survey of the incidence of tuberculosis in the army.

He said it was his duty to make passing reference to previous work on this subject, such as the writings of Melville, Simpson, Johnston and Cummins, but in order to clear the ground and make the survey more comprehensive, it was necessary to refer in some detail to the statistical analysis prepared by the first two of the above-named officers.

These officers, in analysing the statistics for the period 1860 to 1905, showed that the admission rate for tuberculosis in the army fell from 11 to 3; that the total wastage declined from 9·23 to 2·27 per 1,000 of strength; that the losses due to tuberculosis fell from one-fifth to one-seventh of total wastage from all causes; and in comparison with a 50 per cent. improvement in the civil death-rate Melville estimated that the army showed an 80 per cent. improvement.

The factors influencing the army statistics during that period were:—

(i) Improvement in barrack accommodation, which was credited with 99 per cent. of the improvement in the tuberculosis rate.

(ii) Improvement in the hygienic surroundings of the soldier.

(iii) Conditions of enlistment—the introduction of the short-service system, which, by decreasing the average age of the army, increased the number of young susceptible adults.

(iv) Changes in nomenclature, and

(v) Alteration of the regulations for invaliding.

Colonel Kennedy then presented his analysis of statistics for the period 1902 to 1923. He regretted that it was not possible to deal with the war period 1914 to 1920, as reliable statistics were not available. In any case, that period was altogether abnormal, and such statistics as were available pointed to a tuberculosis rate more in keeping with that obtaining in the civil population, as was to be expected.

TABLE I.—TUBERCULOSIS IN THE ARMY, AT HOME AND ABROAD.
Ratios per 1,000 Strength.

Year	Admissions			Deaths	Total losses
	Lung	Other	Total		
1902 ...	—	—	3·2	0·56	1·93
1903 ...	—	—	2·9	0·36	1·66
1904 ...	2·1	0·3	2·4	0·47	2·35
1905 ...	2·0	0·2	2·2	0·40	1·85
1906 ...	2·0	0·4	2·4	0·28	1·15
1907 ...	1·9	0·3	2·2	0·35	1·86
1908 ...	1·6	0·5	2·1	0·25	1·62
1909 ...	1·6	0·3	1·9	0·26	1·72
1910 ...	1·5	0·3	1·8	0·24	1·48
1911 ...	1·3	0·4	1·7	0·22	1·40
1912 ...	1·5	0·4	1·9	0·24	1·42
1913 ...	1·3	0·4	1·7	0·25	1·56
1914 ...	1·2	0·4	1·6	0·22	1·33 (seven months)
1921 ...	1·6	0·4	2·0	0·23	1·66
1922 ...	1·3	0·3	1·6	0·19	1·52
1923 ...	1·38	0·36	1·74	0·2	1·48

The figures in Table I were plotted on graphs and the following points demonstrated:—

(1) A steady decrease in the total admission rate, representing a 50 per cent. decrease from 1902 to 1914.

(2) Following the war period there had been a set-back, but not a serious one.

(3) The death-rate as the result of invaliding regulations had settled down since 1907 to what might be considered a bed-rock level.

(4) Since 1904, the first year in which the statistical returns differentiated pulmonary from other varieties of tuberculosis, there had been little variation in the admission rate for "other" tuberculosis, hence the decrease during the period 1904 to 1914 had been entirely in the admission rate for pulmonary tuberculosis, i.e., from 2.1 to 1.2 per 1000, a 40 per cent. decrease.

Such a steady and gratifying decrease as was evident during the ten years to 1914 demanded an explanation and investigation into the factors at work. No doubt there had been steady improvement in the conditions of a soldier's life, such as better rationing, more rational methods of training, and more careful medical and hygienic supervision; but there had been no drastic and important change such as the improvement in accommodation that became effective in the eighties. It was obvious that tuberculosis in the army must bear close relation to, first, the incidence of the disease in the civil population and, secondly, recruiting. It was necessary therefore to refer in some detail to these two factors.

With regard to the first, the tuberculosis rate in the civil population, the following points were demonstrated by tables and graphs:—

(1) A steady decrease in the tuberculosis death-rate in Scotland and England during the period 1871 to 1921, from 373 and 354 to 122 and 113 per 100,000 respectively; pulmonary tuberculosis alone decreased from 261 and 269 to 84 and 88 per 100,000 respectively.

(2) During the twenty years 1901 to 1921, the decrease in the death-rate from pulmonary tuberculosis was 46 per cent. in Scotland and 32 per cent. in England.

(3) There was evidence of a set-back during the period of the war, less evident, however, in Scotland than in England. The death-rates for the male population reckoned in age periods indicated that it was in the young manhood period—in other words, in men of army age—that the incidence of pulmonary tuberculosis was highest, and in the same period the incidence of "other" tuberculosis was insignificant in comparison.

So far as it was possible to make a comparison between the two sets of statistics, the one a death-rate and the other an admission rate, there was no doubt a marked improvement in both the civil population and the army, though perhaps the balance was in favour of the army up to 1914. Since 1914 the army admission rate had had a set-back but was showing signs of settling down to its pre-war level. It would become evident that this set-back was a war effect.

With regard to the second factor, recruiting, Colonel Kennedy showed that while there had been a steady decrease in the death-rate in the civil male population, the number of rejections for tuberculosis had not been decreasing, in fact the rejection rate had on the whole increased, and from 1910 to 1913 had risen from 1.0 to 2.1 per 1,000 examined.

TABLE II.—RECRUITING. PRE-WAR PERIOD.
Rejections for Tuberculosis—Rates per 1,000 examined.

	1890 to 1899	1900 to 1909	1910	1911	1911-12	1912-13
	(mean of 10 years ratios)					
Lung ...	—	—	—	—	0.98	1.02
Other ...	—	—	—	—	0.66	1.12
Total ...	1.00	1.04	0.98	1.48	1.64	2.14

In his (Col. Kennedy's) view, this indicated an increasing vigilance on the part of the recruiting medical officer and no doubt contributed to the steady fall in the incidence of army tuberculosis, but at the same time it had to be pointed out that a large proportion of the increased rejections was due to "other" tuberculosis. Therefore up to 1914 the steady decrease in the incidence of tuberculosis in the army was seen to fall into line with the decrease amongst the males of the civil population, and increasing care on the part of the recruiting medical officers.

TABLE III.—RECRUITING. POST-WAR PERIOD.
Rejections for Tuberculosis—Rates per 1,000 examined.

	1920-21	1921-22	1922-23	1923-24
Lung ...	1.78	1.42	1.18	1.68
Other ...	2.86	1.89	1.96	1.64
Total ...	4.64	3.31	3.14	3.22

After 1913, and passing on to 1920-21, when accurate statistics again became available, it was shown that the rejection rate for tuberculosis rose from an average of 1'5 to 4'6 per 1,000 examined, to fall again in three succeeding years to 3'3, 3'1 and 3'2 respectively. Again the increase was due more particularly to "other" tuberculosis. Pulmonary tuberculosis, which accounted for 1'0 per 1,000 rejections in 1911-12 and 1912-13, was responsible for a rejection rate in 1920-21 and the three following years of 1'7, 1'4 and 1'7 respectively.

This extraordinary increase in rejections for tuberculosis was an interesting feature of the statistics of recruiting, and was undoubtedly an after-effect of the war. It could be explained by the fact that since 1918 recruiting for the army had been passing through a difficult period. Rapid demobilization demanded immediate replacement of practically the whole army, and the physical standard of forthcoming recruits was undoubtedly of an inferior order. 1919-20 was a most difficult year, when 71,000 recruits were enlisted, and in normal circumstances many of these would have not passed the physical or medical examination. As it was, 321 per 1,000 examined were rejected for physical and medical disabilities. In 1920-21, 44,950 recruits were enlisted, and the physical and medical rejections rose to 489 per 1,000; 382 per 1,000 were rejected on medical grounds alone, and tuberculosis accounted for 4'64 per 1,000. This represented 12'1 rejections for tuberculosis for every 1,000 rejections for all medical causes. This was a high rate, and compared unfavourably with the pre-war period and with the years subsequent to 1921. The corresponding figure for 1903 to 1911-12 was 4'17 (average); for 1912-13, 9'9; for 1921-22, 8'14; for 1922-23, 7'52; and for 1923-24, 8'14.

That the immediate post-war period was a bad one for recruiting was further emphasized by the figures for wastage (deaths and invaliding, excluding those due to injury). During the ten years before the war the wastage per 1,000 strength fell from 19'72 to 9'39, while in 1921 it rose to 26'69, but fell to 19'57 in 1922. The wastage for tuberculosis alone in the corresponding pre-war period fell from 1'97 to 1'56; in 1921 it rose to 1'66; in 1922 was 1'52 and in 1923 1'48.

From these figures, therefore, it would appear that the problem of recruiting after the war was one of real difficulty and was reflected in the general health of the army; and that so far as tuberculosis was concerned, it was the vigilance of the recruiting medical officer that prevented an undue increase in its incidence in the army.

Colonel Kennedy then gave an analysis of the case cards of 253 soldiers who had died from, or had been invalided with, pulmonary tuberculosis during 1923.

TABLE IV.—ANALYSIS OF THE CASE CARDS OF 253 MEN INVALIDED OR DEAD FROM TUBERCULOSIS OF LUNG DURING 1923.

(a) Percentage in periods of service.						Per 1,000 strength.	
Under 1 year	3.9	53.6	...	0.86	1.31
1-2 years	10.7				
2-3 "	12.6				
3-4 "	11.0				
4-5 "	15.4				
5-10 "	24.0	4.40	
10-15 "	9.5	2.85	
15-20 "	9.0	2.50	
Over 20 "	3.9	2.90	
(b) Percentage in age groups.							
Under 18	1.98	13.06	...	1.04	1.31
18-19	2.38				
19-20	8.70				
20-25	50.20	1.12	
25-30	13.04	1.50	
30-35	9.48	1.88	
35-40	11.06	2.80	
Over 40	3.16	2.77	

These figures showed that 53.6 per cent. of the cases occurred in men under five years' service and 50 per cent. in the age period 20-25. From this it would be deduced that it should be possible to eliminate a considerable proportion of tuberculosis in the army by still more careful recruiting, particularly in view of the fact that 14 per cent. occurred within the first two years of service, and 13 per cent. under the age of 20 years.

On the other hand, the percentage figures were misleading if they were not considered in relation to the strength in age and service groups. In the army of 1923, totalling 192,000 men, 157,000 were under five years' service, and 145,000 under 25 years of age.

Reference to the third column of Table IV showed that under five years' service the incidence of pulmonary tuberculosis was low—0·86, compared with the mean of 1·31 per 1,000 strength for the whole army. In the 5-10 year service period the ratio was high—4·40, and could be explained by the fact that men in this category were enlisted in 1914-1918—the war period.

It was desired, however, to focus attention more particularly on the comparatively high ratio obtained in men of over ten years' service, 2·5 to 2·9 per 1,000, a ratio comparable with that in the age groups over 35. This would appear to possess significance. Whatever might be said about the relation of good or indifferent recruiting to the incidence of tuberculosis in men of under five years' service, tuberculosis in men of over ten years' service was a different problem, and for all practical purposes it might be postulated that men who had survived ten years' service had contracted infection since enlistment. Hence it must be granted that a tuberculosis rate in men over ten years' service, or over 35 years of age, must be a truer indication of the liability to tuberculosis in the army than was a ratio based on the total strength of the army, and provided a truer figure for comparison with the civil population.

It would be remembered that the average curve of the death-rate by ages in the male population rose abruptly up to 25 and 35 and then gradually subsided; whereas a similar curve plotted out for the army showed a gradual and steady rise to a maximum after 35 years of age.

Any attempt to compare the incidence of pulmonary tuberculosis in the army with that in the civil population could only be approximate. For every death only three to four cases in living people came to the knowledge of the health authorities, whereas it was proved experimentally, and generally recognized, that for every death in any given district there were nine to ten living cases. On this assumption, in the civil male population over 35 years of age showing a death-rate of 1·2 per thousand, we should expect to find a case incidence of 11 per 1,000. Therefore the liability to pulmonary tuberculosis in the army as compared with civil life was 1 in 4. Examination of statistics for previous years indicated that this ratio had been still more in favour of the army, and it would appear therefore that as time passed a closer approximation was to be anticipated.

The relation of civil tuberculosis to tuberculosis in the army was a subject that could be enlarged upon, and instances might be given of Continental conscript armies showing a much higher tuberculosis rate than that of our own army, and closely approximating to the civil rate.

II.—TUBERCULOSIS IN THE ROYAL NAVY.

Surgeon Commander C. H. DAWE, D.P.H., R.N.—The chart here shown (Chart I) represents the phthisis death-rate per 1,000 at different ages for coal-miners, fishermen, males of England and Wales, 1881 to 1900, and 1901 to 1910, and merchant seamen 1900 to 1902.

The four graphs represent the tuberculosis invaliding rates in the navy, the figures being averages of four years at 1881 to 1884, 1901 to 1904, and 1921 to 1924. The 1921 to 1924 line is obtained from 231 cases of phthisis notified in those years, in the Plymouth Command, in a population of 23,000. The 1923 graph is from 198 cases of tuberculosis invalided from the whole navy in that year. The tuberculosis invaliding rate I take it to be a good index of phthisis rate.

The point to which I wish to draw attention is that forty and twenty years ago, the invaliding rate was higher at age period 25 to 35 than at 15 to 25, whereas now it is lowest at 25 to 35.

Before attempting to try to explain this change, may I refer to the statistical work done by Brownlee and Collis. Brownlee from an analysis of Registrar-General's returns showed that there were three different groups of phthisis, an old-age group affecting miners and others, a middle-age for industrial districts, and a young adult type in agriculture and certain sea-board counties. Collis in his recent Harben Lectures has shown that in purely non-industrial countries the type is that of old age, the incidence increasing with wear and tear of life. Industrialism leads to an increased town population and depopulated agricultural districts, inability to produce sufficient fresh food for population, and importation of food. Food prices rise, the better paid town people can buy, but the non-industrial population suffer from malnutrition. Industrialism shifts the wear and tear period to a maximum at an earlier age than in non-industrial life, and malnutrition of adolescents results in young adult phthisis.

As the agricultural districts slowly recover from their poverty, there will be a decline in the young adult phthisis rate, which has been the notable feature in England and Wales for the past seventy years.

Malnutrition is concerned not only with the quantity of food, but also with its quality, and at the present day we appreciate the importance of fresh food, in particular green leaf food, which includes the carcasses and milk of animals fed on green food. In support of this malnutrition theory, the instance of Ireland may be quoted; there the phthisis has been of

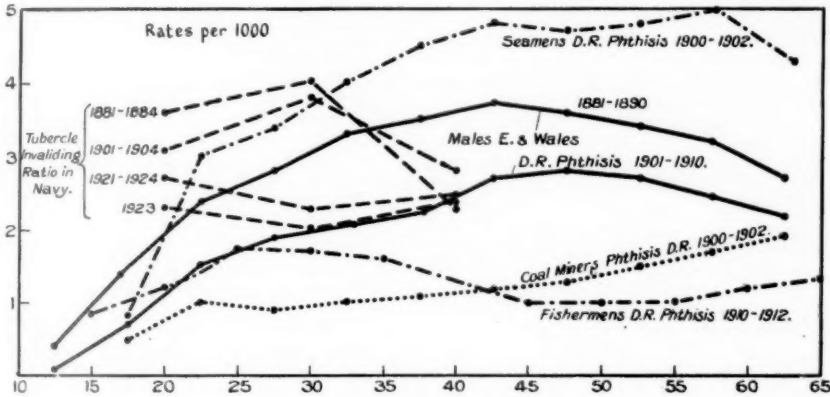


CHART I.

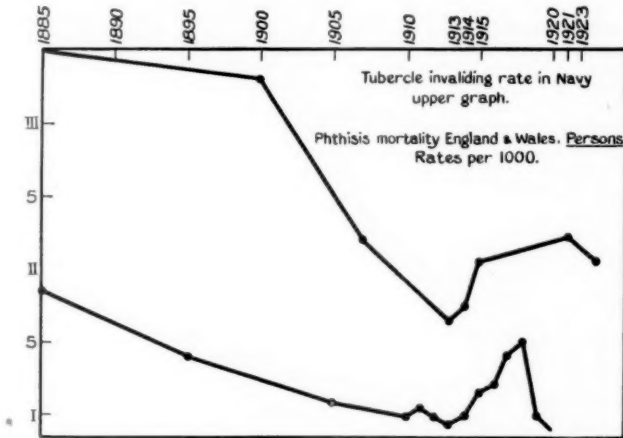


CHART II.

the young adult type for fifty years, and during that time has actually increased, the male death-rate being 3.5 at age 20 (1901-1910). The Irish peasant, I understand, suffers from a lack of fresh meat, and I would suggest that potatoes and pork have little connexion with green food.

The American negro furnishes another example. When in bondage, his tuberculosis rate fell, and the tuberculosis rate of the whites fell year by year. On the liberation of slaves the white tuberculosis rate continued to fall, but the uneducated negro, now exposed to the

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vices and dissipation of a civilized world, rapidly became the victim of increased tuberculosis incidence.

Statistical evidence shows that economic status and general hygiene exercise little influence on the incidence of young adult phthisis, and that from 10 to 30 years of age the tuberculosis problem in civil life is essentially a food problem.

From the next chart (Chart II) it will be seen that under peace conditions, the phthisis rate declined to a minimum in 1913 in both England and Wales and in the navy. The phthisis rate both in England and Wales and in the navy rose with the war, but in England and Wales the rate again declined to the 1913 line in 1919, whilst as late as 1923 the tuberculosis rate in the navy was still quite high. This suggests that hygienic conditions are worse in the navy now than in 1913, or that the navy is suffering from post-war period malnutrition of recent entries; the second suggestion being, I think, the more likely explanation.

From the navy (Chart I), it will be seen that:—

(1) The phthisis incidence at 15 to 25 is practically as much in excess of the civil rate in 1923 as it was in 1881.

(2) The phthisis incidence at 35 to 45 is practically the same now as it was twenty and forty years ago.

These two interpretations suggest that hygienic surroundings in the navy have not improved, or that if they have improved they have had little influence on the incidence of phthisis.

(3) The phthisis incidence at 25 to 35 has greatly improved, falling from rates of 4.0 and 3.8 per 1,000, forty and twenty years ago, to a rate of 2.0 per 1,000 in 1923.

This fall in the 25- to 35-year incidence may be attributed to the greatly improved diet made possible in the navy during the past twenty years, more fresh food being now available. In 1881 the navy ration was salt pork 1 lb. with dried vegetables one day, and salt beef 1 lb. with flour, suet, and raisins on the other day, with 1½ lb. of bread or 1¼ lb. of biscuits. In 1904 it was ¾ lb. salt pork, and ¾ lb. salt beef. At present ½ lb. fresh meat is issued daily, and ¼ lb. salt pork once a week, salt beef being discontinued in 1905. From 1881 it has been laid down in the navy that fresh beef and vegetables are to be issued if available, but it has only been in recent years, with cold storage and the supply of frozen beef, that men have obtained fresh food daily. The sailor is now much better paid, and with the canteens available he is able to obtain a greater choice of food. In addition, with the institution of the general messing system, he is now better fed than ever he was before.

Life at sea of necessity means surroundings far from ideal, and in no shore occupation does one get this sudden rise in young adult phthisis as shown in the navy and in the merchant service.

The fisherman, living largely afloat, shows an adult curve for phthisis.

The coal-miner resembles many naval ratings in that he works in the absence of sunlight and in a humid atmosphere, but he gets good wages, fresh food, good housing and his phthisis rate is low.

It will be noted that in 1900 to 1902, up to the age of 30, the rates in the navy and the merchant service are shown to be much the same. But after 30 years of age, the merchant seaman's rate is much higher, presumably due to the wear and tear factor and the relatively poor hygienic conditions prevailing in the merchant service.

The 1923 graph for the navy is, I suggest, a combination of two graphs, namely:—

(1) A wear and tear graph showing a steady rise with age, on the lines of civilians not working under ideal hygienic conditions, e.g., clerks.

(2) A superimposed young adult curve largely influenced by:—

(a) Malnutrition in recruits.

(b) Diet in the navy at present, which might be improved for recruits who, having left home, are exposed to a sudden change of environment which is fairly strenuous for some of them.

III.—TUBERCULOSIS IN THE ROYAL AIR FORCE.

Wing-Commander TREADGOLD prefaced his remarks by regretting that, owing to the comparatively recent formation of the Royal Air Force and its small strength when compared to that of the navy and army, such figures as were available were of comparatively little value. They did, however, closely correspond both as regards case incidence ratios and proportion of respiratory to other forms of tuberculosis, with army figures for the same periods. He then discussed the question of tuberculosis in the army and R.A.F. abroad and at home, pointing

out that statistics showed that there was little if any greater incidence abroad. This was contrary to what one would expect in view of the lowered bodily health, more adverse climatic conditions, prevalence of dust and poorer food, in Eastern Commands, compared with home stations. No figures were available as to tuberculosis case incidence in such countries as Iraq, India and Egypt, but the prevalent impression was that the rate was a high one. The only explanation appeared to be that at home troops lived in far more intimate contact with the civilian population than they did abroad.

Surgeon Captain R. J. MACKEOWN, O.B.E., M.B., R.N. (*Royal Naval Hospital, Chatham*).—The lesson mainly to be drawn from the three excellent and most instructive papers which have just been read, is that the incidence of tuberculosis in the services could be even more reduced if stricter attention was paid to recruiting and to the dust problem. It was with the purpose of commenting on these two very points that I came up from Chatham this afternoon. As regards recruiting, the portals and medical scrutiny should be made stricter, and a pertinent cross-examination should be made as to history of colds, sickness, stay in hospitals or sanatoria, &c.

In the prevention of tuberculosis it is most important to remember that as there are no surer forerunners of this disease than hæmoptysis and especially pleurisy, all such cases should be discharged as soon as possible to hospital for observation and treatment with a view to survey; because to allow such persons to return to a crowded mess deck of a man-of-war or to a barrack-room is only to invite the spread of the disease. All cases of pleurisy other than those due to traumatism should be invalidated as such, so that they may derive the benefit of dispensary and sanatorium treatment later if necessary. Some medical officers, especially the younger ones, do not appear to be fully alive to the easy spread of this scourge in the navy, otherwise they would not retain cases of so-called *chest diseases* for so long on board before discharging them to hospital.

The Admiralty have for a long time been making, and are still making, the most laudable and satisfactory efforts to improve the hygiene of the sailor; to mention only one in passing, viz., the instalment of electric motor fans, air trunks, &c. It must not be forgotten, however, that these very currents of fresh air, heated to a temperature ranging from 56-62° F. in cold weather, cause dust to fly about and so carry the deadly germs, if present, to healthy lungs. Early detection and removal of chest diseases and elimination of dust are, therefore, an imperative necessity in the way of prevention.

In former days the dust problem was tackled by washing down the upper and mess decks daily; this was most efficacious, but it had the drawback in wet weather of keeping the decks damp. Now the practice is to wash decks twice a week, i.e., Wednesdays and Saturdays, and to wipe them over on the other days. In spite of all precautions, however, dust remains, and it is recommended that vacuum cleaners be introduced to do away with the use of the broom several times a day.

Tuberculosis can be dealt with successfully—if the material that carries it in a dormant or active stage is not recruited—by the early detection and removal of all cases to hospital, and by the removal of dust by vacuum methods. Such steps are being more and more carried out in practice, and when in addition, by provision of A.F.O. 227/25 vacuum cleaners (stated by Director-General Admiral Chambers to be now introduced) have been in use for some time, it is anticipated that the number of cases of tuberculosis will be much further reduced.

Surgeon Commander J. O'HEA, R.N., remarked on the great difficulties presented from the recruiting point of view in detecting the latent and early cases of pulmonary tuberculosis; an early provisional diagnosis of these cases could more often be made by a careful consideration of the history and symptoms than by physical signs. Candidates for the services generally admitted little that they considered likely to prejudice their prospects of entry.

He stated that candidates were questioned as to:—

(a) *Tuberculosis infection in their families*.—Those who had been exposed to the risk of massive infection were rejected.

(b) *As to past illnesses*.—Those who gave a history of the following were rejected:
(1) Past tuberculosis infection; (2) pleurisy; (3) chronic or recurrent chest troubles;
(4) glandular infection.

The chest was fully examined and any abnormal condition was considered to be a cause of rejection. Mouth-breathers were rejected. Candidates below average weight were usually rejected. The urine was always examined.

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Squadron-Leader J. N. MACDONALD, R.A.F., stated that very little reliance could as a rule be placed on the past medical history given by a recruit. By means of a systematic and methodical examination every endeavour was made to eliminate potential tuberculosis cases before attestation in the R.A.F. He considered that the absence of perineal fat was frequently associated with early signs of impairment of the lungs, and said it would be interesting to ascertain opinions as to the value of X-rays in the early diagnosis of tuberculosis of the lungs.

Sections of Medicine, Otolaryngology, and Laryngology.

Chairman—Dr. ROBERT HUTCHISON (President of the Section of Medicine).

**DISCUSSION ON THE CAUSES, EARLY RECOGNITION,
AND TREATMENT OF NON-TUBERCULOUS MENINGITIS.**

Dr. HUGH THURSFIELD.

The practice of lumbar puncture with the examination of the cerebro-spinal fluid—rare twenty-five years ago—has added greatly to our knowledge of meningitis: yet it has by no means solved all the problems. One of the difficulties which are presented to the clinical physician is the exact seat of the condition which it has been agreed to call "meningismus." This condition presents to the physician the clinical signs of meningitis, the fever, the vomiting, the stupor or coma, the squints, the muscular rigidity, and the delirium which accompany true meningitis, and yet on examination of the fluid no indications of inflammation of the meninges are to be found. This condition is in reality the first stage of a meningitis, in which the defensive mechanism of the tissues is sufficient to ward off the attack and render it futile. The next stage, which is no doubt familiar to our aural colleagues is the so-called serous meningitis, in which the cerebro-spinal fluid no longer remains quite clear, but contains a slight excess of albumin and an equally slight excess of cells, either of the lymphocytic or the polymorphonuclear variety, but is sterile on culture. Such a fluid is often found in patients who present the signs of meningitis and yet have a good chance of recovery provided the infecting focus is promptly dealt with. The third stage is that in which, with the other signs of inflammation, there is also found in the fluid evidence of bacterial invasion either in films or by cultivation. The proof that these three stages represent different phases of the same process is partly clinical and partly experimental. In the earliest stages of a true meningitis of bacterial origin, for example epidemic cerebro-spinal meningitis, a clear normal fluid is occasionally found and the same is true of the very earliest stages of a tuberculous meningitis; and in the same case the process can be traced through all the three stages. Experimentally the same facts can be produced.

Passing to the causes of meningitis there can be little doubt that next to tuberculous meningitis, which is by far the most frequent, cerebro-spinal meningitis and meningitis as a complication of ear disease are the most often met with. But there is hardly an organism of the pathogenic class which has not been occasionally found in the meninges, with the possible exception of that of diphtheria, and the foci from which the original infection reaches the meninges may be situated in the most diverse parts of the body.

The diagnosis of the condition must depend, not on clinical signs solely, nor on the examination of the cerebro-spinal fluid alone, but on the consideration of both together. At the same time it is difficult to attain certainty even when the fluid offers some evidence of irritation. Clinically, as I have indicated, meningismus is not meningitis, and yet also there are cases in which the fluid gives no better indication of inflammation although there is certainly meningitis present. The indications on which one has learnt chiefly to rely in the more doubtful cases, are a slight increase in the protein content and in the number and character of the cells present. Clinically, the difficulties are chiefly in the differentiation of tuberculous meningitis from other causes of irritation of the meninges; and next in the recogni-

2 Thursfield—Milligan: *Discussion on Non-Tuberculous Meningitis*

tion of the earlier stages of the infection. The irritation which accompanies the lesions of polio-myelitis in some instances, or of the early stages of encephalitis, produces disturbances in the fluid and in the clinical symptoms which may give rise to difficulty; but on the whole the mistakes made are few, and rather in the direction of failing to recognize the existence of a meningitis which accompanies the last stages of a severe illness than otherwise.

As to treatment, the establishment of the presence of an infection by a known pathogenic organism, by the examination of the cerebro-spinal fluid, may enable us to administer the appropriate serum by the various channels either by means of the theca, by cistern puncture, by ventricular puncture, or by the intravenous route. In the case of the milder streptococcal infections, there is evidence from many sources that repeated lumbar puncture alone is sometimes sufficient in itself to secure a favourable result, but such cases are certainly exceptional. In the more chronic infections vaccine-therapy with sensitized vaccines has had some success, and it is possible that a further step may be made by the transfusion of the blood of a person immunized against the infecting organism.

SIR WILLIAM MILLIGAN.

Speaking generally, from 5 to 6 per cent. of cases of meningitis from all causes seen in a general hospital are of otogenic, and from 1 to 2 per cent. of rhinogenic, origin.

As the result of an acute infective otitis media, the meninges are the structures most likely to suffer, as the result of a chronic process the lateral sinus, the cerebrum, the cerebellum and the meninges. The rupture of a subdural cerebral or cerebellar abscess—the consequence of a septic ear lesion—may also be followed by diffuse purulent infection of the subarachnoid spaces or ventricles. Of the intracranial complications of otitic origin, the early recognition of meningitis is the most difficult and yet the most essential for successful treatment.

Sinus thrombosis is indicated by violent oscillations of temperature and frequently by marked local pain, more especially when there is an associated periphrlebitic abscess; brain abscess by a temperature, subnormal as a rule, and by violent cephalalgia, whereas commencing meningitis, apart perhaps from a rise of temperature to 101-102° F. and possibly a rigor, may show no definite diagnostic symptoms for a few days until an increase of intracranial tension begins to tell its tale.

In young children more especially, among whom meningitis is so often basal in type, the onset of infection is frequently insidious, the pathway being along the venous channels and nerve sheaths of the middle-ear area. Moreover, the imperfect ossification of the walls of the Fallopian aqueduct offer but slight resistance to infection, hence the more frequent presence of facial paralysis in children than in adults.

A point of cardinal importance to bear in mind is that the meningeal infection starts from the immediate area of the primary focus of disease. Hence the importance, in any suspicious case, of freely exposing the middle-ear cleft, tympanum, aditus and mastoid antrum, whether the case be acute or chronic. The free and early removal of the area of infection will quite frequently be sufficient of itself to ensure an immediate disappearance of a threatening meningitis, and is the first essential in dealing with cases of otitic meningitis, serous or purulent.

Luckily for mankind, the dura mater is a resistant structure capable of warding off the invasion of micro-organisms for a considerable period, and of responding by a process of gradual thickening. In meningitis the result of acute septic otitis media, the infective process is usually conveyed by way of the blood or lymphatic stream, or through sutural lines, e.g., the petro-squamosal. In chronic septic ear infections, osseous lesions, e.g., caries, cario-necrosis or actual necrosis with softening and ultimate ulceration of superimposed soft tissues, are the usual sequence.

While the infective process may remain outside the dura for a time, pathological changes may be taking place on its inner surface. The arachnoid membrane, also a resistant membrane, may successfully repel the passage of organisms throughout that interval of time during which Nature asserts herself by a marked increase in the amount of fluid within its cavity in the immediate neighbourhood of the focal lesion. This constitutes the commencing stage of a serous or protective meningitis, a type of meningitis which tends to spread slowly, largely owing to the intricate configuration of the subarachnoid trabeculated spaces.

At the commencement the fluid is clear and under pressure, but later becomes turbid, sometimes very turbid, as the result of an increasing mononuclear pleocytosis. There is also an increase of albumin and globulin and an absence of the normal reduction of sugar. It is, however, sterile.

G. J. Jenkins (*Journal of Laryngology*, June, 1923), says: "If an inflammation of the meninges arises secondary to and due to septic disease of the ear, then it must be regarded as a septic meningitis whether or not an organism has been found in the cerebro-spinal fluid."

With this dictum I am unable to agree. The fluid thrown out I regard as a protective effort on the part of Nature. If we are to regard all such cases of meningitis as cases of septic meningitis, then many cases of septic meningitis are cured, but my opinion is that so long as the cerebro-spinal fluid is sterile, these cases should not be classified as septic meningitis.

As a rule we rely on the results of lumbar puncture, but I wish to point out that lumbar puncture, although undoubtedly the most convenient procedure, is not necessarily the most reliable. The "tap" is far away from the primary focus of disease, and as the diffusive process is slow owing to the sluggish movements of the cerebro-spinal fluid, the character of the fluid removed is not necessarily the same as that in the immediate neighbourhood of infection where tapping *in situ* will at times show a fluid much richer in cellular content. In other words, the fluid is not necessarily homogeneous throughout its entire extent.

The prognosis in serous or protective meningitis is essentially good, always provided that the primary focus of infection and any secondary intracranial lesion, if present, be eliminated and means be taken by the withdrawal, repeated if necessary, of a sufficient amount of fluid to prevent fatal intracranial pressure.

As an aid to diagnosis lumbar puncture is of the utmost value in all cases. As a therapeutic agent in meningitis serosa it is invaluable, but in meningitis purulenta it should never be regarded as an effective means of securing drainage. Information derived from a cell count supports the following conclusion, however—the higher the cell count, the greater the probability of organisms being present.

How long the fluid will remain sterile depends upon the success attending removal of the focus of infection, whether extra- or intracranial. I have records of thirty-nine cases of meningitis serosa in which recovery took place after the elimination of the primary focus and after tapping. The question of real difficulty in otitic meningitis centres, however, around the problem as to when definite purulent infection takes place and as to whether a meningitis is to be looked upon as a strictly septic meningitis when no organisms, or at least viable organisms, are found in smears or cultures from fluid taken either from the lumbar theca or from the immediate neighbourhood of the septic focus. The point may perhaps be regarded as rather an academic one, but to my mind it has a definite practical importance. For I believe that the presence of viable organisms renders the prognosis infinitely worse than when pus cells alone are found; because in the latter instance we have to fear a fatal result mainly from increased intracranial pressure, in the former from pressure and toxæmia combined. In fact, I venture to say that otitic meningeal cases in which recovery has taken place should not be classified as cases of diffuse septic lepto-meningitis,

in spite of the cell count, unless bacteria have been found. I am quite aware of the fact that bacteria may be present and yet may not be found, but this is the exception and not the rule. My personal experience of operating upon septic leptomeningitis with bacteria in the fluid is unfortunate. In only four cases was there recovery.

I suggest that clinically there are three types of meningitis: (1) Meningitis benigna or serosa; (2) meningitis semi-maligna; (3) meningitis maligna. In the first the prognosis is good, in the second grave, and in the third very grave.

All authorities admit that in cases of what is usually described as septic leptomeningitis organisms are frequently not found, and in my search through the literature I find that many of the reported recoveries were in cases in which a turbid or purulent lumbar fluid contained no organisms. I admit that group (3)—meningitis maligna—is the end-result of group (2)—meningitis semi-maligna—but there is a vast difference in the outlook. In the former the exudate is purulent, but without organisms; in the latter the intensity of the process is such that organisms have gained an entrance into the subarachnoid space, demonstrating the persistence or the virulence of the infection.

The presence of dead organisms in the cerebro-spinal fluid indicates to my mind that the tissue fluids of the infected individual are able to deal with the virulence of the infection and that efficient drainage will probably effect a cure.

The practical point appears to be that we should recognize a distinction between the two types with, and without, organisms, the prognosis being so vastly different. Only from 2 to 3 per cent. of cases with organisms in the cerebro-spinal fluid ever recover, whatever is done, whereas a considerably greater percentage of cases without organisms, though with pus cells in the fluid, do recover. The percentage of recoveries would materially increase were the profession to realize that what is so commonly described as septic meningitis is, in a proportion of cases, amenable to treatment, always provided that early drainage is instituted and that surgical interference takes place before bacteria gain access to the fluid.

When the clinical picture suggests the probable presence of meningitis—and in passing I may say that the persistence of pain in any suppurative ear condition where drainage has been effected, should never be neglected—no time should be lost in exposing the dura in the neighbourhood of the area of infection, in puncturing it, withdrawing fluid for examination, and finally in opening it. Many lives are undoubtedly lost because inferences are made from the examination of the lumbar fluid,—a by no means necessarily accurate gauge of what is going on,—the “tap” being too far away from the site of the disease, as I have already indicated. In middle fossa infection sepsis tends to spread slowly in the meshes of the trabeculated spaces, and the employment of strands of gauze or tubes affords perhaps the best means of maintaining drainage. Except in cases of meningitis serosa lumbar puncture should not be regarded as a therapeutic agent. In bacterially infected cases its repeated performance is not without danger, because as a blood infection is apt to take place at an early stage, it is probable that the withdrawal of fluid may cause infected serum from the blood-stream to enter the Pacchionian granulations which lie in intimate contact with the intracranial venous sinuses.

Labyrinthitis, which is responsible for probably 30 per cent. of cases of otitic meningitis, tends to mask the onset of meningitis, but when meningitis is diagnosed as secondary to a labyrinthine infection translabyrinthine drainage, as first suggested by West and Scott, is the operation of election. As the result of labyrinthine sepsis the cisterna pontis becomes involved; this involvement is manifested by symptoms of increased intra-cranial pressure, sickness, headache, Kernig's sign and impaired or absent knee-jerks. Immediate drainage of the cisterna is therefore urgently demanded. A labyrinth exenteration should be first performed and the internal auditory meatus opened up after removal of the columella. Drainage of the cisterna

pontis is maintained by the insertion of a tube passed through the internal auditory meatus. E. D. D. Davis suggests that suction should also be employed, but of this procedure I have no experience.

In advanced cases of lepto-meningitis (meningitis maligna) where pus and bacteria are found in the cerebro-spinal fluid and bacteria in the blood, the prognosis is extremely grave, and the chances of successful surgical interference remote. As autopsies show that in advanced cases the cisternæ are invariably infected, an effort should be made to drain (1) the primary focus of infection, (2) the cisterna pontis, (3) the cisterna basalis, (4) the cisterna magna.

The cisterna basalis is best drained by passing a tube along the upper border of the pars petrosa to the cistern, while the cisterna magna may be drained by Haines' operation or occipito-atlantal puncture. In the three cases in which I have performed Haines' operation death took place. Possibly if the operation had been done at an earlier stage of the disease the result might have been different, although I doubt it, because by the time bacterial fluid finds its way into the cisterna magna, the prognosis has become very grave.

Infection of the posterior fossa, which is distinctly more common than that of the middle fossa, arises from such lesions as suppuration of the posterior end of the middle-ear cleft, cerebellar abscess, sinus phlebitis or abscess of the saccus endolymphaticus. Infection tends to spread slowly between the upper surface of the cerebellum and the under surface of the tentorium, and eventually reaches both the cisterna pontis and cisterna magna. The cerebellar lobe in front of and behind the lateral sinus should be freely exposed, and the exposure followed by incision of the dura and the insertion of strips of gauze. This may be combined with irrigation of the subarachnoid spaces following lumbar puncture.

My experience leads me to believe that lives might be saved were we to classify cases of meningitis more strictly in accordance with pathological findings. Were we definitely to realize that so long as the cerebro-spinal fluid is bacteriologically sterile there exists a good chance of recovery; given immediate operation and continuous drainage our results would be better. At present there is a general impression among doctors that what is called septic meningitis is incurable, and hence they refrain from any attempt to remove the primary focus and drain the subarachnoid space or cisternæ.

CONCLUSIONS.

- (1) Meningitis is a frequent complication of septic otitis media.
- (2) Clinically three types should be differentiated :—
 - (a) The serous, with a sterile although often turbid cerebro-spinal fluid—meningitis benigna.
 - (b) The semi-malignant : a cerebro-spinal fluid with a rich cellular content but no organisms—meningitis semi-maligna.
 - (c) The malignant, with a cerebro-spinal fluid rich in cellular content and with organisms present—meningitis maligna.
- (3) An accurate diagnosis of the type of meningitis present cannot be made from a review of the clinical symptoms only.
- (4) A cytological and bacteriological examination of the cerebro-spinal fluid is essential and is the determining factor in prognosis and treatment.
- (5) The essential element underlying successful treatment is early surgical interference, the removal of the original focus of infection and of any secondary intracranial lesion, combined with free drainage of the subarachnoid space and cisternæ.
- (6) Whenever possible, fluid for examination should be withdrawn from the immediate area of disease.
- (7) A fluid with a high cellular content should not negative surgical interference.
- (8) A fluid with a high cellular content and bacteria—more especially streptococci or pneumococci—almost invariably portends early death.

6 Worster-Drought: *Discussion on Non-Tuberculous Meningitis*

Dr. C. WORSTER-DROUGHT.

In considering the *causes* of meningitis, I would specially draw attention to acute and sub-acute syphilitic meningitis. This meningo-vascular form of neurosyphilis, though somewhat rare, may develop within two or three months of the primary chancre, and, in its clinical aspects, closely resemble other forms of acute meningitis. During the war, when in charge of a large cerebro-spinal fever centre, I admitted three cases as suspected examples of cerebro-spinal fever (meningococcal meningitis), which subsequently proved to be suffering from syphilitic meningitis. It is only by an examination of the cerebro-spinal fluid that the diagnosis is established. The fluid is usually clear, but contains excess of lymphocytes, the presence of a strongly positive Wassermann reaction distinguishing it from the cerebro-spinal fluid of tuberculous meningitis. The blood also yields a positive Wassermann reaction. With treatment the prognosis is very good. In all three cases I have mentioned above recovery was complete, following a course of intravenous novarsenobillon, and mercurialized serum administered intrathecally.

With regard to meningitis following mumps, I would point out that some observers (Massary, Bonaba¹) state that a lymphocytosis is present in the cerebro-spinal fluid of every case of mumps, whether physical signs of meningeal involvement be present or not. This assertion led me to investigate the cerebro-spinal fluid of some thirty examples of apparently uncomplicated mumps. The lumbar puncture was performed as soon as the case came under observation—varying from the first to the tenth day of illness—and I must confess that in no case, apart from definite meningitic signs, was a pleocytosis in the cerebro-spinal fluid ever found.

Concerning those forms of meningitis which are of oto-rhinological interest, the cases I have encountered may be classified as follows:—

(1) Meningitis following intranasal operations. I have met with several examples following simple submucous resection of the nasal septum or the removal of nasal polypi. In such cases meningitic symptoms have usually appeared in sixteen to twenty-one days after the operation, and the organism obtained from the cerebro-spinal fluid has been the streptococcus, or, more rarely, Pfeiffer's bacillus. When autopsy has been possible, empyema of the sphenoidal sinus has invariably been found.

(2) Meningitis, secondary to intranasal or accessory sinus disease apart from operation. Cases met with include one following suppurating nasal polypi and several secondary to empyema of the sphenoidal sinus. In the latter, the pneumococcus has usually been the responsible organism.

(3) Meningitis following wounds of the jaws. My experiences during the war led me to describe these cases under two headings.²

(a) *Wounds involving the upper jaw—meningitis by direct extension of infection.*

In wounds affecting the superior maxilla, meningitis usually occurred by the direct spread of infection from the wound to the subarachnoid space by way of the nasal fossæ and sphenoidal sinus.

(b) *Wounds involving the lower jaw—meningitis indirectly as a secondary phenomenon to infection of the lungs.*

In cases of meningitis associated with wounds of the inferior maxilla, a broncho-pneumonia or lung abscess had developed as the result of septic inhalation, and meningitis—with or without brain abscess—occurred secondarily by infection through the blood stream.

The organisms isolated were streptococci or pneumococci.

(4) Meningitis secondary to suppurative otitis media.

Meningitis usually results by infection passing through an erosion in the bone of

¹ *Journ. Nervous and Mental Dis.*, 1920, September, p. 274.

² *Brit. Dental Journ.*, 1919, April 1, p. 1.

the tegmen tympani, tegmen antri, the lateral sinus groove, or the posterior surface of the petrous bone. I have met with a few cases of chronic otitis media without mastoiditis, however, in which the infection, as shown by post-mortem examination, has spread along the internal auditory meatus by way of the internal ear. In addition to meningitis, an extradural abscess—between the dura and the inner aspect of the petrous bone—has been found, and facial paralysis, caused by pus in the Fallopian aqueduct, has usually been present.

DIAGNOSIS.

For the early recognition of meningitis, I would emphasize only those signs that I have found most useful in actual practice. Among the symptoms are pyrexia, headache (generalized and of increasing intensity) and vomiting, all of which are practically constant.

The most valuable physical signs are: (1) Rigidity of the posterior cervical muscles; (2) Kernig's sign; (3) the pulse-temperature ratio—comparatively slow pulse-rate with high temperature; (4) the presence of delirium or stupor with irritability; (5) in infants, tension or bulging of the anterior fontanelle.

Rigidity of the neck muscles is most important, and in my experience the first physical sign to develop. This "neck-sign" I have found is best elicited as follows: With the patient lying on his back, the observer places his hand beneath the patient's head and endeavours gently to draw it forwards. In the presence of meningitis (with the occasional exception of the tuberculous variety) the head cannot be brought forwards more than two inches beyond the line of the long axis of the body: more often, indeed, it cannot be flexed past this line. This sign is sometimes present within five to six hours of the onset of meningitis and almost always within twelve hours. Head retraction as a sign is useless if meningitis is to be diagnosed early; it seldom develops before the third or fourth day of illness and in many adult cases is absent throughout the course. Kernig's sign is slightly later in its development than rigidity of the posterior cervical muscles. It is often present within twelve hours of the onset of illness, can usually be elicited within eighteen hours, and is almost invariably positive at the end of twenty-four hours. The sign is of no value in children below the age of two years. Brudzinsky's signs are of secondary importance only, and in my experience very seldom obtained in the absence of Kernig's sign.

No reliance whatever can be placed upon the state of the superficial and deep reflexes for the purpose of early diagnosis.

With regard to the mental state in early non-tuberculous meningitis, initial delirium is usually succeeded by stupor with intense irritability, the patient strongly resenting interference.

Retention of urine, when present, is an important symptom; it sometimes occurs within twenty-four hours of the onset of illness.

These signs serve for the early recognition of meningitis, but the diagnosis can only be established by examination of the cerebro-spinal fluid.

TREATMENT.

Two principles govern the treatment of all forms of non-tuberculous meningitis: (I) *The establishment of adequate drainage for the meningeal exudate.* (II) *The application of a specific immune serum in as close contact as possible with the inflamed tissues.*

(I) Adequate drainage can usually be supplied by the performance of repeated daily lumbar puncture. Since 1915, it has invariably been my routine to follow the period of intrathecal serum administration by repeated daily lumbar puncture, with complete drainage of the subarachnoid space, until the cerebro-spinal fluid has become quite clear to the naked eye. With the use of this method I have encountered no recrudescences nor relapses.

8 Worster-Drought—Greenfield: *Non-Tuberculous Meningitis*

When no cerebro-spinal fluid is obtained by lumbar puncture ("dry-tap") by reason of adhesions existing between the meninges and spinal cord at a higher level, or a progressive increase in density of the pus, one has to consider puncture at other sites.

(a) *Cervical puncture*.—Puncture of the theca just below the seventh cervical vertebra I have found quite a safe procedure provided the needle is inserted without a stylet.¹ Puncture at any level in the dorsal (thoracic) region is not advisable, as in many normal individuals the posterior subarachnoid space is absent in this region owing to adhesions existing between the meninges and the posterior aspect of the cord.

(b) *Puncture of the Cisterna Magna*.—This method of puncture is indicated when no fluid can be drained off at any lower level. Occasionally, when purulent exudate occludes the region of the foramen magnum, even this may fail to yield fluid.

(c) If the above measures are unsuccessful, *sphenoidal puncture*—after the method of Bériel and Cazamian—may be tried. This consists of introducing a needle along the outer wall of the orbit in its upper part and entering the most external portion of the sphenoidal fissure.

(d) *Puncture of the Lateral Ventricles*.—In children with open fontanelles this is an easy matter and may be resorted to at once if lumbar puncture fails to yield fluid. The needle is introduced at the lateral angle of the fontanelle, the direction being downwards, slightly backwards and inwards to a depth of about 3 cm.

In older children and adults the usual method of draining the lateral ventricles is to trephine, a procedure which causes a considerable amount of "shock," especially as it is necessary to operate on each side.

More recently, instead of trephining, I have merely drilled a hole through the skull over Keen's point and punctured the ventricle through the small opening thus made. In most cases it is possible to perform this operation without anaesthesia, the "shock" is negligible compared with that resulting from a trephine, and drainage appears equally effective. In two cases of apparent internal hydrocephalus complicating meningococcal meningitis treated by this method there was recovery.

In otitic meningitis, one cannot fail to be impressed with the success of the trans-labyrinthine method of drainage.

(II) As regards the application of a specific immune serum, the full power of the serum can only be exerted when it is brought into contact with the infecting organisms in a concentrated form. Not only does serum injected subcutaneously or intravenously undergo an extremely high dilution in the blood-stream, but there is no evidence that it ever reaches the subarachnoid space.

Consequently, to exert its maximum effect, serum must be introduced directly into the subarachnoid space; and in order to make good the loss in concentration sustained by absorption into the blood-stream, at intervals of not longer than twenty-four hours. In cases showing evidence of systemic infection, intrathecal should be supplemented by intravenous administration.

The serum should be polyvalent for the specific organism obtained from the cerebro-spinal fluid; later, a univalent serum can be substituted if the actual type of organism is determined.

Dr. J. G. GREENFIELD.

The laboratory diagnosis of meningitis is often extremely difficult, and here, at all events there seems to be a need for "that curious hybrid—clinical pathology," for the pathologist must be enough of a clinician to understand the purport of the history that is given him, and enough of a morbid anatomist to be able, when opportunity offers, to test his conclusions on the post-mortem table. Here, too, the mere performer of reactions soon gets out of his depth, for with the cerebro-spinal fluid,

¹ *Journ. Neurol. and Psychopathol.*, 1921, i, p. 216.

much more than with other body fluids, there is a singular disproportion between the multitude of reactions that can be performed and the limited quantity of fluid on which to perform them. We have, therefore, first of all, to make up our minds what we want to know and then to settle how best to arrive at that knowledge with the means at our disposal. And though reactions will help us in this, we have to be their master and not their slave, and to interpret the results of each reaction in the light of other facts.

My experience of these cases, as they arise from infection of the nasal air sinuses, is not very great, and is limited almost entirely to cases of ethmoidal and sphenoidal sinus suppuration, as these cases tend to fall into the hands of the neurologist almost as frequently as into those of the laryngologist. In fact, the first civil case of this kind that I saw had been treated by psycho-analysis for headache and depression for two months before his death. I understand that several complexes had been unearthed, but this had failed to relieve his headaches, and lumbar puncture was performed. I found no increase of protein in the cerebro-spinal fluid, but 100 cells, about half of which were polymorphonuclears; this was certain evidence of some suppurative mischief in the cranium or brain. He died of generalized meningitis within a few days, and at the post mortem the origin of the trouble was found to lie in the frontal and ethmoidal air sinuses.

The problem may present itself to the clinical pathologist in two extreme forms. There may be either—

- (1) A *clear* fluid from a patient who shows signs of meningitis, or
- (2) A *turbid* fluid from a patient with little or no evidence of meningitis.

(1) Let us look at the first problem, *the decision as to whether meningitis is, or is not, present when the fluid is clear*: or rather whether meningitis is impending, because in certain cases, especially those of septic sinusitis or cerebral abscess, there may be no evidence of actual meningitis, and yet the presence of a few polymorphonuclear cells in the cerebro-spinal fluid warns us that danger is at hand. Under these conditions we have to be very sure that the fluid is perfectly normal in every way before we can say that meningitis is not imminent. In two cases of cerebral abscess, recently reported by Hadfield, only twelve cells were present in the fluid at the first puncture in each case, but of these 70 per cent. in one case, and 75 per cent. in the other were polymorphonuclears. In my experience a much lower percentage than this, even 30 per cent., is a danger signal.

As to the other constituents, in very early meningitis the protein is often normal but in spite of this hæmolysin and complement may be present. The combination of these constituents with a normal protein level is in fact almost characteristic of early meningitis. In my experience the hæmolysin test is treacherous: it may often be negative in cases where it ought by all the rules to be positive, for the simple reason that the patient's blood contains no hæmolysin. It is therefore much better to test for the presence of complement only, by simply mixing the cerebro-spinal fluid with sensitized red cells and incubating at blood temperature.

As a routine I use the same mixture as in the Wassermann test, i.e., 3 per cent. sheep's corpuscles sensitized with plenty of hæmolysin. Of this I use $\frac{1}{4}$ c.c. with 2 c.c. cerebro-spinal fluid. The strength of the reaction is easily estimated by using progressive dilutions of cerebro-spinal fluid with the same amount of sensitized corpuscles. Apart from the cytology, this test appears to be the one which gives the first evidence of meningeal involvement. Like the chlorides, of which I shall speak later, it is dependent on increased meningeal permeability, but whereas we need to have a fairly large area of transference from cerebro-spinal fluid to blood to allow for a lowering of chlorides, a very small area suffices for the passage of complement from the blood into the cerebro-spinal fluid. As far as my investigations have gone at present, the chief value of this test is found when there is no appreciable increase of protein and only a slight cellular excess. It is so constantly

positive when larger amounts of protein are present that its meaning could only be arrived at by a comparison of the quantities of protein and of complement, and my statistics are not yet sufficient for this purpose.

I have just said that we must be sure that the fluid is normal in every way, and sometimes an examination of the urine shows that symptoms which were attributed to meningitis were really due to uræmia.

(2) *Cases with Little Evidence of Meningitis showing a Turbid Cerebro-spinal Fluid.*—In my experience these cases present the greatest difficulty. We are given a turbid fluid looking typical enough of meningitis, and yet there is the clinical doubt as to this diagnosis, and as we examine the fluid some pathological doubt arises also. Fluids of this kind are particularly frequent in cases of empyema of the cranial air sinuses and of abscess of the brain resulting from them. Obviously there is pus somewhere about, and some of it has found its way into the subarachnoid space. There is danger of meningitis becoming generalized, indeed it may be generalized already, or it may be limited to the region of the abscess. That, in fact, is the question which we hope an examination of the cerebro-spinal fluid will solve. If the meningitis is generalized the chances of recovery are greatly reduced. If it is localized the prognosis depends altogether on whether or not the surgeon can deal satisfactorily with the local condition, and the presence of pus in the cerebro-spinal fluid does not matter relatively.

The prognosis, it is true, rests to a large extent on the presence of bacteria in the cerebro-spinal fluid and on the virulence of the invading organism when this can be established. So far as I know, the pneumococcus always kills when it reaches the subarachnoid space and the same may be said for the more virulent forms of streptococcus, although it is not always easy to say which types of streptococcus are highly virulent and which are less so. Therefore if we can grow either of these organisms on culture media the prognosis must be bad. But there is a large group of cases which give, on lumbar puncture, a purulent fluid that either shows no organisms on culture, or grows a few colonies of some less virulent bacterial type. Fluids of this kind always contain excess of protein and usually no sugar, and in my experience neither of these constituents is of any value at all for prognosis. The only test which gives us any indication in such cases as to whether or not the meningitis is generalized is the chloride percentage, and this is, I think, of real value. My reliance on this test is based primarily on Mestrezat's work, and secondarily on a series of seventeen cases of head wounds which I was able to follow after the end of the war at Tooting Military Hospital. In these patients lumbar puncture was performed for a variety of reasons, chiefly because of headache. Some had papilloedema or bulging craniotomy openings. Of nine that recovered the cerebro-spinal fluid was perfectly normal in six. Of the other eight that died, seven died from meningitis, and in all of these seven cases the chlorides fell to below 680 mgm. before death. In one case, in which the patient recovered, there were pus and Gram-negative diplococci in the spinal fluid, but the chlorides remained at the normal level of 725 to 750 mgm. This series impressed me so much that since then I have kept a watch on the chlorides in every case of suspected meningitis, and I have never known a patient die from meningitis with the chlorides at their normal level. (It applies chiefly to the lumbar fluid and may not be so absolutely true for the cisternal fluid.) This means that the chlorides always fall when meningitis becomes generalized, and therefore so long as they remain high there is ground for belief that the area of inflammation in the meninges is localized and for hoping that it may remain so.

From what we know of the cerebro-spinal fluid this fall in the chlorides is due to a breaking down of the barrier which separates blood and cerebro spinal fluid: as Mestrezat termed it, there is an *increase of meningeal permeability*. In health the chlorides in the cerebro-spinal fluid are always higher than in the blood, but when the barrier between the two fluids is reduced, their chloride percentages tend to

approximate to one another. This meningeal permeability is increased enormously in craniotomy operations when the dura mater is opened, and serum from the tissues mixes freely with cerebro-spinal fluid. Consequently we find that after craniotomy, especially after decompression of the posterior cranial fossa, the chlorides are often reduced and under these conditions they give us no help in determining the presence or absence of meningitis.

In my experience a turbid sterile fluid is of less dangerous import than a clear fluid showing a few polymorphonuclear cells. The first usually means that the acme of meningeal inflammation has been reached or passed, whereas the second may mean anything and is often the first sign of an acute generalized meningitis.

I would say in conclusion that if I have laid special emphasis on the value of certain reactions, I do not wish that they should displace those in more general use. Protein and sugar estimation are still of value although they often do not tell us much that we cannot see by simply looking at the fluid. My intention has rather been to induce pathologists to complete the examination of the fluid in cases of suspected meningitis by using such tests as will give them the greatest amount of information in each particular case. It is of as little use to estimate the chlorides in a clear fluid as it is to test a turbid fluid for complement. Each case and each specimen of fluid should be taken on its own merits. Sometimes it is possible to say within five minutes that a patient has generalized meningitis, and sometimes an hour's examination is not sufficient to decide the matter, but at any rate an intelligent examination of the cerebro-spinal fluid ought to enable us to get a little nearer to an exact diagnosis.

Mr. W. M. MOLLISON.

Meningitis of nasal origin occurs as a primary and sole complication of injury to the roof of the nose as in fracture of the anterior fossa of the skull involving the cribriform plate; or as a result of damage to the cribriform plate by direct violence during intranasal operations: more often it is the sequel or end-result of some other intracranial complication of nasal disease, such as pachymeningitis, cerebral abscess, cavernous sinus thrombosis, or osteomyelitis of the frontal bone. Before passing to the details of the origin of meningitis, the paths of infection must be mentioned.

Gross lesions of the cribriform plate, from whatever cause, open a direct path from the nasal cavity to the meningeal space, and the course of infection is obvious, especially if there is pus in the nose due to accessory sinus disease. In the absence of such direct route the infection can take one of the following paths:—

(1) *Direct spread, through the bone* by an osteomyelitis or osteophlebitis of the cerebral wall, till infection reaches the dura mater. When the infection reaches the dura pachymeningitis may result, or it may spread at once through the meninges.

(2) *Venous spread.* Microscopically, thrombosis can be shown to be responsible for the spread of disease through bone, but infection by way of the venous anastomoses at the root of the nose is a real danger and no doubt accounts for the thrombosis of the cavernous sinus. Diploëtic veins may also convey infection.

(3) *Lymphatic spread* is doubtless fairly frequent. Lymphatic capillaries pass directly from the nasal mucous membrane to the meninges.

(4) A rare path is that through *congenital defects in the posterior wall of the frontal sinus*, but such paths have been recorded.

I omit the meningitis which is a sequel to fractures of the anterior fossa, and pass to that associated with suppuration in the accessory sinuses. There are two groups: (a) Non-operative; (b) operative.

(a) *Non-operative meningitis* generally occurs in the course of chronic disease. In the majority of cases there is some other complication present, extradural abscess,

cerebral abscess, or thrombosis of the cavernous or superior longitudinal sinus. The frontal and sphenoidal sinuses are those most often the seat of the original disease.

(b) *Operative*.—This group must be divided into two sub-groups: (1) Those cases following external operations on the frontal or ethmoidal sinus; (2) those cases following intranasal operations.

(1) The earlier experience of external operation for frontal sinus suppuration was unfortunate, but with the improvement of technique the percentage of sequent meningitis has greatly diminished.

(2) It is the sub-group in which meningitis follows intranasal operation which chiefly concerns rhinologists.

The number of intranasal operations performed must be very large, but the cases of meningitis are few. Bruhl noted but one case out of 35,000 operations. On the other hand Jansen saw three fatal cases among 800 intranasal operations, and it must be remembered that many such are unpublished. Most cases have followed operations for the relief of sinus suppuration, but a few have occurred after simple submucous resection, removal of the inferior turbinate or cauterization of the middle turbinate. In some instances an operation on the nose has lighted up a pre-existing but unsuspected pachymeningitis, and occasionally the onset of the meningitis has been delayed for months. Loeb has reported 125 cases of meningitis following intranasal operation, and there appears to be no type of operation which may not give rise to the meningitis.

The prognosis of post-operative meningitis is bad, but some recoveries have been reported: Gerber, Piffel, Coffin, Parry, and Blum have recorded recovery and I have had two recoveries out of three cases.

The diagnosis of rhinogenic meningitis does not present material differences from that of otogenic meningitis. The signs are the same and there is no difference to be noted in the condition of the cerebro-spinal fluid. It may be remarked that rhinogenic meningitis as a rule runs an extremely rapid course.

Treatment is of small avail when once the disease is established, especially when intranasal operation has been the immediate cause. The best is to obviate the necessity for treatment, by attention to asepsis and avoidance of any damage to the cribriform plate. Katz gives the following useful rules:—

- (1) Never operate when the patient has an acute cold.
- (2) When possible open all the sinuses containing pus, at one sitting.
- (3) Never plug for more than twelve hours.
- (4) Treat the middle turbinate gently.
- (5) Avoid the olfactory sulcus.

Mr. W. STUART-LOW

said that most of the disastrous results such as septic meningitis were post-operative, and in his opinion were largely preventable by taking greater pains in the proper preparation of the patient for the mastoid operation and by improved technique at the time of operation.

Cases of septic meningitis, still too frequently met with after mastoid surgery, were to some extent a reflection on the surgeon, but there was this extenuating circumstance, that often very septic cases came into the surgeon's hands for immediate operation, and there was no opportunity of preparing the patient, as meningitis might have already begun.

Since he (the speaker) had begun to be scrupulously careful to prepare the patient for operation by rendering the throat, nose, and ear as aseptic as possible by the use of antiseptics, extracting carious teeth, removing tonsils and adenoids, ablating all suppurating foci in the nose, and by the systematic employment of suction in the ear to extract all discharge, he had had no cases of septic meningitis.

Suction was not yet being used as it ought to be, not only before operation on the

ear so as to get a clear field for the operation, but also at the time of operation to remove all deeply seated pus, blood and debris arising during operation. This technique contrasted favourably with the usual method of mopping up, which was still generally practised. Another point in the technique of the mastoid operation that he (Mr. Stuart-Low) found most conducive to a good result was the employment of normal aseptic horse serum as a first dressing. Being of lower specific gravity than the blood it induced a great out-pouring of fluid from the newly-exposed bone, and this flushed the wound and encouraged the leucocytosis, which was so essential to healing.

It was admitted that lumbar puncture was not free from danger. Only recently fatal cases had been reported in the medical journals together with grave warnings against the use of tubes of too large calibre and against repeated puncture. He (the speaker) found dry-cupping a good, efficient and safe substitute for lumbar puncture. The cups should be large and applied under the occiput and along the spine, and kept in position some time, and their application repeated again and again.

MR. SYDNEY SCOTT

said he wished to lay stress on the fact that meningitis might occur with no clinical indications whatever until some days had elapsed; the proof of meningitis was the presence of organisms in the cerebro-spinal fluid. This knowledge made early lumbar puncture absolutely essential in all cases in which there were grounds for suspecting meningitis. He had come to realize that in a surgical case with infection in the upper air passages or in the ear, with any rise of temperature for which no obvious cause could be found, lumbar puncture should be performed. Only in that way could the earliest onset of meningitis be discovered.

He was surprised when authorities in medicine decried lumbar puncture as a therapeutic measure. He had never seen a case of pyogenic meningitis recover without repeated lumbar punctures. He could himself recall thirteen cases of pyogenic meningitis (proved by pathological examination of the cerebro-spinal fluid) in which there was recovery after that procedure, and he was sure there were many other successful cases. The organisms had always been either streptococci or pneumococci in the cerebro-spinal fluid.

Dr. Greenfield had mentioned the frequently favourable prognostic import of a high cell count if no bacteria were found in the cerebro-spinal fluid, and that had also been their experience. He was sure Sir William Milligan would agree, for he had more than once raised the question, elsewhere, as to whether a large cell count in bacteria-free cerebro-spinal fluid should be invariably regarded as evidence of leptomeningitis. Certainly the prognosis in the two classes of case was quite different. The proportion of recoveries from so-called serous meningitis would be higher if we included those cases in which bacteria were not found in the fluid, than among that series in which the term meningitis was restricted to cases in which organisms were found in the cerebro-spinal fluid.

Personally he felt encouraged to hear Dr. Worster-Drought counsel repeated lumbar puncture. He (Mr. Scott) had been convinced of its efficacy for years, and would not allow a longer interval between punctures than twenty-four hours as a general rule; he often preferred twelve or eighteen-hourly intervals, and repeated the process as long as improvement was manifest until the cells and organisms disappeared and the patient was free from pyrexia and pain.

Dr. Greenfield also mentioned the fact that, as a pathologist and morbid anatomist, he frequently discovered ethmoiditis and sphenoiditis, which had not been diagnosed during life. There was need to emphasize the fact, recognized by rhinologists, that inspection of the nose might fail to reveal (in certain cases) that the patient had suppuration in the ethmoidal or sphenoidal cells. X-rays and exploratory puncture were often essential aids to diagnosis, as Dr. Patrick Watson-Williams had so frequently emphasized.

Dr. DAVID NABARRO

said the experience he had had in connexion with non-tuberculous meningitis had been mainly obtained at the Hospital for Sick Children, Great Ormond Street, where, as Dr. Thursfield had already said, most of the cases of meningitis were tuberculous, a smaller number being meningococcal, with a sprinkling of other kinds, such as streptococcal, pneumococcal, and influenzal meningitis. One or two cases were due to rarer bacilli.

It was unnecessary now to stress the importance of lumbar puncture in arriving at a diagnosis. Only on the previous night a case was brought into the Children's Hospital in which lumbar puncture was done and a turbid fluid withdrawn. The house-physician thought it was meningococcal, and asked the speaker whether he should inject serum. In a few minutes, by examination, it was found to be not meningococcal, but influenzal. There were a few extra-cellular bacilli, and they were grown that morning, and he had done a blood culture a few hours ago. There was no doubt about the diagnosis. Many cases began as septicæmia. That called to mind one of the probable means by which meningitis was brought about. A stimulating paper on the subject by Dr. Lewkowiez appeared in the *Lancet* of September 6, 1924, p. 487. That writer considered that most cases of meningitis began as a septicæmia, and were instances of choroido-ependymitis. There were probably embolic foci in the choroid plexus, and the ventricle was the first part of the brain to be involved; thence the flow of the cerebro-spinal fluid carried the infection first of all to the base of the brain. If, as was conjectured, cases might begin as septicæmia, then the child must be treated from the general point of view, as well as from the standpoint of the meningeal infection.

Another point of importance concerned treatment by urotropin. The late F. E. Batten recorded five cases of influenzal meningitis at Great Ormond Street, and the only one in which there was recovery was one in which urotropin 10 gr. was given every four hours for several days. In such a fatal condition the success of the treatment in this case was significant.

With regard to the presence of other organisms in the cerebro-spinal fluid, last year he had a very interesting case in that it illustrated the difficulty with which one was confronted even in carrying out pathological investigations. A child aged 5 weeks was brought in with meningitis, diagnosed as meningococcal. The albumin was 0.8 per cent., and there were many Gram-negative intracellular cocci present. He (Dr. Nabarro) thought it was meningococcal meningitis. Cultivation resulted in the organism growing in a day or two like meningococcus. In the films the intracellular organisms reminded him of a urethral film in the acute stages of gonococcal infection. He sent the culture to Dr. Atkins, of the Lister Institute, who thought at first it was more like gonococcus than meningococcus. Later still Dr. Atkins studied it further, and then said it corresponded exactly to the *Diplococcus crassus*; it was partially Gram-positive, and grew well at room temperature.

Another interesting case was that of a child who came to hospital suffering from recurrent attacks of meningitis; there was a diagnosis of recurrent pneumococcal meningitis. The cerebro-spinal fluid which was withdrawn was found to contain a large proportion of protein, 0.18 per cent. albumin, and sugar 0.68 per cent. The cells were chiefly mononuclears, and the culture looked like pneumococcus. It was not pathogenic to animals. He sent the culture to Dr. Crowe, of Harrogate, who reported that it was undoubtedly a rare streptococcus, not a pneumococcus. The clinical interest of the case was the fact that the patient had now left the hospital, cured for the time being. The patient had attacks every three months, and was also the subject of spina bifida occulta.

Referring to the question whether patients could recover from pneumococcal and streptococcal meningitis, he said he believed that from the first of these there was

scarcely ever recovery. In regard to the streptococcal form, there had been very few cases of recovery at the Hospital for Sick Children. He recalled instances in which a nurse and a child were believed to have caught the disease from another child. *Streptococcus mucosus* was isolated. The nurse recovered, but the child died.

As to the diagnosis of meningitis in association with cerebral abscess and ear disease, some cases of that complication were seen at Great Ormond Street. He had not styled them septic meningitis if organisms failed to grow or had not been seen in films. But it was possible for the cerebro-spinal fluid to contain a good deal of protein and a considerable number of cells, and yet be sterile in regard to micro-organisms.

Dr. W. E. CARNEGIE DICKSON

said he was very glad to hear Dr. Nabarro make reference to another possible path of infection, namely, by the blood-stream and ventricles. He had hoped that, before he left the meeting, Dr. Charles Gray would have spoken. During the war Dr. Gray was his colleague at the Fulham Military Hospital, where meningitis cases were dealt with for the London district. Dr. Gray and he both believed that very probably the meningococcus attacked the ventricles by way of the choroid plexuses *through the blood-stream*, to which the meningococcus might obtain access from the ethmoid cells or the sphenoidal cells, or the roof of the naso-pharynx, rather than by direct spread through the bone and meninges. At that hospital he did 100 post-mortem examinations on meningitis cases, and he was struck by the fact that inflammatory reaction in the ventricles was, almost always, in excess of the lesion on the surface of the brain. Writing in the *British Medical Journal*, of April 7, 1917, p. 454, he put that fact strongly in a preliminary note on these post-mortem examinations. The meningitis on the surface of the brain was just where the infected fluid in the ventricular chain approached the surface, e.g., in the neighbourhood of the great transverse fissure, the pial roof of the fourth ventricle and the floor of the third ventricle and infundibulum; and these were the sites where the earlier meningeal exudate was seen, and where it would naturally come to the surface if it attacked the brain from within, rather than from without.¹

Dr. Greenfield had discussed the pathological side of the subject so thoroughly that there was little else to say about it. He (the speaker) had come across instances of rare infection, such as by paratyphoid bacilli, and "leptothrix" and influenza-like organisms: the two last-named groups he considered still needed a good deal of working out. He had been struck by the distribution in the "leptothrix" cases: It was over the forehead, i.e., over the anterior and the under aspects of the frontal lobes, especially over the ethmoid region, differing therefore from the posterior basal distribution of the meningococcal infection. Had Dr. Nabarro noted the same at Great Ormond Street?

Two of the most striking cases of meningitis on which he had made post-mortem examinations were in cretins. In one of the cases the cretin child was sitting up in bed playing with its toys, its intelligence at the time appearing to be about the usual, when it suddenly fell back, dead. At the autopsy there was found to be very extensive meningitis. The end of the other cretin was very similar. One case was tuberculous, the other pneumococcal. Thus meningitis might be masked by other conditions, of which cretinism was one.

With regard to immuno-therapy, he had hoped Dr. Gray would have said something on the use of blood-serum taken from the patient himself, or from other persons who had recovered from meningococcal meningitis. When the specific serum supplied by the Government seemed for a time to deteriorate in quality, during the war, Dr. Gray used serum from recovered patients—sometimes from the patient himself, and the results so obtained were fairly good.

¹ See Beattie and Dickson's "Special Pathology," 2nd ed., 1921, pp. 955-6.

Dr. ROBERT HUTCHISON (President of the Section of Medicine,
Chairman),

said he thought everyone would agree that the discussion had been a very satisfactory one, and it illustrated the advantage accruing from different Sections, each from its special standpoint, joining in the exchange of views and experiences concerning a certain set subject. On this occasion he had heard much which was new to him. No doubt otologists and others had learned something from the pathological presentation, and the pathologists had learned a good deal from the views expressed by representatives of the other Sections.

Apparently, all the speakers were agreed as to the great difficulty in the clinical diagnosis of meningitis in the early stage, as distinct from meningismus—if there were such a condition, and if it were not merely a question of degree.

He was able to speak of the fallaciousness of many of the clinical signs, so-called. Dr. Worster-Drought, for instance, mentioned rigidity of neck muscles as an early sign of meningitis; but he (the speaker) had several times seen it present in acute rheumatism in children, and in a degree closely simulating that in meningitis, therefore it was not a trustworthy sign for purposes of differentiation. He welcomed the very exhaustive and admirable account of the help which could be obtained from the performance of lumbar puncture, and the significance of the findings, which Dr. Greenfield had stated. And the Society was especially indebted to him for having emphasized not merely the importance of bacteriological examination of the fluid, but of the cytological examination, and of taking all points of view in arriving at a conclusion. To give a sound opinion in some cases required a man with the kind of experience Dr. Greenfield had had, as it was matter for an expert. It was agreed that more help in these cases would be derived from the pathologist than had been the case in the past.

Referring to treatment, he (the Chairman) said he had not gathered very much from the debate. Dr. Worster-Drought described various methods, but was discreetly silent about the statistical results which had accrued. If organisms could be found in the fluid, the outlook was distinctly bad; that was agreed. Therefore the great need was to prevent the occurrence of meningitis. It was not a question of what the physician could do, because the cases he saw were not those arising from a septic focus; they were, rather, expressions of a general infection, or a septicæmia. Those following rhinological procedure should, theoretically, be preventable.

**Sections of Psychiatry, Neurology, Study of Disease in
Children, and Epidemiology.**

Chairman—Dr. R. H. COLE (Vice-President of Section of Psychiatry).

**DISCUSSION ON THE MENTAL SEQUELÆ OF
ENCEPHALITIS LETHARGICA.**

Dr. EDWIN BRAMWELL.

OUR conception of epidemic encephalitis has, you will agree, undergone very considerable modification since we met with the earlier cases in the spring of 1918. At that time I think I am right in saying that we regarded the disease as a more or less acute affection, which, should the patient survive, leaves residua in its wake somewhat after the manner of acute poliomyelitis, although more or less complete recovery might be eventually anticipated in the majority of cases. Further experience has however served to demonstrate that the clinical pictures are very much more varied and the ultimate outlook much more serious than was at one time supposed. We now know that in some cases the onset is insidious and the course, at least for a time, slowly progressive, while in addition it is generally recognized that, months or even years after the patient appears to have made a more or less complete recovery, remissions may occur. Indeed we now realize how impossible it often is to express ourselves with any degree of certainty as to when the patient is to be regarded as out of danger, in the sense that he is no longer liable to relapse, and as to whether, or to what extent, the existing manifestations of the disease will be recovered from. For the same reason it is difficult to draw a sharp line of distinction between the actual symptoms of the disease, in other words the phenomena directly attributable to a still active process, as contrasted with the sequelæ—if we restrict this term to the after-effects which have been determined by damage to the nervous system, although the activity of the causative process has definitely terminated.

This evening our attention is focused upon the mental sequelæ, in other words, the later mental changes which result, persist, and may even prove permanent. I take it indeed that the discussion will centre round the character of these mental manifestations, their frequency, their variations in different outbreaks, their mode of production, the manner and extent to which they interfere with the activities of the individual, and the problem of their eventual disappearance or persistence. Again, cases of epidemic encephalitis are met with in which the mental sequelæ predominate and may indeed monopolize the clinical picture, consequently the question arises, and will no doubt be discussed by subsequent speakers, as to whether the mental symptoms are in themselves sufficiently characteristic and distinctive so as to permit of a diagnosis apart from the help that may be afforded by the history and the presence of physical manifestations. Personally, I believe that a diagnosis based upon the mental symptoms alone is rarely if ever possible, and since this is a joint meeting it may not be out of place, while the interest of the discussion will I think be thereby enhanced, if brief allusion is made to certain points in the history, also to certain physical phenomena the occurrence of which may permit of a diagnosis in cases in which the mental symptoms are insufficient for this purpose.

I propose in the limited time at my disposal to direct your attention in the first place to the mental sequelæ, referred to by the patient or by his friends, which have especially impressed me.

The mental sequelæ most often complained of by adult patients, if they complain of anything at all, may be—to judge from my personal experience—summed up in the words slow cerebration, mental fatigue, and defective application. Thus, among the expressions used by these patients to indicate their disability were the following: "mental dullness," "lack of mental alertness," "stupidity," "mental languor," "a feeling of mental lassitude," "no mental energy," "the brain is easily tired," "difficulty in concentrating," "anything in the way of concentration takes it out of me," "after going (concentrating) for a little time I come to a full stop like an automatic machine which has run down and requires re-winding." Some defect of memory, more especially for recent events, is at times complained of; but in my experience this symptom seldom constitutes a serious disability, although I have met with two or three cases in which it was a striking feature. In this connexion it is worthy of note that a patient who has had a more or less acute attack of epidemic encephalitis can seldom recall any of the events connected with the earlier stages of his illness. I may remind you in passing that when the expressionless face so characteristic of the Parkinsonian syndrome is present, the patient's appearance may convey a very false impression of his mental capacity.

Again, the relatives will often tell you that the patient is very silent and that he shows a lack of interest. Thus, one patient who had previously been a great talker "will sit for hours without making a remark"; another man "will sit doing nothing and saying nothing unless he is spoken to, when his replies are confined to monosyllables": while of other patients it was said: "he no longer asks for his letters," "he never asks for the paper," "he takes no interest in his business." Yet there is seldom any suggestion of mental deterioration to which the term dementia is strictly applicable. Irritability is sometimes referred to, and, exceptionally, these patients are unduly emotional. Pronounced mental depression is, I should say from my experience, uncommon. I have never met with a case of suicide, though one patient volunteered the statement that she felt she might do something desperate; another man, a neuropath prior to his illness, stated that he had on one occasion wandered out of the house with the idea that he might end his life; while a third who suffered from severe pains—he was also a man of nervous temperament—told me that if the pains continued he feared he might injure himself. A case which impressed me is, I think, especially instructive, since it illustrates a type of mental alteration which is, I should say, often met with in varying degree as a sequel to epidemic encephalitis. This patient, who could tell a good story and make an amusing after-dinner speech, lost these gifts after an attack of acute encephalitis. When seen a year after his attack, he told me that although he was able to do his routine office work satisfactorily he was quite incapable of applying his mind to, and coping with, a new business proposition.

Finally, I would say that the most common and obvious mental sequelæ in the adult, met with in very varying degree in individual cases, are slow cerebration with defective concentration and brain fatigue, impairment of spontaneous expression and action, and lack of initiative. A true psychosis calling for certification is, to judge from my experience, a very exceptional sequel to epidemic encephalitis. Occupational delirium, hallucinations, disorientation and more or less mental confusion are, as we all know, often observed about the time of the onset in an acute case, and I have met with instances in which the patient was violently maniacal and had to be forcibly restrained. These symptoms, however, are to be associated rather with the acute stage of the disease, and since they are no doubt determined by the activity of the infective process, they cannot perhaps be strictly regarded as sequelæ. While the subject of lethargic encephalitis does not as a rule worry about his responsibilities,

I have been impressed by the number of cases recently met with in which a patient, who, having so far recovered that he is able to return to work, finds himself unable to cope with his duties and develops in consequence symptoms of anxiety, defective concentration, indecision, sleeplessness, and so on. Indeed one has to bear in mind that the patient's incapacity may not only be determined by physical and mental residua—a direct result of the disease—but that in addition his disability may be in part attributable to the secondary psychical sequelæ to which I have just referred.

The great majority of the cases I have met with in children have conformed more or less closely in their symptomatology to the adult type, although there may be striking differences. Thus the relatives in individual cases made use of the following terms: the child is "dull," "stupid," "stolid," "sullen," "silent," "sleepy," "he was well up to the average before his illness but now he is unable to learn anything," "he cannot concentrate," "his memory is poor." It would seem that these children lose the capacity to learn. Although they may retain the knowledge acquired before their illness, their power of assimilation is subsequently *nil*. The eventual outcome in these cases is a question regarding which we require information. Again, the relatives have said of some of these children, "he is no longer interested in games though formerly he was very keen on football," "he never wishes to play with other children," "even a new toy does not interest him," "he is very careless about his clothes," "he will not wash himself, although he used to be very particular for a boy." I have met, too, with quite a number of cases in which the parents have remarked on the altered disposition of the child and have complained that he is irritable, quarrelsome, mischievous, that he teases the other children and so on. We are all familiar with the histories of children who, following upon an attack of epidemic encephalitis, have become filthy in their habits, destructive and cruel, who steal and lie and use obscene language, but it is interesting to note that in only two of twenty-nine cases under sixteen years of age which I have observed was this moral deterioration a conspicuous feature. These moral defects do not appear to be met with in the adult. Another symptom very often observed in children, though less common I think as a late manifestation in adult cases, is the disturbance of the sleep rhythm. The restlessness displayed by some of these children during the night is indeed astonishing. They will toss about and throw their bed-clothes in all directions. One child, the inmate of an institution, took a fiendish delight in keeping all the other children in the dormitory awake, a small boy used to whistle continuously well into the early hours of the morning, another child would lie awake listening for and imitating the hoots of the owls, much to the annoyance of his elder brother, while a fourth would count aloud for hours on end. The explanation of the differences presented by the mental sequelæ in the child, as contrasted with those observed in adult cases, affords in itself an interesting topic for discussion. Further, I hope that those present who have had special opportunities of observing and following up these cases will be able to give us some information as to the ultimate prognosis in children, both as regards the moral defects and the child's eventual capacity to acquire knowledge.

The history may afford conclusive evidence as to the diagnosis in many cases essentially characterized by mental sequelæ. I shall merely mention some points of especial importance in this connexion. A history to the effect that the patient has never been well since an attack of "influenza" is suggestive, and particularly so when it is ascertained that the patient did not suffer from catarrhal symptoms, that he was delirious, and that no other member of the family was similarly affected. Perhaps the most valuable corroborative evidence is afforded by a statement to the effect that the patient on one occasion saw double for a few days. Of almost equal importance, though not by any means so often met with, is a history of onset with an agonizing pain, localized, it may be, in the head, trunk, or one of the limbs, for which no satisfactory explanation could be found. Drowsiness and insomnia, noisy

breathing, a history of so-called chorea, or of jerking of the muscles, are among other points which may give the clue to the diagnosis in doubtful cases.

Similarly there are certain motor sequelæ which are sometimes invaluable in enabling us to arrive at a diagnosis. Among these the Parkinsonian syndrome is, because of its frequency, of great importance. Again and again some slight want of mobility of the face, an unnatural pose of the arm or hand, or an apparent stiffness about the trunk when the patient turns, has suggested the correct line of inquiry when one might otherwise have failed to recognize the condition. You will recall that Hughlings Jackson used to say of the early diagnosis of Parkinson's disease that the clinician is in a very similar position to the big game hunter who, walking through the jungle, comes face to face with a tiger, for unless the hunter recognizes the tiger at a glance he will probably not have another opportunity. Then, again, I may remind you that in cases of epidemic encephalitis we meet with a great variety of anomalous movements, the presence of which may indicate the nature of the condition.

There are many matters I should have liked to discuss in detail but I have attempted rather to touch on a number of points which will no doubt be elaborated by the speakers who are to follow.

Professor ARTHUR HALL (Sheffield).

The mental changes produced by epidemic encephalitis in children are often of a kind hitherto but rarely seen at this age. Higher centres are affected, changes in which have hitherto been confined largely to adult or advanced life, for instance, persistent insomnia, paralysis agitans, and various forms of hooliganism. In the acute phases of encephalitis the mental effects in childhood differ but little from those seen in adults. They may be positive or negative. In these different actions on different persons they resemble the diverse effects produced by too much alcohol. One man becomes excited, loquacious and truculent, another silent, morose, and drowsy.

Amongst the mental effects in the chronic phases, four groups may be specially mentioned for discussion.

(1) *The Idiot Group*—which occurs up to about five years of age. In this there is complete functional destruction of the higher nervous centres, and permanent idiocy.

(2) *The Parkinsonian Group*.—More frequent in children above than below ten. The mental changes are sometimes difficult to estimate. They may be slight, but increase with the extent and severity of the condition. This group is not infrequently combined with the following:—

(3) *The Apache Group*.—Usually in children rather older. Serious changes in moral character arise quite suddenly as the result of infection. Sometimes they are of such a kind as to bring the patient into serious conflict with the police, and even to be a danger to the community. This group is small, but important. Its very nature tends to make its size seem larger.

(4) *The "Super-difficult" Child*.—This type may occur at any age; it is by far the most common sequela in childhood. There is no exact line to be drawn between this and the preceding group. Each may be associated with Parkinsonism.

The problem of how to deal with the third group and the more severe members of the fourth group is a difficult one. The third group, from the point of view of public safety, probably requires institutional treatment, although this is likely to be of little value to the individual affected.

For the fourth group home treatment if suitable seems the best. This is already bringing about improvement in individual cases. Where the environment is not suitable, some form of institutional treatment is called for in order to prevent the child drifting into the third group. Such an institution, however, must be a real improvement upon the existing home conditions, and should be in the nature of an

open-air residential school, with very highly trained teachers, and sufficient of them to allow of personal and individual attention to each child. It will be somewhat costly, but the number of cases in which it is required should not be very large, and the saving of a human mind is worth the expenditure of money and trouble.

Dr. F. C. SHRUBSALL.

During the last three years the members of the London School Medical Service have endeavoured to keep in touch with all children attending schools in the County of London who have suffered from this disease, and to obtain reports on their condition through the School Care Committee, as well as to arrange for their medical examination, both in the course of ordinary routine and as special cases when need has arisen. In the earlier years only those cases could be followed up in which the children had come under notice on account of some symptoms exhibited on their return to school, so that the numbers only represent a fraction of the total number. But in 1924 it was decided to endeavour to obtain reports on all cases of children who had been notified to the local authority as sufferers, in addition to any others who might come to be notified on account of the sequelæ of the disease, although the nature of the original attack was not recognized. In consequence, the figures for last year must represent a much higher proportion of the total. As, however, some time elapses before reports can be obtained, or before any satisfactory evidence of the progress of the patient is available, the data which can now be presented for 1924 cover only those who were affected in the earlier part of the year. Those instances in which the initial attack proved fatal are excluded. The extent to which the ground was covered is shown by the relation between the total notifications and the reports for 1924 (notifications, children age 0 to 15 = 212; reports on notified cases 111; on unnotified cases 8).

The following table shows the condition in the later part of 1924 of 193 children who had suffered from encephalitis:—

Year of onset of disease	1924	1923	1922	1921	1920	1919	1918	1917	1916	Total
Number reported upon ...	119	11	8	12	22	6	12	2	1	193
Of these, No. notified ...	111	8	5	5	6	1	—	—	—	136
No. not notified ...	8	3	3	7	16	5	12	2	1	57
Apparently completely recovered ...	26	5	3	1	6	1	—	—	—	42
Partial recovery, only some deteriorating ...	93	6	5	11	16	5	12	2	1	151
Predominant symptoms in 1924.										
Sleep impaired ...	40	1	1	2	3	—	1	—	—	48
Intelligence impaired ...	26	2	4	4	5	1	2	2	1	47
Paresis ...	6	2	—	1	12	—	3	—	—	24
Twitchings, tremors, or "fits" ...	12	1	1	1	3	1	3	—	—	22
Ocular symptoms ...	10	—	—	1	1	—	—	—	—	12
Conduct changes ...	44	2	5	10	13	6	7	1	1	89

Five cases reported previous to 1921 had developed a Parkinsonian syndrome, while two cases previously noted had died and so are excluded from the return.

The proportion of the cases which seem to have recovered completely is low, but it is probable that the milder ones do not come to be noticed, and in the earlier years the only cases recorded were those with sequelæ. Soon after the acute attack the sleep disturbances are more obvious, while impairment of intelligence or disturbance of conduct may not attract attention at first.

The most characteristic feature is a heightened irritability, a rapid onset of fatigue, lack of continuity of attention, such as is noted in all who are recovering from such an illness as influenza, but which in the case of encephalitis persists for a prolonged period and renders the study of the mental processes of the patient difficult.

From the intellectual side, the common story from school is that the subject's work had fallen off badly, that his attention wandered, and often that he was sleepy, if he did not actually fall asleep during class. In the cases of children who slept it was often found that the child had been restless and sleepless all night, so that sleep in the day time was a natural reaction. Insomnia and nocturnal restlessness

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are common early sequelæ of encephalitis. In most cases the attention to school work and the value of the response have steadily improved, commencing with the more interesting lessons; in some, however, there has been for the rest of the period of observation a diminished capacity for education. Intellectual hebetude, emotional instability, heightened irritability with reduced inhibition and increased fatigability, are features of those who have exhibited definite mental sequelæ. When the initial obstacle of wandering attention is surmounted it is usual to find that the response to intelligence tests, whether of the Binet type or of a performance type, is not so much altered as would at first sight appear. In some instances observations soon after return to school showed a marked lowering of the mental age score, but rapid improvement in examinations at intervals of a few months. In the younger children the rate of mental growth seems to have been seriously retarded, and in some mental deficiency has followed; this has been most evident in the case of those attacked by encephalitis during the pre-school age. In older children it has seemed in some instances as if mental growth were continuing but accompanied by the gradual evolution of a dementia, gain and loss going on together, until finally the loss exceeded the gain, and the resultant mental state began to deteriorate obviously. In such cases the responses to mental tests show an increasing degree of scattering. However, for the present it seems difficult to make positive assertions owing to the great difficulty in feeling sure that the co-operation of the subject was secured, without which mental tests are of no value as measures of intelligence. In the early stages this lack of co-operative effort may be the entire cause of the inferior response. In many cases, especially after an interval, it is quite striking that great disturbance of conduct may occur without any diminution of the power of response to mental tests, or to the working ability, though in some the working ability is no longer exhibited as continuously as was said to be the case before the illness. The employers of some who had had encephalitis in later childhood or adolescence, and who since have committed repeated offences against the law, either in the form of pilfering or assaults, have been willing to take them back into their employ because they were otherwise good, skilled, and even conscientious workmen.

The changes in conduct noted as sequelæ to an attack of encephalitis are of a very varied character, but seem to take their origin in the state of irritability, lack of inhibition, and consequent impulsiveness which immediately follows the acute phase of the illness and becomes fixed, either by habit formation or by secondary development of psycho-neuroses, dependent on the particular features of the individual environment. At the younger ages general unmanageableness (particularly at the hand of the relations) and persistent petty pilfering are the two chief causes of complaint; at a later age, stealing has brought several youths into contact with the police.

TABLE II.—NATURE OF CHANGES IN CONDUCT FOLLOWING ENCEPHALITIS.

Predominant symptom	Onset of Illness	
	In 1924	Prior to 1924
Irritability and impatience	17	10
Excitability, noisiness, restlessness	12	6
Lack of control, destructiveness, spitefulness, violence	8	18
Hysterical manifestations	2	1
Suicidal	1	—
Stealing	2	15
Obsessions, or compulsions	1	—
Dirty habits	—	1
Indecent assaults on children	—	2

In the table an attempt has been made to group the main features of changed conduct, but if details were set out in full, each case would have a category to itself. Judging from the results so far to hand, irritability, impatience, excitability, and spitefulness may pass off, and control may be regained provided favourable conditions are attainable, but often the home environment is such as adds to the difficulty.

When the child's condition is such as to lead to his being kept in the foreground or alternately petted and baited, many secondary manifestations follow. One mother at least confessed to the family being amused at some of the antics of a sufferer, and admitted he was occasionally teased intentionally. There is no doubt that such more or less intentional baiting does occur from time to time at school, despite the best efforts of the teacher, so that it is little surprise that a boy with a slight paresis of a limb develops a marked paralysis whenever the question of return to school is raised. Though he can sometimes be decoyed into using the paralysed limb, this discovery has no effect on his assertion that it is entirely useless. In some cases, too, the conduct observed, although said to be foreign to the previous record of the sufferer, is such as might have been expected to occur from the admitted traits of other members of the family, the illness having perhaps precipitated the onset of a feature that would in any case have arisen in due course. Thus the mother of one girl who was out of control, although previously she had been placid, explained excitedly that "her (the girl's) father would as soon knife you as look at you." With temperamental instability on both sides what degree of control was to be expected in such a home? Also in some of the cases of persistent theft, there had been examples in which other members of the family have shown similar conduct without having so ready an excuse. No cases of petty pilfering in the home have been included under the category "stealing" in the table, which relates only to definite offences of which cognizance had to be taken. In many of these the habit or compulsion seems to be deeply ingrained, so that the trait persists even while the subject is undergoing detention as a result of a former charge. It cannot be said that the subjects are unaware of the nature or quality of their acts, or are ignorant of their wrongful nature, for they can render lip service to ethical ideals; but the fact that they will commit the offences under circumstances which ensure their almost immediate detection, and indeed seem at times to go out of their way to provide clues, would suggest as at least an arguable case that at the moment they may be under the influence of an irresistible impulse. Bicycles are a favoured subject for their attention, and it might be thought this was associated with wandering habits, were it not that the boys usually sell or endeavour to sell them later. In one case at least a boy went off with two bicycles at once, and then gave his father's correct name, address and occupation when trying to sell them. The very few sex offences, too, were committed under circumstances which ensured early detection, at any rate in the case of one youth. So far there have been very few. Although the habit of stealing has not been checked in some of these children by education in industrial schools, the orderly life therein has reduced their general irritability, so that in time they became more companionable and even popular. In other cases, where general lack of control was a feature, the life of the school seemed to be doing the individual boys and girls good, although slowly and with many relapses. But the effect of the presence of the sufferers was proved to be disastrous to the well-being of the school, and the slight allowances which had to be made in their case impaired the general discipline to such an extent that it was rendered evident that, however much contact with normals was desirable for the sufferer, it was of greater detriment to the community.

ILLUSTRATIVE CASES.

(1) *Persistent Bicycle Stealing.*

(a) H. C., previously normal, attacked at beginning of 1922 by "influenza and chorea," absent school some months, then reported by teacher dull, heavy and backward for age. Examined May, 1923, after stealing a bicycle, mental age response = 9. June, 1923: jerky movements more obvious; mental age = 10; not sent to industrial school. Charged again October, 1923, stealing bicycle, mental age 10+, sent to industrial school. Report January, 1925, had been troublesome at first, but soon settled and became popular; put in band and did well, but stole bicycles twice under circumstances which ensured detection. Now in army

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and doing well, playing saxophone in band. Seemed settled except for bicycles. Said he knew it was wrong, but he could not resist taking any stray bicycle he saw. Got to Standard VI; good on whole for age.

(b) H. T., previously normal, attacked by encephalitis lethargica in 1918, attended ordinary school and did fairly, but took bicycle. After leaving school charged with stealing bicycle; intelligent adult, described as good worker.

(2) *Persistent Thieving.*

(a) G. E., attacked with encephalitis lethargica at age 8, after which difficult to manage, pilfering habits and mental apathy. Had psycho-analytic treatment. Seen 1922, aged 11, mental age 10, but slow answers; charged with stealing. Sent to industrial school. Report: gives intelligent answers but unsteady in her work, cruel, spiteful; choreiform movements, inveterate thief. Leaned over wall of school and stole purse from lady with perambulator while pretending to admire the baby. Does not improve, and impossible to think of her in a situation.

(b) W. J., attacked with encephalitis lethargica 1918, out of control at once, and took to pilfering; sent to industrial school. At first had violent outbursts and threatened suicide, &c., but told he was not immune from punishment; improved and did well. Sent to army band, but stole bicycle and dismissed. At home improved again at first, but then repeated charges of theft of bicycles. Now in a Borstal institution.

W. A., encephalitis lethargica 1920, at age of 10. First seen in February, 1922; reported to have somewhat defective hearing, tremors in hands, increased knee-jerks, hesitant gait and speech. March, 1922: Reported to fall asleep in school repeatedly, and to scream at night. Sent to camp school in 1923, mental age 9-10. Generally out of control. Sent to industrial school; continued out of control. Transferred to another school. Said to spring on other boys and to bite them, also to have slow incontinence and lack of bowel control. January, 1924: Mental age about 10, uniformly retarded. Capable of a certain amount of self-control, but generally acts on impulse. Has regarded himself as immune from punishment. The boy was transferred to a Poor Law institution and is now in Darenth.

Dr. A. SALUSBURY MACNALT.

Encephalitis lethargica is a disease which is claiming an increasing toll of victims. The figures of the annual notifications testify to this and it will be observed that those for 1924 largely exceed the figures for each preceding year of notification. The deaths are omitted from the 1924 return because the data are not yet available. From 1919-1923 inclusive the case-mortality has averaged about 50 per cent. on the basis of notified cases, but during the recent outbreak there have been indications that the severity of the disease has declined. We are concerned here to-night with the mental fate of the surviving 50 per cent.

In 1918, in the course of the initial investigations carried out by the Ministry of Health (then the Local Government Board) and the Medical Research Council into encephalitis lethargica, certain manifestations such as an alteration in the mental condition, residual cranial nerve palsies, athetoid movements and paresis were observed in a limited number of patients who in other respects had recovered from their attacks of illness, and a guarded opinion was expressed as to the possible risks of after-effects in the disease. Unfortunately this warning has been fully justified and accentuated in the progress of time by further observations. It is now sufficiently established that "the new disease" brings in its train sequelæ both of neurological and medico-legal importance.

The manifestations of the after-effects of encephalitis lethargica may be grouped (a) into those appearing in the course of the original acute malady and persisting after partial or complete disappearance of all other symptoms, and (b) those which ensue after the original acute malady has apparently terminated or in some cases has passed unrecognized. Such effects appear after a latent period of extreme variability ranging from some weeks to over two years. In a certain number of reported cases the after-effects cleared up temporarily, but reappeared some months later. No definite opinion, therefore, can be expressed, until after the lapse of some years, as to whether

an attack of acute encephalitis, however mild in appearance, may or may not result in serious sequelæ.

It is as yet unknown whether the late manifestations are true after-effects or whether they are indicative of persistence of the original infection, an interval of latency having occurred.

It is impossible also at present to say what proportion of surviving cases of encephalitis lethargica exhibit after-effects, whether mental or physical. Investigations are now being made by the Ministry of Health on this question. It appears to be probable that mental after-effects, especially in children, occur in a large proportion of the surviving cases.

My colleague, Dr. Allan Parsons, in 1922 studied the after-effects recorded in the case of 271 patients who were seen at periods varying from two to eighteen months after the onset of their respective illnesses. In 136 out of 250 of these patients, reports of whom were received after an interval of not less than three months from the onset of symptoms, there appeared mental symptoms in some cases associated with crippling defects. The mental changes in these patients were subdivided as follows :—

Mental impairment	57 cases
Persistent drowsiness	15 "
Neurasthenia	7 "
Melancholia	4 "
Loss of memory	6 "
Psychic changes	47 "
Total						136 "

In 121 of the 250 patients there were crippling defects such as ocular palsies or paresis of the face and limbs.

Dr. Parsons and I have noted that in all but mild cases there is much physical exhaustion and muscular weakness, more marked sometimes than the length of illness would suggest. With this physical exhaustion is often combined some degree of mental impairment ranging from a slight blunting of the intelligence to amentia.

In a number of instances reported to us it was necessary for the patient to be certified and sent to a mental asylum owing to the development of maniacal or homicidal symptoms.

In the intensive outbreak of encephalitis lethargica which occurred in a girls' home at Derby in 1919 in which twelve persons (females) were affected out of twenty-two persons (females) resident in the institution, five cases proved fatal. Of the seven survivors two subsequently developed mental symptoms.

The great frequency of the Parkinsonian syndrome is a marked feature in the Continental literature; it is difficult to say whether the preponderance also obtains in this country owing to the fact that the acute illness passes unrecognized and unnotified in many cases. Once declared, however, the late manifestations cannot be ignored; mental symptoms complicate the clinical picture at all ages. Fewer cases of disease are more pathetic than the juvenile cases of paralysis agitans, now familiar to the neurologist since 1919, and alike in children and adults the Parkinsonian syndrome must be regarded as the gravest of sequelæ.

The treatment and provision of institutions for patients presenting mental after-effects are problems of considerable difficulty. Cases of mental disorder following on encephalitis lethargica in the adult appear on the whole to be more recoverable than similar conditions in children. At the same time it is my personal opinion that many juvenile cases would do well if recognized early and if suitably and sympathetically treated. Between these cases and the severe mental disorders with a hopeless prognosis are a number of cases of mental derangement in which special treatment would be well worth trying with a view to recovery. We also need facilities for medical observation and study of these conditions. I may add that the question of

making provision for the special care and treatment of the after-effects of encephalitis lethargica, more especially the mental after-effects, is under the consideration of the Ministry of Health and other Government Departments at the present time.

Dr. PHILIP CLOAKE.¹

The mental symptoms and sequelæ of encephalitis lethargica are so varied striking, and bizarre that most writers have been content to describe them without making any effort to understand them in relation to one another and to the disease which evoked them. Kirby and Davies² alone occur to me as writers who have dealt convincingly with the nosology of the symptoms of the acute disease, and I am indebted to their paper for much of what I have to say upon that subject.

I shall try to offer some suggestions, which, owing to limitation of time, must appear more like assertions than I would wish, having as their object the establishment of a standpoint from which these manifestations may appear a little less chaotic.

Many acute febrile illnesses, and even afebrile toxic states, cause delirium, which is more likely to result if the toxæmia is severe, though it is probable that a constitutional factor of psychic or nervous instability is a predisposing factor. But where the brain is itself the seat of infection by a virus which appears to have a special affinity for nervous tissues, it is not surprising that the degree of disturbance of cerebral function is relatively greater. In encephalitis lethargica one commonly has such a disturbance in a marked degree, often with little or no fever, and with only slight general somatic symptoms.

The mental symptoms which arise in toxic infectious states are of a fairly uniform character. Hoch's classification of the different types of mental reaction in disease includes the organic mental reaction. This has been arrived at by a consideration and analysis of the mental symptoms found in all diseases in which the brain suffers temporary or permanent injury. The mental symptoms seen in toxic infectious conditions are included as an acute sub-group of the organic mental reaction.

These symptoms on analysis have been found to be: (1) Impairment of apprehension—of environment, &c.; (2) impairment of the power of retaining mental impressions; (3) defects of memory and difficulty in the activization of memory; (4) variability in mental capacity and in the level of attention.

These disturbances result in the well-known clouding of consciousness, often amounting to delirium, with hallucinations and illusions, disorientation, &c. According to the predominance of one or other of these symptoms several clinical forms of acute organic mental reaction are recognized, in one of which the personality of the patient and psychogenic reactions play an important part in producing diverse mental pictures.

It is important to realize that the mental symptoms of acute lethargic encephalitis belong essentially to this toxic infectious group and are of the nature which I have just briefly stated. But, in addition, there is one most important complicating factor, one which gives the disease its distinctive character and is closely bound up with the pathogenesis of all its mental manifestations—that is lethargy. Though the other acute mental symptoms appear to be due to a toxic affection of the brain, and hence are more marked at night when toxic effects are commonly most apparent, it is difficult to escape the view that lethargy arises from quite another cause. The lethargy appears to be due to the localization of the disease process predominantly in and about the mid-brain. Other lesions of the mid-brain, such as neoplasms, may be similarly associated with lethargy. It has been suggested by Dr. Farquhar Buzzard

¹ The writer wishes to express his gratitude to the Medical Research Council for their grant which enabled him to carry out work on this subject.

² "Psychiatric Aspects of Epidemic Encephalitis," G. H. Kirby and T. K. Davies, *Arch. of Neur. and Psych.*, May, 1921.

that the real determining factor is internal hydrocephalus produced by blocking of the Sylvian aqueduct by inflammatory swelling or scarring, and this view receives support from the fact that other causes of internal hydrocephalus, such as cerebellar neoplasms, may result in lethargy.

The state of sleep introduces psychological features and problems of its own, and the more we understand about sleep the more we shall understand of the psychic manifestations of encephalitis lethargica.

Lethargy, then, greatly modifies the mental picture resulting from the toxic-inflammatory state. Sometimes lethargy preponderates, the toxic symptoms are slight and we have the state of prolonged sleep so common in the early days of the acute illness; at other times the toxic symptoms are more severe and we have an active delirious state at night which may either quieten down in the early morning, then unmasking the drowsy state, or may continue in a milder form during the day as a quiet occupational delirium.

The view that the lethargy is not a result of the toxæmia is supported by the fact that it is quite easy, as a rule, to awaken the patient, when he is frequently as clear as if awakened from normal sleep—a very unlikely possibility if the drowsiness were really a toxic manifestation. (Coma, however, when it occurs in severe cases is probably toxic in origin.)

When mild delirium exists by day it frequently appears more profound through the simultaneous presence of drowsiness, and I have found it possible to arouse immediately to a much higher level of consciousness a patient who was talking deliriously about his work and was making movements in the air (which were efforts to hammer hallucinatory nails into an imaginary board), and in this state he answered personal questions sensibly and accurately.

In the co-existence of lethargy, a phenomenon perhaps dependent upon inflammatory changes in the mid-brain, and the toxic infectious mental reaction, is to be found the explanation of all the psychic symptoms of acute encephalitis lethargica; and I have thought it wise to introduce these considerations here in order to make clear my view of the pathogenesis of the sequelæ.

On this hypothesis it is possible to understand the curious inversion of sleep rhythm so often seen in children in and after the acute stage. I suggest that the real explanation is that a mild, sub-acute, toxic state stimulates the cortical functions, the condition being similar to, but more marked than, the effects of mild alcoholic inebriation. As one would expect of symptoms arising from toxæmia, they are most marked at night.

The effect of the stimulation (a word probably to be understood in the same sense as the common word "stimulant" for alcohol—that is, as a depressor of higher inhibition) is to produce not merely insomnia but hyper-activity of a remarkable kind in which the child would appear to seek any and every means of expending energy for the sake of expending it. That highest psychic functions are depressed is seen in the weakness of attention and self-control and the freer play of partially uncontrolled instinctive activity, such as over-affectionateness and irritability.

A very demon of malignant mischief appears to possess the children at times, and just as mischief in a high-spirited energetic child is a natural activity so these children are prompted by their excessive psychomotor activity to every conceivable prank. The worst example I have met was that of a small boy who during the night collected the boots of the whole household, took them down to the pantry and filled them with jam. Their behaviour is on the whole very uniform, however, and takes the form of whistling, talking, constantly getting in and out of bed, an unnatural eagerness to wash the face and brush the hair, and so on. They do not cease such activities until 5 or 6 a.m., and then they cease, not, I think, because they are exhausted, but because the toxic state has subsided and allows the sleep of fatigue to overwhelm them until mid-day or so.

I think the so-called hypomanic insomnia of children and the prolonged insomnia of adults after encephalitis are toxic in origin and akin to delirium. I think there is often evidence of mild mental confusion, and frank delirium is occasionally seen as an interlude in such subjects. On the other hand, this sequel is often very prolonged and it is probable that a habit can be formed and can persist after the toxic state which gave rise to it has disappeared. This happens in some cases and, as a result, one hears of great improvement in the symptom effected by removal of the patient from home, the environmental factors which have tended to maintain the habit being thus abolished. Sometimes these patients relapse when they return home.

A prolonged hot bath at bedtime is said to have a beneficial effect upon children with hyper-active insomnia, and this may act in part by lessening the toxæmia as well as by draining the blood into peripheral vessels.

When inflammation of the brain has disappeared one would expect mental symptoms peculiar to the toxic infectious reaction to subside. I think the delirious and confusional state is present only while the disease is active, and its persistence after the acute illness is evidence of chronic cerebral inflammation. But although they disappear in this way, one would not expect the brain structures to recover suddenly and completely from the damage they have sustained. If, then, mental sequelæ occur, one would expect them to be predominantly of the chronic organic mental reaction type, the kind of psychic changes encountered after slight or moderate cerebral damage from other conditions, such as head injuries, cerebral concussion, apoplexy and so on.

One is not surprised to find the mental sequelæ showing a close resemblance to those of head injury. Strecker and Ebaugh¹ have recently recorded the persistent psychic effects of head injury in children, and their description will serve equally for those of encephalitis lethargica. They record persistence of mental defect for an indefinite period; behaviour difficulties such as incorrigibility, thieving, irritability, and outbursts of temper and violence; a total change of disposition and character; hyperkinesis; impaired power of concentration; impaired memory, and easy fatiguability. I am sure that, with encephalitis in mind, one cannot but be struck by the closeness of this resemblance.

Let us consider the mental symptoms of cerebral damage a little more closely. These on analysis include:—

(a) Narrowing and weakening of mental capacity or grasp, the extent of the field of thought is narrowed, remoter associations are lost, there is diminished power of reasoning, especially where intricate associations are necessary, hence impairment of memory for remoter events. (b) Weakening of power of attention, hence impaired memory for recent events. (c) Slowing up of the stream of thought. (d) Easy mental fatiguability. (e) Impaired inhibition of psychologically lower forms of mental activity, especially of instinctive processes.

The manifestations which result from this are dependent upon the nature of dominant instincts, formerly perhaps controlled with difficulty. If instinctive activity is also weakened, no evidence of loss of control may be present, and apathy results. Such symptoms are frequently encountered after encephalitis lethargica.

Weakness of power of attention is often complained of by the patient, who says he cannot give his mind to anything; slowness of thought and easy fatiguability are prominent. Impaired inhibition of instinctive tendencies is well seen in the common irritability and violent outbursts of temper and pugnacity, in thieving, which is due to loss of inhibition of the acquisitive instinct, and consequent lying to escape punishment, over-affectionateness and occasional sex transgressions. Impairment of instinctive activity is marked in many patients, and is complained of when they say they take no interest in anything, since the feeling of interest arises from the activity of instinctive tendencies.

¹ "Neuro-psychiatric Sequelæ of Cerebral Trauma in Children," Strecker and Ebaugh, *Arch. of Neur and Psych.*, October, 1924, p. 443.

Whether the impairment of mental capacity—roughly, what we call intelligence—exists, is a much debated question.

Most observers report a minor degree of deficiency, as measurable by intelligence tests, in many cases, and this may be greater where the subject is a young child. My own observations agree with this view, but Sherman and Beverley's criticism¹ must be remembered. They pointed out in all their cases of children with behaviour difficulties after encephalitis, that the power of application to the task was often impaired temporarily by brief crises of mental distraction, and if these were ignored, one would be liable to consider that the child had failed in a task which, when undistracted, it could pass quite easily.

The organic mental reaction, then, is an important conception in the understanding of many post-encephalitic mental disturbances. But this cannot be the whole explanation, for in many cases where the character changes are marked, the features of the organic mental reaction are not apparent. Some patients, whose intelligence is above the average, who exhibit no slowing of thought, fatiguability, or weakness of attention, show a marked depression of spontaneous activity, lack of initiative, and feel no interest in their surroundings, but require outside stimulation to perform anything. Thus, one child of fourteen years, with intelligence of the sixteen years' standard, regularly gets ten or fifteen marks out of 100 in his school examinations and is reported to be inattentive and lazy. He plays no games with other children, but sits and plays at home with the toys he had at the time of his illness five years ago, and is indolent, indifferent and feckless. Here, I think, we must recall the second factor in the acute mental changes, namely, lethargy.

Drowsiness persisting after encephalitis is common enough, but lethargy includes more than actual hypersomnia; indeed, I am inclined to the view that it can exist without this symptom, and the mental picture of many patients has an undoubted tincture of chronic lethargy when their hours of sleep are normal or too few. Sometimes they say they feel sleepy but cannot go to sleep, at other times even this is absent. Lethargy has a marked contributory effect in the loss of interest and initiative already referred to, and its effects may be present without impairment of intellect, or it may complicate the symptoms already described as attributable to organic cerebral disturbance.

The commonest, and most characteristic, of the mental sequelæ of encephalitis lethargica is a condition in which lethargy is the important factor in pathogenesis. It is often associated with the Parkinsonian syndrome.

This sequel has been carefully investigated by Naville² who gave it the name of "bradyphrenia." He writes:—

"It is especially characterized by a diminution of voluntary attention, spontaneous interest, initiative, capacity for effort and for work with objective fatiguability and slight subjective impairment of memory. They want solitude, become uncommunicative, and do nothing unless constantly exhorted; they have lost psychic tone." "The condition is quite pathognomonic of encephalitis and is not found in any other condition except perhaps in certain cases of classic Parkinson's disease."

It is significant that Naville finds a

"diminution in the rapidity both of movements and of intellectual operations,"

and that

"this slowness sometimes completely disappears if an external stimulus to effort occurs or even an internal stimulus capable of initiating movement, showing there is no organic obstacle to the regular and immediate execution of acts, and that the habitual slowness

¹ "Deterioration in Children showing Behaviour Difficulties after Epidemic Encephalitis," Sherman and Beverley, *Arch. Neurol. and Psych.*, Sept., 1923, p. 329.

² "Étude sur les complications et les séquelles mentales de l'encéphalite épidémique. La bradyphrénie." Naville, *L'Encéphale*, 1922, p. 369.

resides in the quality of the initiation of the movements." "In its apathy, indifference, inertia combined with good judgment and intelligence and the waking to an external stimulus, the state resembles that of half-sleep."

It would be interesting to consider whether mental processes can be disturbed as a result of the disorder of automaticity seen in the Parkinsonian syndrome, but I am sure that the lethargic mental state can exist in the absence of the syndrome. In some cases it is associated with adiposity and depressed sexual function (amenorrhœa, loss of libido) and these cases forcibly remind one of the Fröhlich syndrome of hypopituitarism in which lethargy is often prominent—witness the instance of the fat boy, Joe, in "Pickwick."

It is not impossible that normal pituitary secretion may be necessary for the optimum activity of cortical cells, and that the lethargy here recorded may be sometimes due to inadequate functioning of the pars anterior of the pituitary or to imperfect absorption of its secretion. In this connexion it is interesting to note that lesions of the anterior lobe of the pituitary have been frequently recorded after encephalitis lethargica, though possibly not of such an extent as to cause recognizable disturbance of its function.¹

The last point which I will mention is that psychoses of the constitutional type probably do not occur, except accidentally, in relation with encephalitis lethargica. One reads of states of mania, of melancholia, of dementia præcox arising in or after the disease. These states are not of the well recognized constitutional type. Psychomotor exaltation certainly occurs, but it is of the toxic, psychotic type and the extremely rapid free association of thoughts, and the flight of ideas seen in *mania* do not occur. Moreover, the patient usually has some insight into his condition.

Depression, again, is not uncommon after encephalitis, especially in the Parkinsonian cases, and these patients often become very ego-centric in their ideas, sometimes hypochondriacal to the point of losing interest in everything else, though they recognize that their thoughts are too much occupied with their own state, which is depressing enough. In this depression, however, there is usually no real retardation of thought, the physical picture is quite different, and again there is good insight into their condition.

I have neither seen nor read of *paranoia* or *paraphrenia* occurring in relation with encephalitis. Delusions, when they occur, are weakly held though persistent, and show neither progressive accretion nor systematization. One such patient complained of a feeling that people were talking about her, that some harm threatened her and her family, and she heard voices abusing and threatening her. She had mild Parkinsonism and was depressed, dull and inactive. This delusional state had lasted more than a year and still persists, but is vague. She is prepared to discuss her delusions, and is half convinced at times that her delusions are false yet unable to rid herself of them.

There are remarkable symptoms associated with the Parkinsonian syndrome, especially when occurring with bradyphrenia, which have given rise to the statement that dementia præcox is a sequel of encephalitis lethargica. The most striking of these are: (1) The hypertonus of the antagonistic muscles in attempted passive movements of the limb in Parkinsonism, which resembles slightly the negativistic resistance of dementia præcox. (2) The fixed vacant smile occurring in both disorders. (3) The perseverations and sudden arrests of thought seen in both. (4) Bizarre attitudes, compulsive acts and stereotypy in both. (5) The extreme slowing of the thought process, with loss of interest and lethargy referred to above, and the slowness, dullness, and detachment from reality of the precocious dement.

But the differences between the two conditions are easily recognized, especially the mental clarity and coherence of the post-encephalitic patient as compared with

¹ "Morbid Anatomy of Epidemic Encephalitis as regards the Endocrine System." Howe, *Neurological Bulletin*, March, 1921, p. 92.

the apparent obfuscation, incoherence and inaccessibility of the dement. At the same time it is interesting to note that there is a resemblance, as Naville¹ has recorded, between katatonics, chronic dementers and these post-encephalitic patients in respect of gait, stance, rigidity, tremors, muscular twitching and disharmony of associated and automatic movements, especially as in such dementers basal ganglia lesions have been reported by Dide and Lavastine.

Psychotic sequelæ do not conform, then, to the patterns of the constitutional psychoses but are best understood as manifestations of organic cerebral disease.

Dr. C. WORSTER-DROUGHT.

In comparison with the frequency of gross physical disabilities, cases that exhibit serious mental disorder—that is, mental disorder reaching certifiable limits—are, in my experience, comparatively rare. Of the many chronic cases I have seen since 1918, very few have become definitely insane. When this has occurred, the condition has been either a progressive dementia, a delusional state, or more rarely a hallucinatory confusion. Minor psychical abnormalities, however, abound—in fact, very few cases escape them entirely.

In my experience, there are no really characteristic mental symptoms. The most frequent one, perhaps, is an apparent apathy—particularly in the Parkinsonian type of the disease—while lack of spontaneous interest, of initiative, and rapid fatigue on mental effort are often present. Personally, I believe that the apathy is more apparent than real and that the cerebration is not so slow, nor the patient so stupid, as has been considered. One may be so easily deceived by the picture of slow responses and sluggish movements.

In dealing with many cases of chronic encephalitis lethargica, it appeared that the patients showed little or no impairment of intellect and that the so-called apathy consisted mainly in retardation of the somatic efferent component of the nervous system—speech, expression, motility, &c. With the object of testing this hypothesis, Dr. Hardcastle and I subjected a series of nineteen cases of chronic encephalitis—chiefly of the Parkinsonian type—together with normal controls, to certain time-reaction investigations. By means of an electrical apparatus fully described in the *Journal of Neurology and Psychopathology*, August, 1924², we obtained in each patient: (1) the psychomotor reaction time; (2) the cerebration time for selection.

As compared with a number of normal controls, the psychomotor reaction time was considerably lengthened in all the cases of residual encephalitis (an average of 0.36 second as compared with a normal average of 0.24 second). In only three patients, however, did the cerebration time for selection exceed the figures obtained in normal subjects, while the average cerebration time for the nineteen patients was slightly less actually than the average in normal controls (0.14 second as compared with 0.18 second). We concluded, therefore, that in most cases of residual encephalitis lethargica: (1) the psychomotor reaction time is definitely lengthened—usually by about 50 per cent; (2) the actual cerebration time is not appreciably affected.

To general intelligence tests, only three patients fell below the standard adopted from our investigation of normal subjects, the deficiency in these three cases being chiefly under the headings of "reason" and "memory."

Profound Parkinsonism may be mistaken for dementia præcox; the resemblance, however, appears to be in extrinsic physical signs only—especially of katatonia.

A striking feature in sufferers from residual encephalitis is the frequency of psycho-neurotic symptoms—such as apprehension, undue self-consciousness, associability,

¹ *Loc. cit.*

² C. Worster-Drought and D. N. Hardcastle, *Journ. of Neur. and Psychopathology*, 1924, v, pp. 146-150 [January 27, 1925.

obsessive ideas, terror dreams, &c. In this respect, one's observations entirely agree with those of Jones and Raphael, viz., that a latent psychoneurosis before the illness may afterwards become manifest. There is no doubt that encephalitis lethargica leads to a definite lessening of volitional inhibition, consequently psychoneurotic tendencies, previously well under control, are no longer adaptable. The same reasoning applies to so-called moral deterioration—the immoral tendencies were no doubt latent and with the advent of encephalitis ceased to be inhibited.

The prognosis of such moral deterioration following encephalitis lethargica is often quite good. Two of my adolescent patients with a history of abnormal sexual conduct following an attack of the disease eventually returned to normal behaviour. One had actually suffered imprisonment for his offence.

The completely unmanageable child is well known. As in a case I showed at the last Clinical Meeting of the Section of Neurology, the unruliness may be associated with symptoms of dyspituitarism—obesity, increased sugar tolerance, &c. The symptoms of disordered pituitary function are probably due to involvement of the nuclei of the tuber cinereum—above the pituitary gland—rather than to disease of the pituitary itself. So far as I am aware, no lesions of the pituitary gland have been discovered at autopsy in cases of encephalitis lethargica, while Camus, Roussy and others have produced typical dyspituitary syndromes in dogs by comparatively slight injury to the base of the brain above, but not involving the pituitary body. This observation is of interest, as a few years ago I recorded a case of moral imbecility associated with dyspituitarism apart from encephalitis lethargica.¹

I have also seen a case of precocious eroticism associated with somatic pubertas præcox following encephalitis lethargica. In such a case one is tempted to ascribe the symptoms to involvement of the pineal body.

In the larger number of child delinquents the prognosis, in my experience, is good. Given sufficient time, a gradual improvement usually occurs both in self-control and behaviour.

Dr. D. N. HARDCASTLE

said that Dr. Worster-Drought had already stated most of the results of their work together on this matter. He (the speaker) was anxious to stress what they found to be the psychological aspect associated with the lethargy or the psychomotor retardation which was discovered in patients.

Seventeen of the nineteen cases investigated were of the Parkinsonian type, and they were all definitely chronic cases, not acute. In dealing with the cases, he felt that if one could get down to what was beneath the surface, the patients would be found to be not so apathetic as the lethargy would at first lead one to think.

Another point was, that there seemed to be a great likeness between these cases and those of "shell-shocked" soldiers which he saw at "shell-shock" hospitals, especially with regard to the alteration in the sleep-cycle. He was tempted to regard it as having a "psychological basis." In encephalitis lethargica there was usually some definite damage to the base of the brain, which might involve the sleep-centre, while in the shell-shock cases it was doubtful if there was any such damage, although Sir F. Mott had demonstrated the existence of punctiform hæmorrhages in some cases; but the difficulty of getting to sleep and the sleeping at abnormal times were striking in both.

He fully agreed with Dr. Worster-Drought as to the fact of latent psycho-neurotic tendencies sometimes becoming evident afterwards. The psycho-motor retardation, by increasing the difficulty of the patient's adaptation to reality, might lead to the development of psycho-neurotic symptoms in these latent cases, as a refuge from reality. And he felt that the same would apply to the psychoses. He had had two

¹ *Journal of Mental Science*, January, 1921.

patients whom he had treated by psychotherapy during the last eighteen months. They had been suffering from anxiety and depression, and one had marked dreams and phobias. After the psychotherapy the phobias cleared up completely. Therefore he thought that these cases might be regarded as having a definite psychological basis rather than being determined by any definite organic injury.

Professor Hall, in his description of the "difficult child" group, referred to neurasthenics, and Dr. Shrubsall also called attention to similar conditions. In the psycho-neurotic there was difficulty of concentration, a lack of attention, and the rapid onset of fatigue.

There were one or two other points noted in the nineteen cases investigated. For example, the onset in some was preceded by a definite and striking sense of well-being; in one or two cases there was certainly euphoria. One speaker referred to impairment of memory. In this series of nineteen, he (the speaker) carefully took from the patients their history of the illness, and, except in the case of the patients who had been in a condition of profound sleep for a time, a graphic account of the illness was forthcoming. He did not think that the patients were fabricating.

A further point was, that the onset of the mental changes, in these nineteen did not coincide with the onset of the fever or the defervescence, but was considerably delayed, often not becoming established until the lethargy had well set in. In one case there was a six months' interval after the acute onset. With the establishment of the lethargy in that patient there was the onset of phobias, fear, anxiety, &c., there being a period of apparently complete recovery in the interval.

Dr. EDWARD MAPOTHER.

During the past two years forty-five cases of encephalitis and its sequelæ have been under observation at the Maudsley Hospital—twenty-two as out-patients and twenty-three as in-patients.

I do not propose to make any complete analysis of this series as that would involve much useless repetition of what has been said here and elsewhere, but rather to emphasize certain special points.

Our cases of course throw no light on the total frequency of mental disorder because with few exceptions the patients were brought on account of mental disorders in a wide sense, or at any rate on account of what were regarded as such.

The bulk of the symptoms in most cases fell under three heads:—

- (1) *Mental anergia* in various degrees, from difficulty of sustained attention to morbid sleep.
- (2) *Morbid restlessness* in various forms, and insomnia.
- (3) *Demoralization*.

Our cases might serve to illustrate the respective frequency of different types of mental symptoms, but probably there is a large fallacy in that symptoms of a positive kind are much more likely to lead to advice being sought at a hospital catering for mental troubles. These cases should, however, serve to illustrate any correlation of either mental disease as a whole, or special types of this, with particular features of the onset, or with other sequelæ. Such correlations are very hard to trace and it seems almost impossible to find any rules without numerous exceptions. Dr. Duncan, who traced the after-histories of a large number of cases at the London Hospital, thought that positive mental symptoms during onset (e.g., delirium and restlessness) were negligible, but that marked negative symptoms (e.g., lethargy) were ominous. So far as one can judge from the retrospective history obtained in cases at the Maudsley Hospital there is little in this opinion. It may be statistically correct but there are so many exceptions that I think the rule would be useless for individual prognosis.

As to the relation of mental disturbances to age, in twenty of our cases the age was under 14, in nineteen between 14 and 30, and in only six over 30.

34 Mapother: *Mental Sequelæ of Encephalitis Lethargica*

Extreme restlessness of various forms was by far most common in quite young children under 8, demoralization far most common in cases starting between 8 and 20. Both seem rare in adults in whom the negative type of symptoms predominate.

There is no constant proportion between the organic and the mental sequelæ.

On the whole, mental anergia tends to prevail more in cases showing organic Parkinsonism, but either condition may be marked without being accompanied by the other. Restlessness, on the other hand, is commoner where Parkinsonism is slight, but they may co-exist.

There would seem to be *a priori* obvious antagonism between mental inertia of various degrees and restlessness, but either may be paroxysmal and they may alternate in various ways.

Demoralization is in practically no proportion to the rest of the symptoms. It may be well marked in a case with or without anergia and with or without restlessness.

I agree with Dr. Cloake that the mental disturbances do not resemble those of ordinary constitutional psychoses. Those of encephalitis seem to have two characteristics of outstanding importance: (1) the predominance of conative disturbances; (2) the marked spontaneous variation from time to time.

(1) As to the first point, the position is analogous to that on the organic side.

On the organic side sensory symptoms such as pain and hyperæsthesia to heat may occur, and also visceral troubles like salivation, tachycardia, and disturbances of respiration or of liver function, but motor symptoms predominate.

So, occasionally, intellectual and affective disorders and their consequences may be present in some cases, but conative disturbances are the outstanding mental features of most.

As to negative intellectual disturbances, in the absence of definite sleepiness one is continually surprised by the intelligence and the slightness of defects in perception and memory. Inattention seems to explain most of what defects there are. In the minor grades of anergia the patient has much greater difficulty in initiating manual performances than in calculation. Generally the patient fully appreciates his own slowness and inefficiency and is much distressed at this. He often has a sense of effort and fatigue and it is a question how far the need of continual efforts may help to cause the sleepiness.

Demoralization bears no relation to impairment of intelligence. The patient is, as a rule, normally capable of recognizing as an abstract proposition the quality of his delinquencies. They seem to be due to defect of inhibition, so that a wish results in action without consideration of results. Some of the demoralized children are described as being abnormally devoid of fear.

As to positive intellectual derangements such as delusions and hallucinations, these seem very rare, apart from the delirium of onset. They are usually quite absent in later types of motor excitement.

There is usually no evidence of dreams accompanying the drowsy state.

Lying is common in the demoralized type, but it is conscious lying, not fluent production of fantasy in which the patient at least half believes, as in hysteria.

As regards emotional changes, morbid facility of laughing and crying existed in only one case.

Apathy seems commoner than excess in any one direction. Initial depressions play a smaller part than in most ordinary psychoses.

There is a striking absence of definite emotional exaggeration accompanying the restlessness in encephalitis, contrasting with true manic-depressive insanity and resembling rather the restlessness of the senile.

(2) The second striking characteristic of the mental symptoms following encephalitis is the tendency at least in many cases to *marked spontaneous variation*. This suggests the continuance of an active process of disease.

Cases showing usually only a moderate degree of retardation may at times lose all capacity for spontaneous activity and become, though awake, like a decerebrate animal.

There are two sorts of restlessness, a continuous fidgety type and paroxysms of much greater intensity. Some of the demoralized cases show the tendency to their delinquencies continuously, others only at intervals.

Probably those two points, the predominantly conative nature of the symptoms and their frequently variable character, are responsible for the rarity with which these cases are certified. A recent investigation of all the Mental Hospitals of the London County Council revealed only fifteen cases due to epidemic encephalitis. Theoretically, of course, the need for detention should rest on conduct. Practically certifiability depends on the possibility of stating convincing facts observed at an interview and, usually, to satisfy a magistrate, there needs to be evidence of gross intellectual disorder. Many of these cases seem obviously to need institutional treatment, either because the patients are incapable of self-support or because they are a nuisance to others. The demoralized are undoubtedly better off and better behaved in an institution than outside. But they seem ineligible for treatment under existing arrangements unless the mental is coupled with well-marked physical disorder. They supply another argument in favour of providing for the pure voluntary treatment of uncertifiable degrees of mental disorder under acceptable conditions.

Lastly, with regard to the diagnosis of these cases from purely functional ones. This may be rendered difficult by the fact that abnormality is mainly in conduct and very variable and therefore one has to depend on history. But the distinction I think should be made to depend on whether the more explicit physical signs, such as the Parkinsonian syndrome or cranial nerve palsies, are, or have been, present. All other indications such as febrile attacks, sleepiness, moral changes, choreiform movements, are tempting but insufficient foundations.

There is a tendency to suspect encephalitis in many patients with a history of such things when the case is really amentia, dementia præcox or hysteria. On the other hand, at least as common is the mistaking a case of encephalitis for hysteria through overlooking physical signs or misinterpreting them.

The only two cases of acute encephalitis treated at the Maudsley Hospital were both sent in as neuroses though they had well marked ocular palsies. One had been ascribed to fear of a boiler explosion a week before, and the other to anxiety regarding marriage, which was due in three weeks.

Another very striking case illustrated this and a second source of fallacy, viz., that not only is there much spontaneous variation in all types of symptoms, but often also much increase in capacity with attention and effort.

In the Parkinsonian state all defects are much more marked in regard to what is habitual and automatic than in regard to the best of which the patient is capable. Occasionally one does see hysterical exaggeration of real incapacity and utilization of it for the patient's own purposes. But, generally, on the contrary the readiness with which the patient co-operates and puts forward the effort necessary to reduce his habitual disability is quite different from what is seen in hysteria.

Dr. GRAHAM FORBES

said that in the course of public health work, and for the purpose of diagnosis and for notifying infectious conditions, he had had the opportunity of seeing a considerable number of cases of encephalitis lethargica in the acute stage; but it had not been his good fortune to see much of the post-encephalitic condition; that had fallen to the lot of his colleague, Dr. Shrubbsall, whose very valuable contribution was listened to with great interest a fortnight ago in regard to children of school age. Perhaps time would show, but he would like to know, from those who had had considerably more experience than he had, whether epileptic manifestations might be added to

the formidable sequelæ associated with encephalitis. He had not heard of such. He had had, however, in the acute stage of encephalitis, to deal with cases in which this disease complicated epilepsy.

He had seen two such cases, and had notes of a third, in which it was extremely difficult to know whether one was dealing with a post-epileptic condition, or whether it was simply the lethargy of acute encephalitis. One, a girl, aged 14, who had been under treatment at the Royal Northern Hospital for some time on account of severe epileptic attacks, was seized with drowsiness without rise of temperature, and the speaker was called in to see her. With this history of epilepsy, he was very puzzled as to whether a recent epileptic attack was the cause of the lethargy, or whether it was a new complication in the form of an acute attack of encephalitis. On the whole, he was inclined to think it was probably a post-epileptic condition. However, she went to the Royal Northern Hospital as an in-patient, and the conclusion formed there was that it was acute encephalitis occurring in an epileptic subject. Apparent recovery from encephalitis took place in the course of two months, but she continued under treatment for epilepsy.

In the second case referred to, he was in the same dilemma, and he had not been able to decide whether the condition of drowsiness without temperature or other signs was due to encephalitis or whether it was a post-epileptic condition. The patient was a girl of 18, for three or four years subject to epileptic attacks, which subsequently continued when the lethargy had passed off.

He and his colleague, Dr. Brincker, were following up, as far as they could, all the cases which, in the course of their public health work, they were called upon to visit at the request of medical officers of health and practitioners in London, with a view of helping in diagnosis and deciding whether the patients were notifiable as cases of encephalitis.

It was important that every effort should be made to follow up the cases, whether in adults or in children, in order to ascertain the subsequent course, and with the help of the medical officers and general practitioners concerned it was hoped that in that way the nature of some of the cases, often at first obscure, might be cleared up.

DR. CRICHTON MILLER

said that there were two questions he would like to ask those who had had more experience than he had, especially the neurologists. From his experience of the mental results in the Parkinsonian type, it seemed that sedatives, particularly hyoscine, had an unquestionable, almost a specific, effect on the apathy and the anergia. He did not know whether that conclusion was generally accepted. His second question was, if what he had just said was accepted, what was supposed to be the mode of action of hyoscine? He could not think of any other condition in which anything so striking was observed in the way of a re-integration, or a partial re-integration, of the psyche from the administration of a sedative drug.

DR. ALICE HUTCHISON

asked whether there was any consensus of opinion as to how long children who had suffered from this disease should be kept away from school.

DR. C. WORSTER-DROUGHT (in further comment)

said he had now under his care four cases of epilepsy in whom the seizures dated from an attack of encephalitis lethargica. One was a boy aged 10, two were girls aged 19 and 21, and the other was a woman aged 39. In one of the girls there was an associated hemiparesis of mild degree. These cases, as a rule, responded very well to luminal.

He agreed with Dr. Crichton Miller that hyoscine had a good effect, especially in cases of the Parkinsonian type. The objection to its use, however, was that once administration of the drug had been started it had to be continued indefinitely, as the

patients immediately got worse when it was discontinued. He thought it acted as a sedative by lessening the tremors, and to some extent the muscular rigidity, and rendered the patient more comfortable generally.

Dr. P. C. P. CLOAKE

said he was interested in the relation of psychic trauma to the onset of encephalitis lethargica. He thought that it was remarkable how often one found a history of some psychic trauma, even allowing for the readiness of relatives to remember something of the kind. He had learned of a psychic trauma in a number of cases, sometimes a serious one, which occurred at or about the time of the onset of the encephalitis. It did not seem impossible that a slow encephalitis might be aggravated and made more apparent by a mental shock. In one case he had, the boy was forcibly pushed into a bath against his will, and encephalitis began on the next day; he wondered whether there might not be a connexion between the two factors.

Dr. Worster-Drought's research pointed to the paramount factor in the lethargic and asynergic state as being a psychomotor retardation, with the accent on the motor execution of the actions. That finding was not in agreement with those of Naville and Claparède, who also had worked on the subject. Although they noted that retardation was considerable in many cases, they found, in addition, that by increasing the difficulty of the psychic side of the task the retardation was very considerably accentuated. They also performed tests which were intended to eliminate, as far as possible, the motor element of the retardation, and they found there was a very considerable psychic retardation, which they considered to act primarily in the psychic order, the fiat which set the motor action going. Hence, if their results were reliable, there was a very definite psychic retardation, apart from the execution of motor acts. That he understood, Dr. Worster-Drought found in only three cases. Naville attached more importance to the lethargy, especially in the Parkinsonian patient, than Dr. Worster-Drought did, and the latter more than Naville did to the motor element.

Dr. Worster-Drought also said he had no knowledge of any damage to the pituitary gland in these cases. He knew of two papers in which damage to the pituitary was recorded in a number of cases. Howe, in the *Neurological Bulletin* (*loc. cit.*, see footnote, p. 30), published an investigation of the endocrine glands following attacks of encephalitis, and the only gland which seemed to be at all abnormal in appearance was the pituitary, i.e., its anterior lobe; but he considered the damage was possibly insufficient to cause symptoms of an alteration in the function of the gland. In the case of the endocrine glands, ordinarily there was so much more of the gland than was necessary for the maintenance of its function, that slight damage to it would still leave sufficient healthy gland to continue the function unaltered. It had been shown that damage to the tuber cinereum, to the hypothalamic grey matter, and to the infundibulum, were alone capable of causing hypopituitary syndromes.

With regard to the use of hyoscine, the experience of many people was that hyoscine was only really effective when it was injected, and the difficulty was that as a rule the injections had to be left in the hands of the patients and their friends when the patients left hospital. It was given in cases of cerebral excitement, and he was surprised to hear that the lethargy was improved by it, though it would be easy to understand that the rigidity and the tremor would be improved by a sedative action on the cerebral cortex and upon the motor nerve endings in muscle.

Concerning the return of children to school, the essential thing to be realized in the treatment of this disease was, that it was a very serious form of cerebral injury, and, as in other forms of cerebral injury, the longer the rest the patient got after the injury, the better, on the whole, the prognosis was likely to be. For that reason

he had advised parents to keep these children away from school for a longer period than seemed necessary; and that was important in view of the possibility of sequelæ developing many months later.

In conclusion, he wished to stress again the aspect he put forward when the discussion was initiated. He considered that the essential feature of the psychic change was that it resulted from a definite organic injury to the brain. One could understand the mental sequelæ best if one appreciated that the highest psychic functions were likely to be most damaged, and that those functions were not necessarily intellectual, but rather those of self-control and the ability of the patient to adapt himself to his social environment, which were the latest acquired functions of the human mind.

The CHAIRMAN (Dr. R. H. COLE)

extended the thanks of the Section of Psychiatry to those members of other Sections who had come forward and contributed to the success of the debate, which had resulted in a better understanding of this disease.

It was true that psychiatrists did not see many of these cases; probably he had not seen more than a dozen, and he would refer to some of them.

He believed the disease was infective in only a very minor degree, but difficulties arose.

Only that day he had learned of the death of a patient with the disease about whom he was consulted ten days ago, a well-to-do clergyman, who had been seen by one or two distinguished neurologists. He was so unmanageable that he had to be removed from his vicarage, and the speaker considered that he would have to be certified. He gave addresses of several institutions, but not one of them would take him, because of the nature of the disease; though he learnt that there was no objection to receiving such cases in the ordinary wards of the Maudsley Hospital.

It had fallen to his lot to have to report recently on an epidemic of encephalitis lethargica in Kent. A boy, who undoubtedly had the disease, was removed to St. George's Hospital; then his nurse became a victim, developing diplopia, lethargy, &c., and the matron finally developed similar symptoms. The officials of the infirmary where it happened became alarmed, and wired to London for a "brain specialist," after communicating with the Ministry of Health. He (the speaker) reported that the other contact cases might have been coincidences, and that all possibly had derived it from the same source. People who had patients in private homes were naturally afraid of the idea of infection.

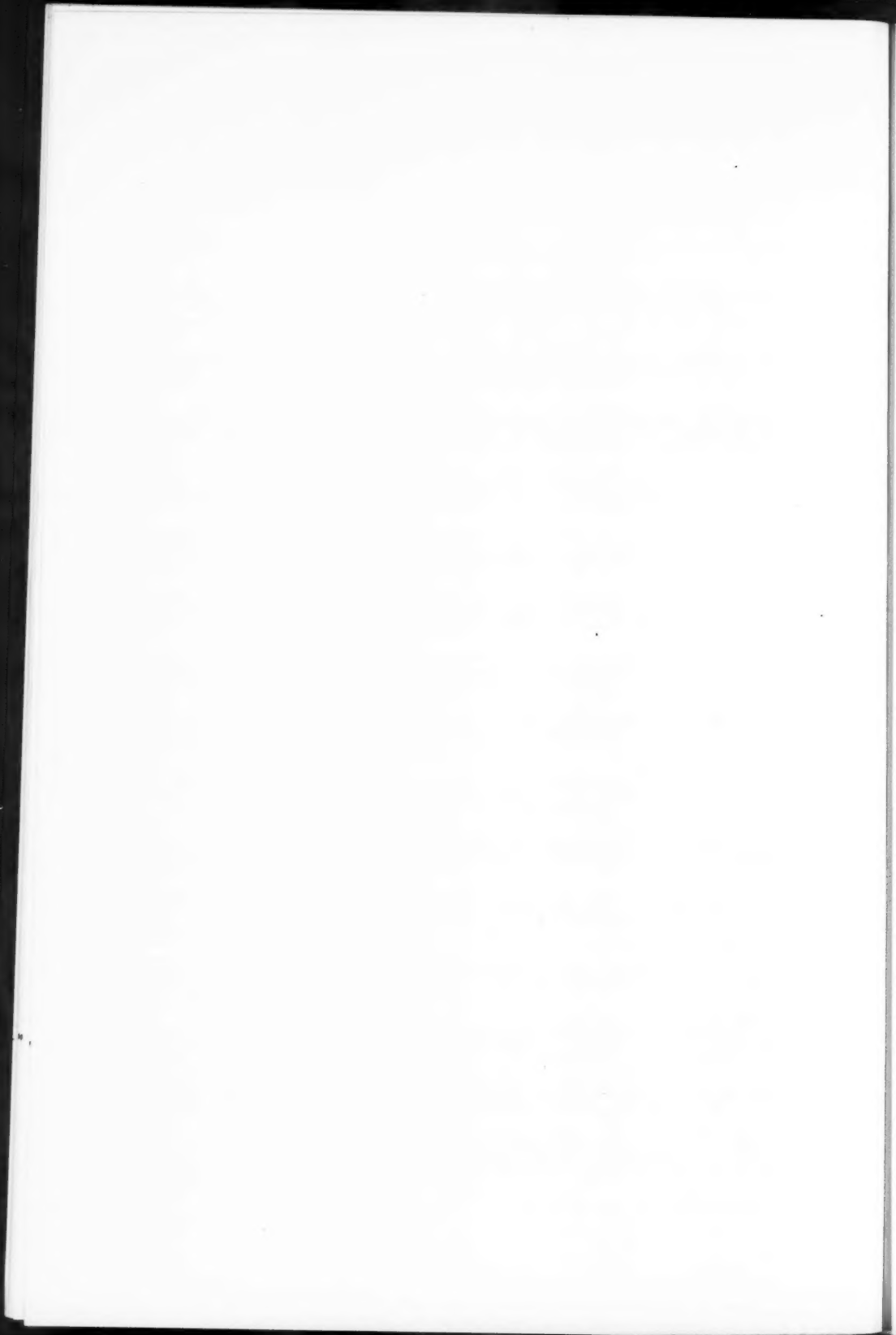
He would shortly be seeing a boy of 15, who had been the subject of the disease. His father was a policeman, and his position was becoming impossible owing to the boy's vagaries. The onset occurred nine months ago, and latterly he had been lying and thieving and constantly getting into trouble. There was a reluctance to certify a boy of 15 under the Lunacy Act, and it was not quite proper to bring him under the Mental Deficiency Act, which stated that the mental affection must have dated from birth, or the moral imbecility must have been from a very early age. He therefore referred the decision to the Board of Control. The boy's intellect was stated to be clear, but his conduct was incorrigibly wrong. As a Visitor under the Mental Deficiency Act he had seen many cases of moral defect in children, and to class a child intellectually normal as a mental defective owing to encephalitis lethargica would be going further than seemed strictly correct.

With regard to hyoscine, its action and value in psychiatry was well known, and he thought it was agreed that it was a drug which should be used only in acute cases as an emergency measure, and not for any prolonged time. Patients who had recovered from mental disorder had afterwards complained to him of the drug, as its effects seemed to bind them as in a vice: there was a desire to move but an inability to do so. It had even been described by patients as a "cruel drug." He believed it deteriorated the nervous tissue of an irritable brain. In some cases of paralysis agitans and encephalitis lethargica undoubtedly it did good, especially in the Parkinsonian type of the latter; but in other cases he felt it was better to have

the disease than take the drug. Some people indeed regarded the administration of hyoscine as a measure of unjustifiable chemical restraint in psychiatry. A drug which had not been mentioned was urotropine. He believed this formed some compound in the cerebro-spinal fluid, and was beneficial to mental cases of toxic origin.

One man treated in a hospital for encephalitis lethargica was sent down to his out-patient department afterwards, and he formed the opinion that the patient was a general paralytic, which proved correct. He believed some cases of influenza were diagnosed as encephalitis lethargica, especially where the temperature was high. In another doubtful case, to which he was called in consultation by the local doctor, there was diplopia, and the man was confused and hallucinated, having apparently become so rather suddenly. There were no signs of general paralysis, and no history of syphilis or alcoholism. He died in ten days. Post mortem, there was nothing to indicate the cause of death, which was probably encephalitis lethargica.

He did not think it likely that there would be found many cases of post-encephalitic sequelæ in the mental hospitals.



Sections of Surgery, Anæsthetics, Medicine, Obstetrics and Gynæcology, and Pathology.

DISCUSSION ON THE PREVENTION AND TREATMENT OF POST-OPERATIVE PULMONARY AFFECTIONS.

Mr. E. C. LINDSAY.

THERE is a growing belief that the post-operative lung complications such as pneumonia, pleurisy and collapse, which have been an endless source of contention and mutual reproach between surgeon and anæsthetist, are not the simple matter of either irritation or congestion, and infection, as has been alleged. Wharton and Pierson, in the *Journal of the American Medical Association*, 1919, stated it as their considered opinion, after the investigation of 1,600 gynæcological operations, that 40 per cent. of the cases of pleurisy, and 12 per cent. of those of pneumonia, after operation, were in reality cases of pulmonary embolism and infarction.

It would be as well, at this stage, to point out that the size of the embolus is the important factor, and, dependent on this, we may distinguish:—

(a) The classic type of massive embolism, occluding one or other pulmonary artery, or bestriding the bifurcation and obstructing both; more usually the right is affected, as Rupp has shown.

(b) The infarction type, with severe onset followed by pleurisy and by signs of consolidation.

(c) The type in which the site of the embolus prevents recognition as it is an infarction on the inner surfaces of the lobes. This type is associated with slight increase of the rate of respiration, a slight evening rise of temperature, and the expectoration of small collections of blood and mucus.

There is but little mention in English surgical literature of this last type, but De Quervain, Wharton and Pierson, and Lee, all recognize its significance in giving warning of the onset of the more severe types. Two cases of my own illustrate this condition.

Case I.—Duodenal ulcer. Patient, a male, aged 52, upon whom a gastro-enterostomy was performed. On the fifth day he expectorated a little blood and mucus, as he described it, "from the back of the throat." There was no pain in the chest on deep inspiration, but his temperature rose to 99.4° F. for two or three nights. His chest was examined on two or three occasions by a physician, and no physical signs could be detected. We contented ourselves with the old story of a temporarily lit-up tuberculous focus (an illusion which dates back to my house-surgeon and registrar days). The whole disturbance quieted down, the patient was up and about, until, on the eighteenth day, he had an urgent desire to defæcate. He was then attacked by intense dyspnoea and cyanosis, and expired in five minutes, with all the signs of massive embolism.

To digress for a moment, it will be noted that this patient had a sudden desire to defæcate followed by the signs of embolism. My experience has been that this is a common premonitory sign. The ward-sisters at the London Hospital who see these unfortunate accidents occur, state that it is much more commonly the case for a patient to call for a bed-pan and fall back dying, than to have the attack while actually on the pan. The only explanation I can offer is, that the clot, moving up from the femoral or iliac vein, sets up a vaso-motor reflex (or it may be a congestive effect) in the sigmoid flexure of the colon, which the patient interprets as a need for evacuation of the bowels.

Case II.—Patient, a male, aged 26, in a ward at the London Hospital at the moment. I removed his gangrenous appendix and he went on well until the ninth day, when he expectorated the same type of collections of blood. There was no disturbance of the chest at all in this

case; my corresponding physician did not detect any signs, although he knew my suspicion. The evening temperature rose to 100° F. On the thirteenth day, definite signs of thrombosis of the veins of his right calf were detected.

Sir Charles Gordon-Watson has emphasized the importance of this slight rise of evening temperature; Wharton and Pierson have given it as a constant premonitory sign. Such a type is not common, yet its origin is presumably the same as that of the massive type (*a*). I would emphasize once again this deciding factor—the size of the embolus, in its relation to the result produced.

On looking hurriedly through the literature I was struck by the absence of any really large collection of cases, and also by the fact that there was a general tone of alarm at the increase in the frequency of the condition—an alarm for which I hope to show later there is no real ground. I therefore had recourse to the records of the London Hospital on the subject of the massive embolic type (already referred to as type (*a*)) and found that certain cases of pulmonary infarction (type (*b*)) were also included under that heading. I began by taking all cases of pulmonary embolism in the surgical records from 1914 to 1924, and found that the total for these years was 152; but as the notes for the war years, 1914-1918, were scanty and not altogether reliable, and the proportion of autopsies to deaths very much lower, I excluded thirty-eight cases occurring in these years, leaving 114 cases.

Incidence.—It has been a matter of comment that the condition is on the increase. I certainly shared that impression when in one year among my private patients I had two cases of infarction, and one of massive embolism—all three perfectly clean cases. And I notice that Sir Charles Gordon-Watson has had a similar experience. Some surgeons state that about 1916 they began to notice an increase in these tragic complications of operative surgery. In the war we saw practically none of them, and the reason is clear now that it is generally recognized, as Mr. Lockhart-Mummery has stated in his address, that sepsis is only an incident in its production, and that the principal factor is venous stasis in patients of advancing years. One eminent physiologist has advanced the theory that there has been a change in the character and coagulability of the blood, possibly dependent on national food. About 1916 we were undoubtedly feeling the full force of the importation of foreign albuminoids, such as Chinese eggs and American bacon. Certainly the figures over the whole eleven years would indicate that the numbers have been greater since the war; at the same time, the numbers of each year since the war remain about the same. It is probable that the old diagnoses of sudden heart-failure and shock after operation included a fair proportion of passive embolism cases.

In the series presented here, there are five fracture cases, three carcinoma cases, and one prostate case, in none of which operation was performed. There were, in addition, nine cases of sepsis, carbuncles, abscesses, &c., in which simple incision only was made. The post-operative list is thereby reduced to 96 (and this, I think, may be accepted as the mortality from this source) in 31,426 operations for the years 1919-24, which works out at 0.3 per cent. This agrees fairly closely with Rupp's figures published in the *Archiv für klinische Chirurgie*, 1921.¹ He found that as the result of investigations of 22,689 operations from 1903 to 1920 inclusive, there was a mortality of 0.26 per cent. from pulmonary embolism, a difference of only 0.04 per cent., or one in 2,500. From the figures of 1919-24 at the London Hospital we may fairly conclude that its frequency remains much the same. It must be remembered, however, that with the large increase in the number of operations performed, the absolute mortality of pulmonary embolism will naturally rise.

Lest it should be thought that surgery is mainly responsible for cases of pulmonary embolism, it is of interest to recall here the collateral figures which were

¹ "Post-operative Thrombose und Lungenembolie," *Arch. f. klin. Chir.*, 1921, cxv, pp. 672-688.
 "Zur Lokalisation der Lungenembolien," *ibid.*, p. 689.

published by Rupp from 13,000 autopsies over the same period of eighteen years. He found that the mortality from pulmonary embolism in internal disease without operation worked out at 1·1 per cent., that is, four times as great as that following operation.

The Age Factor.—On analysing the series on this point, a highly important fact emerges; i.e., that massive embolism is a disease of later life. The average age for this series was fifty-two; for some diseases it was higher. The urinary bladder and prostate operations worked out at sixty. Even appendicitis cases showed an average age of 40·4 years, and, considering that there were included three cases whose ages were respectively 2½, 11 and 13 years, the average age of the remainder can only be considered as a very late age for acute appendicitis to occur. The average age of cases of hernia suffering this complication was 53.

If, further, we examine the frequency in different types of operations (and I may say here that the short time at my disposal has not allowed of the complete analysis of 31,000 operations) we find that in stomach operations twenty-one occurred in 2,778 operations; in major gynæcological operations (excluding obstetric cases) twenty-two in 2,004; in appendix cases seventeen in approximately 4,000, and hernia twelve in 1,967. In other words, as in other published series, gynæcological operations head the list.

All groups show that it is in the older cases that this complication occurs. This may be some explanation of the apparent increased frequency, as there is no doubt, with the perfected technique and team-work of recent years, age has not been considered the great contra-indication to operation that it once was.

Site of Thrombus.—Post-mortem examination has revealed in 42 per cent. of cases thrombosis in the veins of the pelvis and lower extremities, 50 per cent. of which were in the left iliac or left common iliac vein. Wharton and Pierson agree with this when they state that 41 per cent. of cases are associated with femoral phlebitis. In gynæcological cases, thrombosis of the pelvic, iliac and femoral veins was found in 47 per cent., and in the stomach cases thrombosis of similar veins was discovered in 57 per cent. of cases. It can definitely be said that this is lower than would have been the case if full post-mortem examinations of the whole body had been allowed, and not of the abdomen only, as is so often the case after death following abdominal operations. I have mentioned these two types of operation especially, as one is portal and the other systemic. In none of the notes of the post-mortem examinations was there any mention of extensive thrombosis in the area of operation. In many of the cases crossed thrombosis occurred. As has been noted above, the left common iliac vein accounts for 50 per cent. of the cases in which a thrombus was discovered. Presuming that stasis is one of the most important factors, with the sigmoid flexure and the rectum emptying irregularly, or with the aid of enemata, it is possible that there is a greater stasis on the left side. The less direct drainage of the left common iliac vein may be a further factor. The time when embolism took place after operation in this series averaged out at the tenth day for stomach, appendix cases, &c.

The Anæsthetic.—As the operations were practically all carried out under general anæsthesia, ether or its mixtures, I have no information as to the effect of anæsthesia. The question of primary pulmonary thrombosis has been discussed at length by Professor Glynn, and commented upon by Mr. Lockhart-Mummery and by Sir Charles Gordon-Watson. Professor Glynn claimed that of his thirty-five cases death was due in thirty to primary pulmonary thrombosis. While admitting that pulmonary thrombosis may be responsible for a certain proportion of cases of sudden death after operation, and more especially by giving rise to cerebral emboli, I think it is difficult to reconcile the high percentages of known sources of thrombi in this series with such an explanation. The size of the thrombus, corresponding to that of the big femoral or iliac veins, its frequently unbranched appearance, its considerable

length (as much as eight inches in one case and of very nearly equal diameter throughout), the fact that it is nearly always rolled up, seem to me to be strong arguments in favour of the formation of a thrombus elsewhere and not in the lungs. Rupp says the embolus is nearly always in the right lung. Finally, the pulmonary artery, though carrying venous blood, is arterial in type to support the blood-pressure arising in the ventricle, and it would seem unusual for a thrombus to become loose and move backwards against the blood-stream. The experience of all, I think, goes to show that the embolus in the lung is loose and coiled up. It has a thin external shell of lamination, and a core of red unorganized clot.

Summary.—(1) There is no evidence, from a consideration of statistics, that the condition has relatively increased. (2) Age is the most important factor in its production, the average age in the series being fifty-two years. (3) The primary formation of the thrombus occurs in the veins in which there is marked stasis, namely, the pelvic and femoral veins. In this series, in 42 per cent., this mode of formation was discovered. (4) Sepsis is probably only a contributory cause, as it acts by lowering the vitality of the patient, and therefore promoting stasis. Why thrombosis should occur in conditions of stasis in the aged and not in the young, seems to me to be a problem for the physiologist rather than for the surgeon, and when that has been discovered we shall be nearer to a method of prevention of this complication.

Dr. F. E. SHIPWAY.

For the purposes of this discussion I am not dealing with massive pulmonary embolism, because I think that the anæsthetic plays little if any part in this complication.

The two outstanding features of the two main post-operative pulmonary affections, viz., bronchitis and pneumonia, are (1) their more frequent occurrence after abdominal, especially upper abdominal, operation than after operation elsewhere, and (2) their rarity in private practice. A recognition of these facts helps to elucidate the problem of their ætiology, and therefore of their prevention.

Bronchitis, not often directly fatal in the healthy patient, may be indirectly the cause of death by leading to bursting of the wound and necessitating immediate operation with the risks of sepsis, of aggravating the bronchial affection, and of the formation of adhesions. It is a very serious complication in the unhealthy. It is important, also, for it is an essential factor in the causation of the great majority of the surgical pneumonias. It sometimes arises to a mild degree in a perfectly healthy patient after ether anæsthesia, and is caused partly by exposure, but chiefly by the cooling and irritating effects of high concentrations of ether vapour, which, moreover, lower the resistance of the lung tissues to bacterial invasion. Further, deep anæsthesia abolishes the regulation of the production of heat and of the loss of heat, and if protracted causes an appreciable fall in body temperature.

We have in these facts and in the immunity of chloroform anæsthesia from bronchitis, a sufficient explanation of the ætiology of this common affection, and are forced to conclude that profound and prolonged ether anæsthesia is definitely harmful, particularly to certain types of patients, and that, whenever opportunity offers, the concentration of the vapour should be materially diminished and relaxation obtained, especially in obstinate cases, by substituting or adding chloroform, or by the use of a local anæsthetic. Ether is definitely contra-indicated for asthmatics and bronchitics. Spinal analgesia is often invaluable for these patients. The wonderful results obtained in the war with gas-oxygen-novocaine are, I think, in danger of being forgotten; and I would again plead for the more frequent use of local anæsthesia in combination with light general anæsthesia.

Ether should, I believe, be given warm except in hot weather. Warm vapour causes less irritation and diminishes loss of body heat. The quality of ether demands consideration. Peroxides and aldehydes, highly irritating impurities, occur in appreciable quantities in some methylated ethers and quickly increase in the presence of

strong light. In choosing an ether, samples from several manufacturers should be subjected to the most searching tests, more searching than those of the British Pharmacopœia, which may well be repeated on the arrival of each fresh consignment.

The rarity of these pulmonary affections in private practice as compared with hospital practice, may be accounted for by differences in pre- and post-operative conditions and in the quality of anæsthesia. Further, there is little difficulty in ensuring punctuality in the time of the injection of atropine; this is a very important point.

Occupational exposure to weather is a potent factor; in a surprisingly large number of hospital patients, emphysema and chronic bronchitis are found. The patients are less well nourished and have habits different from those of the private patient as regards temperature and ventilation of living rooms, baths and sleeping attire. In hospital they are made to conform to the standards of a higher social class and often suffer in the process. Further, the design of the average hospital has not, for economic reasons, taken into account the large percentage of abdominal cases that will have to be nursed in the wards. The wards are too draughty, and cannot be maintained at the optimum temperature for each individual patient, and the patient on his return from the theatre is exposed to the draughts and cold air of lifts and corridors. In the spring and winter months, therefore, he is likely to catch a "cold" during the pre-operative period or after leaving the warm theatre. Nor is this difficulty met by operating in a cool theatre, for the loss of heat is greater. Further, the patient is more exposed in hospital to infection in times of epidemics of influenza and colds.

A recent "cold" is sometimes overlooked in the busy life of a ward. The history of one is not always obtained in private practice. Either from ignorance of its importance or a natural reluctance to postpone operation at the last moment, the patient may not give the anæsthetist any clue which will put him on the track. I have notes of three cases of pneumonia in private practice which illustrate this point.

Liability to bronchitis after operation other than abdominal, even though ether be the anæsthetic and anæsthesia is prolonged, is very slight. I have given intra-tracheal insufflation of ether in nearly five hundred cases for facial, nasal, buccal and pharyngeal operations. Many of the patients were advanced in years and were chronic bronchitics. Many of the operations were prolonged plastic operations. Bronchitis was extremely uncommon and pneumonia never occurred. I have also frequently anæsthetized patients suffering from acute and subacute infective conditions of the upper air-passages for operation on the nose or ear. I have never seen harm result. Such conditions, however, are an absolute bar to abdominal surgery, except in cases of emergency, and even then I cannot conceive of ether being chosen as the anæsthetic. There is, it is true, no necessity as a rule to use deep anæsthesia in such cases, so that ether bronchitis would be less likely to happen. But pulmonary affections occur after abdominal operations under local, occasionally spinal, and chloroform anæsthesia. There is, then, some factor at work connected with that part of the body which is the seat of operation. That factor is, I believe, collapse of the lung, massive or partial.

The work of Pasteur¹ has thrown a flood of light on the ætiology of surgical pneumonia. Briscoe, Elliot and Dingley, Whipple, Elwyn and others, working on the same lines have produced a body of evidence which justifies one in believing that pneumonia is, in the great majority of cases, preceded by collapse, rarely massive, usually partial, of one or both lobes of the lung, which becomes inflamed as the result of pre-operative or anæsthetic bronchitis. Both Whipple and Elwyn lay stress upon the rarity of true pneumonia and of embolic pneumonia. The latter is clearly marked off from the atypical surgical form, which is in reality a broncho-pneumonia, by the history of onset and the absence of rusty sputum. The sputum is always muco-purulent in the "surgical" form. This condition of collapse, which is much

¹ *Lancet*, 1908 ii, p. 1334.

more common than is supposed and is found frequently when looked for, can be demonstrated by X-ray examination. It occurs comparatively rarely after pelvic operation. Elwyn states that he has known it disappear after a fit of coughing. What is the mechanism of its production, whether it be a reflex inhibition of the diaphragm, sudden blockage of bronchioles by secretion, sudden reflex spasm of bronchioles via the vagus, or surgical paralysis of abdominal muscles, is not clear. Recent observations seem to point to a nervous origin, particularly as it may follow a local or spinal anæsthetic.

The value of local anæsthesia in preventing these complications has been challenged. I have not seen any convincing evidence that the case against it has been proved. Judging also by the increasing use of this method in America and in this country, whether alone or in combination with light general anæsthesia, it would appear that the verdict of those who have special knowledge of it either from personal experience or from visits to Continental clinics is favourable. Hillman and Apperly,¹ in what I hope is a preliminary communication, speak well of splanchnic analgesia, especially for subjects who are unsuitable for general anæsthesia. On the other hand it is obvious that the prolonged exposure which is inevitable with some of the methods of injection may subject a patient of this type to a risk which is as great as that incurred in the administration of a general anæsthetic; and it seems clear also that if *this* risk has been reduced to a minimum, pain and enfeebled vitality may prevent a patient from making the necessary efforts to help himself in the important twenty-four hours following a severe operation. If the lung-collapse theory is correct, pulmonary complications are inevitable in some patients whatever method be adopted, but are less likely to occur in the absence of irritation due to the anæsthetic.

The results of my experience of spinal analgesia for operations on the bladder at St. Peter's Hospital and in private practice during the last six years show that out of 258 such operations, 197 were on the prostate, in 174 of which the Thomson-Walker operation was performed. Two patients died from pulmonary affections, one aged 77 years, on the fifteenth day, from renal insufficiency and bronchitis, and one, also aged 77 years, a chronic bronchitic, on the fifth day from broncho-pneumonia. Owing to excitability this patient was given C.E. to a moderate depth throughout the operation. He made good progress in spite of the lung complication until the sudden death of the patient in the next bed from pulmonary embolism alarmed him so much that he died shortly afterwards from fright. Out of thirty-three operations for bladder growths (including diverticula) one patient, aged 61, who was very stout and alcoholic, died from broncho-pneumonia; the duration of operation was one hour thirty-seven minutes, C.E. being given for the last twenty minutes. Out of 185 operations on the bladder under C.E., 69 being on the prostate, three patients died from broncho-pneumonia; in two of these three kyphosis made spinal puncture impossible.

Predisposing causes to pulmonary affections are, in addition to debility, sepsis and cachexia, lowering of blood-pressure caused by shock or the action of the anæsthetic. It is by its depressing effect that chloroform produces pneumonia in bronchitic subjects for it leads to pulmonary stasis and thence to infection. Cyanosis and excessive secretion of mucus must of course be avoided. After anæsthesia complicated by hypersecretion, an injection of atropine is advisable and coughing must be encouraged. Atropine is also useful in the after-treatment of chronic bronchitics. Limitation of movement by tight-bandaging is highly prejudicial. Expansion of the bases of the lungs either by coughing or breathing exercises is to be recommended. Rutherford Morison advocated, in 1911, vigorous coughing to clear the lungs on the day following operation, the patient's abdomen being supported by strong hands and arms. Now, it is an interesting fact that by the modern practice

¹ *Proceedings*, 1924-25, xviii (Sect. Anæsth.), p. 9.

of immediate drugging after operation by rectal injection of bromide and aspirin, the circulation and respiration are depressed, return of the cough reflex is delayed, and elimination of the anæsthetic is retarded. It is difficult to reconcile this practice with the adoption of the sitting posture some hours later, and of deep-breathing exercises to prevent pulmonary stasis. I believe that it is wiser, also, to postpone the injection of morphine until such time as pain and restlessness compel its use. Pain has its value by promoting activity.

Aspiration pneumonia is rarely seen in modern practice owing to improved surgical and anæsthetic technique. The vomiting of intestinal obstruction can best be controlled, failing the use of a local or spinal anæsthetic or insufflation anæsthesia, by the excellent plan of passing a stomach tube after induction and leaving it *in situ*. Syphoning of regurgitated stomach contents goes on continuously.

Ether is better avoided in patients with chronic pulmonary tuberculosis. I have heard of several cases which have been awakened into activity by its use.

Finally, I believe that conclusions drawn from hospital statistics as to the influence of the various anæsthetics are vitiated: (1) by the absence of details as to the pre-operative condition; (2) by the omission of the reason for choice of anæsthetic, or for any change of anæsthetic during the operation; (3) by the absence of statements as to the behaviour of the patient during the anæsthetic, or as to the nature of his response to surgical stimuli; (4) by the extent of the surgical trauma, and by the after-progress. Masses of figures taken from post-mortem records are apt to be misleading.

MR. J. P. HEDLEY.

Gynæcological operations have the unenviable reputation of being more commonly followed by fatal pulmonary embolism than any others, but the records of St. Thomas's Hospital do not show any greater frequency in this respect in the Gynæcological Department than in the Surgical.

In the figures published by Wilson from the Mayo Clinic there were ten fatal cases in 7,993 gynæcological operations or about 0.12 per cent., while there were twenty-three in 12,229 operations on the gall-bladder, liver, prostate, colon and rectum, and for hernia—a percentage of 0.18.

The prevention of pulmonary complications after operation is far more important than their treatment, but the great difficulty in the prevention arises from the fact that their causes are not definitely known. There are some who think that nearly all the cases are the result of embolism, whereas others think that many are due to the disturbances arising from the anæsthetic.

In the type of pulmonary disturbance that begins with signs of bronchitis within twelve to thirty-six hours of operation, some, if not most, seem to be definitely due to the anæsthetic.

When a patient has a common cold the dangers of causing lung disturbances by giving an anæsthetic are well known, so I can see no difficulty whatever in assuming that many post-operative pulmonary complications are due to the anæsthetic stirring up a mild or latent infection of the lungs.

Old people, and those with emphysema, are particularly liable to lung complications after operations.

There are undoubtedly many arguments in favour of the embolic theory of the causation of pulmonary complications, notably, that operations on certain regions of the body are much more likely to be followed by these disturbances than others, and that they have frequently followed operations under local anæsthetics; but, on the other hand, on many occasions an examination under an anæsthetic has been followed by pulmonary disturbance, and the freedom from these dangers after accidental wounds and fractures is very striking.

On the assumption that the bronchitis in a fairly large proportion of these cases is due to the disturbance of the anæsthetic with such contributing causes as

emphysema, mild or latent lung infections, general feebleness of the patient, chill, or the inhalation of vomit, certain reasonable precautions can be taken to prevent these complications.

When the lungs are damaged, where possible, operation should be avoided or performed under local or spinal anaesthesia, and deferred if the patient has a cough or cold.

The temperature of the operating theatre should be about 70° F., and after an operation a patient should be protected from cold and draughts. At St. Thomas's Hospital the introduction of a tent over the trolley on which the patient is returned to the ward from the theatre, and stricter rules about the use of bed curtains after operations, have reduced the number of these "bronchitis" cases. After operation care should be taken to prevent the inhalation of vomit, mucus or blood while the patient is still semi-conscious.

In the cases in which pulmonary complications develop later there seems to be no other explanation for them than that of embolism or thrombosis. The emboli reach the lungs from the seat of operation, or from a thrombus in some vein, or thrombosis of one of the pulmonary arteries. There is evidence that particles of tissue detached by the operation may be carried to the lungs and are held up there by the lung capillaries.

Thrombosis in veins near the seat of operation may be the source of emboli, and where sepsis is present and is mild in degree, the likelihood of emboli being set free seems to be increased. The type of case in which the patient has an attack of pain over a small area in the chest, followed by signs of pleurisy near the painful area, is fairly common. Such attacks may be followed by similar ones, but in my experience are not likely to end fatally.

In these cases there is usually some evidence of a mild infection of the wound, such as a moderate disturbance of temperature for a few days after operation, or a vaginal discharge in cases of total hysterectomy.

On the assumption that cases of this type are caused by small emboli becoming detached from infected clot in veins near the operation area, the obvious method of preventing this is by avoiding any damage or bruising of tissues round the seat of operation which may lead to thrombosis, especially damage by clamping large pieces of tissue or transfixing veins, and, most important of all, by avoiding infection. The treatment, when an attack has occurred, is by means of local applications to the chest wall and keeping the patient in bed and at rest.

In the cases in which a large pulmonary embolism suddenly occurs, there is usually very little to attract attention beforehand, though there is, generally, a mild disturbance of temperature suggesting some infection of the deeper parts of the wound. These cases are definitely due to a big branch of the pulmonary artery being plugged by a large clot which may have come from veins in the operation area, or from some deep vein remote from it; or it may be one that has formed in the pulmonary artery itself.

A patient whose circulation has been depleted by prolonged hæmorrhage before operation, or by excessive bleeding at the operation, is rendered more liable to thrombosis.

Starvation before operation and excessive purgation may both add to the risks of thrombosis in any part of the body, and roughness in handling the tissues during operation increases the risks of its occurring locally. When patients are lethargic, and lie very still after operations, there is an increased fear of this complication.

To prevent thrombosis that may lead to massive pulmonary embolism, where possible, operations for conditions such as uterine fibroids that produce anæmia should be undertaken before the patient has become profoundly anæmic, and loss of blood during operations should be properly controlled. Bruising and tearing of tissues should be carefully avoided by exercising the greatest possible gentleness in retracting, swabbing, and pulling on tissues.

The old method of giving drastic purgatives before operations, and starving the patient, is useless, and increases the risk of thrombosis. It should be given up, and patients should be encouraged to move their limbs and make some effort for themselves; they should be propped up in bed within a day or two after the operation.

I agree that the marked dyspnœa and shock produced by a pulmonary embolus that blocks a large branch of the pulmonary artery is due to the sudden embarrassment of the right side of the heart, and if the strain on it can be relieved and the patient tidied over the first few hours, there is a good chance of recovery.

Blood-letting and the inhalation of amyl nitrite are the best methods of treating the emergency, but morphia is also valuable in relieving the patient from the anxiety caused by the attack.

In the routine management of operation cases the patient should be encouraged to move about in bed in order to reduce the risks of thrombosis occurring, but when there is reason to believe that a clot has already formed the dangers of pulmonary complications developing are reduced by keeping the patient absolutely at rest.

LORD DAWSON OF PENN

said he considered that thrombi formed within the auricles of degenerated hearts which had been enfeebled by operation or other cause; but he agreed that the majority of emboli in the lung originated in the systemic veins, the femoral and iliac being favourite vessels, mostly those on the left side. The accidents under discussion were those prone to occur in the second half of life, when the circulation was sluggish and the vessel walls deteriorated. Though trauma was so very common during the war, embolism of the lung was relatively infrequent, the subjects of these traumata being young and vigorous. Before thrombosis could occur he thought some exciting agent in addition to sepsis must be operative. People with low blood-pressure were specially apt to develop thrombosis. He (the speaker) thought the pneumonia in these cases was nearly always of the lobular type, and instances of this following operation were so few that they could not be regarded as anything more than accidents. The milder forms were probably unrecognized. Pneumonia might be the first indication that embolism had occurred. It was not always that the sputum was blood-stained following infarction.

With regard to infection, there should be a careful routine examination of the upper air-passages before these abdominal operations. The influence of a common cold was well known. As regards massive collapse there might be first immobilization of the diaphragm and interference with movement in the lower intercostal spaces, and this would lead to stagnation of the lung and impairment of the circulation of both air and blood, resulting sometimes in a blocking of the afferent bronchi, and so also of collapse. There was usually recovery from massive collapse, the symptoms disappearing long before the signs. It was a manifestation of exhaustion. Broncho-pneumonia could be much reduced by avoiding operation in non-urgent cases at times when influenza and lung troubles were rife, and by getting the upper air-passages clean before the abdominal operation was undertaken.

Sir CHARLES GORDON-WATSON

said that a surgeon embarking upon this discussion had to steer carefully between the Scylla of Medicine on the one hand and the Charybdis of Anæsthesia on the other. He did not intend to bump into the anæsthetist, nor to be drawn into the whirlpool of medicine.

Lord Dawson had referred to pneumonias after operation, and he (the speaker) would refer to the same type, pneumonias which commenced on the day after operation, or on the second or third day. Those cases often came like a bolt from the blue after a severe operation on the upper abdomen. Dr. Featherstone, who had

written a most illuminating monograph on the subject, produced some very important figures. That gentleman took 222 consecutive severe operations on the upper abdomen at Birmingham General Hospital, and found that 10·8 per cent. of the cases developed definite pneumonia, and most often on the right side. He also gave figures with regard to operations on other parts of the body. For example, he went to an orthopædic hospital, and found that after 11,000 anæsthesias there was only one case of pneumonia.

Elwyn had produced statistics of 1,080 operations, all laparotomies; in those the percentage of pneumonia cases was 6·2. In 1,852 cases, excluding laparotomies, the percentage was only 0·7. These figures showed the tendency for this complication to occur after laparotomy, especially when the upper abdomen was attacked.

Featherstone found that in only 2 per cent. of hysterectomies the patients had suffered from this complication.

These considerations brought one back to Dr. Pasteur's work,¹ whose results were confirmed by war experience. Many of the Fellows here had had opportunities of seeing collapse, paresis, or relaxation of the diaphragm in association with severe abdominal wounds, and especially wounds which involved both abdomen and thorax. The explanation given by Dr. Pasteur was, that in acute abdominal conditions, any pain in the abdomen which was severe enough to cause reflex rigidity of the abdominal wall, might produce a corresponding relaxation of the diaphragm. The diaphragm was antagonistic to the muscles of the anterior abdominal wall, and when the latter were rigidly contracted, the diaphragm was relaxed.

Collapse of the base of the lung, especially on the right side, was extremely common in abdominal wounds during the war, and, once collapse occurred, then, as Lord Dawson said, there followed congestion, cedema, blocking of bronchioles and stasis; if oral sepsis was present, infection was then more liable to occur, leading to lobular pneumonia. That was the type most commonly seen. He was not referring to the septicæmic pneumonias which occurred after post-operative sepsis.

With regard to the prevention of pneumonia, if severe operations on the upper abdomen gave 10·8 per cent. of pneumonia, it was important to consider the subject of prevention. There must be many other accessory factors, one of which was the irritation of the anæsthetic, and another, chill at and after operation. Dr. Featherstone exonerated the anæsthetist—he (the speaker) believed Dr. Featherstone was himself an anæsthetist—and he also showed that ether was not the serious agent it was generally supposed to be, as he found that, taking autopsies after operations, although ether had been given more often than other anæsthetics, in a small majority of those which developed pneumonia ether was not the anæsthetic employed.

Concerning gas and oxygen, it was of interest to watch the anæsthetic development in the war. At the beginning of the war, he (the speaker) was at the Base, and the severely wounded were sent there, as the casualty clearing stations had not then been adequately prepared for operations. The cases arrived at the Base severely wounded, septic and very exhausted, and in that condition they stood operations very badly. It was found that the best way to keep them alive after operation was to use warm intravenous ether and saline. This method was not suitable for the casualty clearing stations later on, and Dr. Geoffrey Marshall, realizing that matters were going badly, i.e., with open ether, adopted the excellent device mentioned by Dr. Shipway, namely, a warm ether apparatus. Then the percentage of pulmonary complications went down, from 54 per cent. in his cases to 14 per cent. Proceeding further, Marshall got on to the use of gas and oxygen for the severely wounded, and his statistics still further improved. This anæsthetic became almost universal wherever it was obtainable for rapid amputations. Since the war advances had been made with gas and oxygen anæsthesia. Boyle and others had designed an apparatus which allowed gas, oxygen and ether to be easily administered together, and the intratracheal

¹ *Lancet*, 1908, ii, pp. 1351-55.

method had been developed. He (the speaker) now used the intratracheal method for all his work. He believed it was the best method, not only for the avoidance of shock, but also for the prevention of pulmonary complications. It also prevented infection of the lungs by aspiration, a condition especially liable to occur in cases of pulmonary collapse.

With regard to pulmonary embolism, it was somewhat remarkable that with the improvement in surgical technique, and with greater facilities for preventing shock, this bogey of embolism should still persist. At St. Bartholomew's Hospital for the three years before the war, the percentage of surgical deaths in the post-mortem records from pulmonary embolism was 0.5; and for the three years after the war the percentage was 1.7; while for the following two years the figure was 2.9. If that meant anything, it meant that pulmonary embolism was on the increase. In those years the total number of surgical operations was over 29,000, and the deaths from pulmonary embolism, on the surgical side, were 22.

Lord Dawson had referred to the cases of pulmonary infarction. A few years ago he (the speaker) had studied a large number of charts of cases of post-operative pulmonary infarction, and he did not find one where infarction was recorded in which there was not some rise of temperature, and in many there were recurrent attacks of pulmonary infarction with recurrent rises of temperature. Small fragments were liable to become detached if a thrombus broke down, but this did not occur unless the fixed thrombus in the vein had become to some extent infected. He drew a clear distinction between that type of case and that of massive embolism. He had examined a number of these cases, and had been present at the great majority of the post-mortems on fatal cases of pulmonary embolism which occurred at St. Bartholomew's Hospital for many years. In cases in which the origin of the thrombus was ascertained, the wall of the vein had been found to be smooth, and on several occasions he had replaced the curled-up clot from the pulmonary artery in the vein and had made it fit. Those massive clots were seldom infected and the veins were not inflamed; there was no phlebitis in the accepted meaning of that word.

As to causation, he placed trauma first, then stasis, and sepsis last. Some other factor must come into play to produce the complication; it might be the absorption of thrombo-kinase, as Mr. Lockhart-Mummery suggested. It was clear that trauma to a small branch of vein, whether twisted, torn or ligatured, might originate thrombosis, which could easily spread to its parent vein; he had shown that such was the case.

Why did these complications usually occur after abdominal operations? Sometimes they occurred when the wound was in the appendix region, and in many of those instances the deep epigastric vein was ligatured or damaged by retractors, or by some manual violence to the wound. Thrombosis commencing in the epigastric could spread to the external iliac. He was not sure that in some cases after, e.g., gall-bladder operations, the spread of thrombosis did not occur through the superior epigastric vein. There was a fatal case of his after a gall-bladder operation in which the thrombus came from the superior vena cava, and not from the inferior vena cava.

In regard to the sluggish circulation, the first factor was the loss of fluid before, during, and after operation, due to purging, vomiting, bleeding, and the withholding of adequate fluid after operation. The next factor was the lowering of the blood-pressure due to the shock of a prolonged operation, and the minimum of movement after operation, aided by morphia, and diminished respiratory effort. A further factor was concerned with the posture of the patient during the operation. Was not the constrained position of the patient during gall-bladder operations a factor in the production of obstruction to the inferior vena cava? In the Trendelenburg posture the whole abdominal contents pressed against the ventricles and auricles, which must obstruct the inferior vena cava, thus aiding stasis.

With regard to the treatment of acute cases, he had, on one occasion, an unfortunate experience, namely, that of watching a case of massive pulmonary embolism from its onset to its termination two hours later. When he first saw the man he was in a desperate condition, breathing with the utmost difficulty, his face an ashen colour, and his pulse very rapid, but his condition improved greatly under the influence of morphia and oxygen, though he was unable to survive the ordeal, as his main pulmonary artery had become largely occluded. Amyl nitrite had been used most effectively to relieve the immediate symptoms.

Another question was, should bleeding be employed on these occasions? On this point he asked for information. Mr. Hedley had suggested blood-letting to relieve the urgent symptoms. Shock, however, was an important factor in these cases, and one of the causes of shock was stagnation of blood in the lung. When the pulmonary artery was blocked, owing to the natural negative pressure in the lung capillaries, backward suction of blood from the other side of the heart occurred, i.e., blood flowed back from the left auricle, and the lungs became stagnant with blood. In that way much blood was withdrawn from the general circulation, and it was doubtful whether further withdrawal by venesection could be borne in these desperate cases.

Dr. FEATHERSTONE (Birmingham)

said he was an anaesthetist, and as such he was anxious to ascertain the true position of ether in the causation of these post-operative troubles which were under discussion. In view of the extreme frequency with which post-operative pneumonia followed operations on the upper abdomen, and its infrequent occurrence following even severe operations on the head and neck, it must be felt that ether, which was used in great quantities in these head and neck operations, should, if it were an important factor, cause pneumonia in many of the cases. But it did not. As Dr. Shipway pointed out, intratracheal ether was particularly safe as an anaesthetic; Dr. Shipway had given it in an enormous number of cases without pneumonia occurring.

He felt that in ascertaining the cause of post-operative pneumonia the patient's previous history should be carefully investigated. The fact that such a complication occurred in the presence of epidemics of colds, or even of influenza, and its comparative infrequency in the summer months, pointed the same way. He (Dr. Featherstone) went through post-mortem statistics before the war relating to this question of post-operative pneumonia, on one occasion when there was an influenza epidemic, and again when influenza was less prevalent. He found that the post-operative incidence varied in proportion with the frequency of influenza among the main population.

He therefore thought that severe operations on the upper abdomen should, as far as possible, only be performed during periods of general health among the population, and not when influenza was rife.

Last week, at the Birmingham General Hospital, there were two cases of the type under discussion. One was that of a boy who had an appendix operation, the other that of a man who had gastro-enterostomy. In both cases, on the third day, cyanosis occurred. There was complaint of pain in the chest, there was cough without expectoration, and the outward movement of the lower costal margin seemed more pronounced on the affected side. Both cleared up on the third or fourth day after the onset of those symptoms, and the improvement was not followed by marked increase in expectoration as after an attack of ordinary pneumonia. He (Dr. Featherstone) suggested that they were instances of massive collapse of lung, or, as it had been called by Whipple, "pneumonitis."

Dr. Pasteur said the chain of events was a mechanical one. But he (the speaker) thought it would always be found there was a rise of temperature, some expectoration,

and other symptoms of inflammatory trouble. Massive collapse of lung seemed difficult to explain on purely mechanical grounds when there was a paralysed diaphragm on one side—that the lung should then collapse because the diaphragm was not working, and should pull the heart across to the side where the diaphragm was not strong enough to pull against the healthy side. He had put this point to Sir Clifford Allbutt, who agreed that something more should be looked for than the mere weakening of the diaphragm. Perhaps the closing up of the lung was due to blocking of the bronchioles caused by congestion and some inflammatory cedema in the motionless lobe.

He (Dr. Featherstone) therefore emphasized the point that, as far as possible, in order to avoid post-operative pneumonia, operations on the upper abdomen should be done in periods free from epidemics of such diseases as influenza.

DR. ROBERT HUTCHISON

remarked that he had not much to contribute from the standpoint of the physician. The physician saw these complications in two forms—embolism and inflammatory complications. As had been rightly stated, progress would be made if the cause of embolism were known. One factor had been referred to as a possible cause of thrombosis, namely, an increased coagulability of the blood. Mr. Lindsay put forward the ingenious suggestion that possibly the total coagulability of the blood of the community had gone up in recent years on account of a possible dietetic factor due to war conditions. He (the speaker) did not think there was much in that suggestion. There was, however, one dietetic factor which might have a bearing on the problem, and that was the degree to which milk entered into the diet immediately before and after operation. In the old days a case of typhoid fever was treated exclusively by a milk diet, and more cases of thrombosis of the veins of the leg were seen then than now, and it was said that this was due to the increased amount of lime in the diet. That was a possibility, but he did not think the coagulability of the blood had much to do with the occurrence of the complication under discussion. He thought the post-operative thrombosis which resulted in embolism was largely the result of postural conditions. That had not been sufficiently emphasized. It was agreed that this accident was particularly frequent after abdominal operations, and after such operations the patient was usually placed in the position of a double inclined plane; he was propped up and he had a pillow under his knees. The result was that the patient got a double kink, and this must greatly interfere with the return of venous blood. The abdominal operation still further favoured that, because, for the time being, it practically paralysed abdominal respiration, which acted as a vascular pump to promote the return of venous blood. One saw the same thing if a patient had been lying for a long time on the back and was then turned on the side. It was when this turning on the side took place that thrombosis was apt to occur. Sir Clifford Allbutt said that he prevented that occurrence by instructing the nurse to put a soft pillow between the legs when the patient was turned over, as the trouble was due to one leg pressing on the other and so causing venous obstruction.

By attention to such small details much could be done to prevent thrombosis and the results arising from it; there was no need to look for any recondite factors, as he considered it was largely a mechanical matter.

With regard to the inflammatory complications, such as pneumonia and bronchitis, several speakers had rightly emphasized the important part played by reflex paralysis of the diaphragm in causing retention of secretion in the lower parts of the lung, and in that way giving organisms their chance, because organisms did, of course, play a part in the process. But, here again, he wished to draw attention to a simple factor, reference to which had been made only briefly by Mr. Hedley, namely, chill. The importance of this was apt to be overlooked. The question of microbes had come

so much into the foreground that the significance of chill was likely to be forgotten. A patient who had undergone an abdominal operation would often be found propped up, with all the bedclothes below the chest, which was covered only by a thin pyjama jacket, and generally there was an open window close by. In this way chilling occurred and inflammatory complications were set up. Every patient after an abdominal operation should wear a bed-jacket both day and night, and the craze for "fresh air" should be allowed to go by the board for the time being, the prime consideration being to keep the patient warm.

It was probably by attention to these details—really nursing details—that more would be done to prevent these post-operative complications than by any other means.

Dr. W. J. McCARDIE (Birmingham)

remarked that much of what he had intended to say had been dealt with by Dr. Shipway; and his colleague, Dr. Featherstone, had put the matter admirably from the anæsthetist's standpoint in his monograph.

These cases of lung troubles following operation nearly always occurred after abdominal operations, especially operations on the upper abdomen, and the anæsthetic used for these was the same as for other parts of the body. That showed that the anæsthetic itself was not responsible for these lung troubles. In the neighbourhood from which he came the surgeons often asserted that etherization was the cause of these troubles.

The comparative infrequency of these effects in the case of private patients was very noticeable; his own figures showed them to be only half as frequent as in hospital cases. A large part of this difference was due to the fact that hospital patients often did not undergo that prolonged preliminary preparation which was allotted to private patients, hence the latter were in a better condition at operation. Afterwards they were returned to a nice, warm room, with the windows closed and a good fire, and a special nurse was in attendance. Some of these important factors were absent in the case of hospital patients, the ward temperature being about 63° F., i.e., the temperature of a living room.

In hospitals there should be recovery wards for those who had undergone these big abdominal operations. Such wards existed in one hospital in Vienna, and patients in them received special attention for several days.

In the city where he worked, the surgeons suggested little in the way of prevention, except that chloroform should be the anæsthetic; but it had been shown that these complications occurred as frequently after chloroform as after ether. He considered that the responsibility for them rested with the surgeon, not with the anæsthetist.

Dr. F. LONGHURST

said that at a recent meeting of the Section of Anæsthetics the fact came out that there were very few statistics on the question of the effects of different forms of anæsthetics upon the lungs. He wondered why that was so, but he had found out since, as he had been over the records of 700 consecutive cases at St. George's Hospital with sufficient care to obviate the point raised by Dr. Shipway, that the value of statistics was small owing to insufficient detail, and had found that though one could arrive at an approximate value of the many factors involved, yet it was another question when one came to publish the findings in black and white as statistics. He had watched cases from the moment of operation until the patient was either discharged or else died. Whenever cough arose in a case, he made a note of it, and particularly followed up the case.

This discussion seemed largely to be dealing with pulmonary embolism, and, speaking as an anæsthetist, he said he did not know how to stop that condition. In ordinary broncho-pneumonias, probably the anæsthetic was to some degree

responsible, but when the surgeon and the nurse had done with the patient there was very little for the anæsthetist to account for.

He agreed that the after-care of patients, especially in hospitals, was disgraceful. A patient was sent back to a ward containing twenty-six patients, for whose needs the windows must be kept open, and that did not accord with his idea of what should be done for a patient recovering from an anæsthetic. He had just come from an operating theatre the temperature of which was 80° F., and the contrast between that and the temperature of the ward was much too great. He had himself been under anæsthetics, and his first sensation on recovery was that of a horrible shivering, which brought on vomiting.

Though statistics were admittedly unsatisfactory, he had notes of a certain number of cases, carefully made. In 700 consecutive cases given by several anæsthetists at St. George's Hospital there were subsequent lung complications in 22. His experience in these 700 cases did not support the idea that lung complications were due to the use of ether as an anæsthetic. In these 700 cases there were 3 cases of trouble arising after chloroform pure and simple out of 15; 1 after gas and oxygen out of 23 cases; 4 cases out of 193 of C.E. mixture; 3 out of 179 after closed ether, and 7 out of chloroform followed by ether. Thus one was still left in the dark. If there was one form of anæsthesia to be avoided, he thought it was open ether, because evaporation was so active that it must depress the vitality of the lungs. In his series there had been only 5 deaths, and in only 10 of the 22 cases of complication could the trouble possibly be attributed in any way to the anæsthetic.

Dr. G. H. BURFORD

said that a good deal had been said about the local action of anæsthetics on the respiratory mucous membrane, but the possible untoward effects lay deeper than that. In the last two or three years he (the speaker) had had three cases of pulmonary embolism or infarction, acute respiratory embarrassment following prolonged anæsthesia for abdominal troubles; and in two of the three cases there was discovered unexpectedly a marked glycosuria synchronizing with the acute respiratory symptoms, at first so acute as to threaten life. The glycosuria disappeared in about ten days, before the lung condition had entirely cleared up. Glycosuria was looked for in the second, on the basis of the first case, and it was found to be present in so marked a degree that an ounce of glucose was excreted in twenty-four hours. There was also present a considerable amount of diacetic acid and acetone, and these disappeared in due course. He suggested that these conditions should be looked for in cases, because the main complication, the effect of anæsthetics on the parenchymatous structure of liver and kidneys, was not yet fully understood.

Dr. Z. MENNELL

said that from 1903 to 1906 at St. Thomas's Hospital there were seventeen deaths from pulmonary embolism after operation. The present rate was, he felt sure, lower than that.

He remembered those figures because, in 1906, at the old Society of Anæsthetists, he read a paper and, in conjunction with Dr. Dudgeon, tried to ascertain whether there was any shortening of the coagulation time of the blood after operation. They thought there was, but there were so many outside factors to be taken into consideration that it meant the expenditure of a great deal of work and time to investigate it, so the research was not pursued.

He had watched two cases of pulmonary embolism: one was fatal in about fifteen minutes, the other went on continuously for about a week. In this last case the effect of morphia in keeping the patient going was remarkable.

He did not think the anæsthetist could be blamed in any way for the onset of pulmonary embolism, but, speaking as an anæsthetist, he believed some of the inflammatory effects after operations on the chest could be attributed to the anæsthetist. Ether had, in his view, a more irritating effect than chloroform, and a cold in the patient, or an infection of the nasal passages, predisposed to pneumonia. Often the surgeons were scarcely fair to the anæsthetists, for if it was said that it was known the patient had a cold, the surgeon's view was that it did not matter, as the temperature was not raised.

The use of atropine before operation was valuable, and he did not think large enough doses were usually given. At St. Thomas's Hospital, in 1909, there was an epidemic of post-operative pneumonia, and it synchronized with the time that one of the lifts was out of order and patients had to be carried along a long corridor. In this connexion he felt sure Dr. Hutchison's remarks on the effects of chill were true.

A further point concerned the treatment of pneumonia or bronchitis when it occurred. He felt very strongly that the routine mixture given at St. Thomas's, consisting of atropine, adrenalin and strychnine, aided the recovery of these patients very materially.

DR. C. F. HADFIELD

discussed a subject which he had mentioned on another occasion, namely, the irritation caused by ether. At St. Bartholomew's Hospital many minor operations were done on people who were there only a short time, and went out an hour or so after the operation. Most of these patients were anæsthetized with ether, excepting the gas patients. A house-surgeon, a year ago, drew his attention to the fact that pulmonary complications after these operations were practically unknown. If ether was the dangerous substance for the lungs it was supposed to be, a large proportion of these patients, after going home in whatever weather prevailed—perhaps fog—might be expected to return next day with bronchial trouble. His idea was, that if one could stop the excessive secretion in the lungs which took place in some people, ether would not be harmful; it did not injure the healthy lung. At the Anæsthetics Committee, which was working jointly with the Medical Research Council, there had been much talk of impurities in ether; there was no doubt that the peroxides and aldehydes which were formed did cause damage, and it was these substances which were formed in the layers of mucus in the air passages.

Some years ago he was the only anæsthetist at a certain hospital who was using open ether as a routine, and he had at times heard a surgeon say "You gave that patient horrible bronchitis." He had then asked which patient was referred to, and on consulting the records, had found that someone else had given the anæsthetic, and that it had been chloroform. But it was known that he was accustomed to give ether, and he was accordingly made the scapegoat.

SIR CHARLTON BRISCOE, Bt.,

said that he had taken considerable interest in this subject since the time that Dr. Pasteur had published his work. He thought that Dr. Pasteur was on the right lines, but it was unfortunate that he argued from the pathological to the normal. It surprised him (the speaker) that true pulmonary conditions occurred so seldom after anæsthetics. It was not realized what happened to the lungs of the normal individual who lay quietly in bed for a number of days, either supine or propped up at an angle of forty-five degrees. After an abdominal operation the individual exerted himself to the minimum on account of the pain, and therefore lay more still than another individual not so operated upon. The result of this inactivity of the abdominal muscles was that the lower part of the chest became and remained everted, a condition that was followed by deflation of the lower lobes of the lung. If anyone took the trouble to go round the wards when the backs of the patients were being

washed, and to examine the lower part of the chest, he would find in the cases of abdominal operations that at the end of thirty-six to forty-eight hours there was evidence of deflation of the lower lobes, diminished resonance, defective breath sounds, distant tubular breathing, &c.

During the period of the administration of the anæsthetic, on the other hand, the lungs were distended to the maximum, the scalenes holding the thorax elevated and the diaphragm descending to the full. There was thus ample opportunity for infective material to be inhaled from the mouth or upper air-passages, and especially into the lower lobes.

The common post-anæsthetic infective condition of the lungs generally occurred within two to four days following the anæsthetic, accompanied by a rise of temperature to 102° F. and the expectoration of some red or green mucus, which did not give the test for blood-pigment. This was probably infected mucus secreted by the bronchus, and not coughed up owing to the part of the lung supplied by the tube being deflated. Some of this infected material would be absorbed by the lymphatics and taken to the pleural surface, and could there originate a local attack of pleurisy.

He did not think the fact of the onset of the pain being sudden necessarily indicated an infarct, because any attack of pleurisy might originate with great suddenness.

He agreed with the remark of Dr. Hutchison that coughing was very painful after abdominal operations, and the condition was always worse when the incision had been made in the upper part of the abdomen. The reason for this was that the incision had generally injured the upper part of the transversalis originating from the seventh, eighth or ninth costal cartilages. This was one of the muscles which controlled those ribs during expiration. The condition could be assisted by lower thoracic constriction, the effect of which was to increase the movement of the diaphragm, to replace the action of some of the abdominal muscles, and to expand, to some extent, the lower lobes.

It was an obvious preventive measure to see that the mouth was as clean as possible preceding the operation, and to avoid operating when the air-passages were affected, unless under urgency.

Mr. J. E. ADAMS

said he had learned much from this discussion, and felt grateful to the speakers. He thought the great merit of this discussion had been the spirit of accusation, possibly excuse, on the part of speakers—physicians, surgeons, anæsthetists, and there was a class unrepresented. He was deeply interested in Dr. Hutchison's observation, but the physicians had not said enough about the cause of these conditions. It was obvious that the surgeons were responsible for a great deal. He (the speaker) confessed he was guilty of putting patients into a posture in which the venous circulation was impaired, and he would in the future use it with that fact in mind.

Nearly all the anæsthetists had concluded that they were free from blame: possibly they had to act too much as the servants of the surgeons, and had to give the best anæsthetic they could under the circumstances. But one very important suggestion had been made, namely, that operations should not be done during the winter months, and that would mean a delightful holiday for the surgeons. Unfortunately, patients were always with us, and presumably they must receive some surgical attention.

It was further suggested that there should be recovery wards in hospitals, but he did not think that was practicable at present. Still, much good would ensue if the plan adopted at St. Thomas's Hospital were made general, namely, that of having a curtain put over the end of the beds containing patients who had been recently

anæsthetized. He was inclined to adopt it for all patients on whom he operated during the winter months, especially when influenza was rife.

The class he referred to as not represented at this meeting comprised the nurses. Apparently, all here at this meeting were prepared to blame the nurse for some of the post-operative pulmonary complications, and he feared there was some justification for that attitude. It might sound ungallant, but he thought that some of the patients were rather over-nursed; certainly some of them were over-washed, a process which resulted in a marked lowering of temperature, especially in the chest region, and this should be carefully guarded against.

MR. E. C. LINDSAY

(in reply) agreed that there had never been a general discussion of this Society in which members of different Sections had shown so much accord.

Mr. Hedley, however, misunderstood him on the subject of gynaecological responsibility in the matter of emboli. He gave twenty-two as the number in 2,004 operations. Gynaecological operations formed only part of general abdominal operations, therefore the total responsibility of the gynaecologist was less than that of surgeons operating on other regions of the abdomen.

Some of the smaller emboli arose from the seat of operation, and they could be discounted, because in operations on organs connected with the portal system naturally small emboli would pass through the liver and it would be impossible for them to reach the general circulation.

Lord Dawson said that possibly an abscess would form in the area of infarction, but in the cases he (the speaker) had examined, in only one did an abscess form after preliminary infarction.

As to what Sir Charles Gordon-Watson said concerning exercises in breathing after operation, he (Mr. Lindsay) had recently met Professor Fullerton, Professor of Surgery in Queen's University, Belfast, and had mentioned the matter to him. Professor Fullerton replied that he would like him to tell Sir Charles that he had read his paper, and went home full of enthusiasm for the treatment. He did an operation for acute gall-stones and cholecystitis, and commenced movements in the first twenty-four hours; the patient died of pulmonary embolism on the fourteenth day.

He had suggested that certain cases of infarction occurred which were not recognized except by a slight rise of temperature and blood-stained sputum, and he was hoping someone would have noticed that point. The assumption was that it was only a small particle separated from a massive thrombus elsewhere. At what time should one consider it safe for the patient to get up? There was one case in which embolism occurred at the forty-first day, and in the figures he had investigated the twenty-ninth was the latest on which embolism occurred. If, therefore, there is a warning of infarction, twenty-eight days should pass before the patient should be allowed to get up.

Sections of Medicine, Surgery, and Pathology.

Chairman—Dr. ROBERT HUTCHISON

(President, Section of Medicine).

DISCUSSION ON THE TREATMENT OF SEPTICÆMIA.

Sir THOMAS HORDER, Bt. (Section of Medicine).

THIS subject was formerly, and still is, one in which I have taken a great deal of interest, and I accepted the Secretary's invitation, on that account, a little readily. But on thinking over the matter during the last few days I have come to the conclusion that I am ill-fitted to fill such an important rôle in the discussion. And for this reason, that I find my knowledge and treatment of a septicæmic case differs very little from that of ten years ago; so little, that I was rather aghast at the discovery.

But if I have nothing new to advance with regard to the treatment of septicæmia, at least I can ask some questions which have troubled me for a good number of years, and which trouble me still, hoping that the answers to these may be forthcoming from others.

It is, I think, desirable to limit and define our subject. Extended knowledge of blood-cultivation has informed us that micro-organisms may be recovered from the blood-stream in more diseases than we formerly thought possible; though here, again, I think the position has not changed, in any material respect, during the past ten years. We knew ten years ago that in pneumonia, in typhoid fever, in cerebro-spinal fever, and in some other diseases which we do not regard as forms of septicæmia, the blood-stream at an early stage of the disease process contained the specific micro-organism. But we did not call these conditions septicæmia, although we recognized a septicæmic type of the diseases concerned. Nor, if we come to chronic infective processes, do we include such conditions as gonorrhœal rheumatism, the various grades of septic arthritis, or coliform infections in the concept "septicæmia"; though we have evidence here, also, that the blood-stream, from time to time, is infected. Septicæmia, though often associated with a primary site of infection, is largely independent of such association. In some of the most characteristic cases of septicæmia the primary focus may be so inconspicuous as to be undetected throughout the disease. Indeed, this state of things is the crux of the situation which faces us to-night, and it is what I shall bear in mind in the remarks I have to offer on the treatment of septicæmia, a disease in which micro-organisms exist in the blood and multiply there, independently of the existence of a primary focus of infection.

I shall, naturally, omit all considerations of pathogenesis, except as they bear upon treatment; but the nature of the infecting agent naturally bears on the treatment very much. And, apart from certain much less common organisms, there are two especially which are concerned with the disease as I have defined it. One is the *Streptococcus pyogenes*, vel *hæmolyticus*, *longus*, *erysipelatosus*; and the second is *Staphylococcus aureus*. For the purposes of convenience, I have put down what I regard as three main principles underlying treatment of a case of septicæmia. There are *general or non-specific measures*, an ambiguous term, inasmuch as the word "specific" is sometimes used for *bacterio-therapeutic measures*, and sometimes it is applied to *forms of chemo-therapy* which, it is thought, might be

bactericidal in their effects. There is a certain amount of overlapping, but there are these three main lines of treatment. And, as I shall say presently, I think it very important that, as far as is possible, all three of these lines of treatment should be passed in review in regard to any individual case (Schema shown).

(1) I put the general measures of treatment first, if only for the reason that there will be very little difference of opinion as to the advisability of pursuing a very thorough plan of campaign in respect of those measures. Another reason for putting general measures first is, that promptness in the treatment of acute septicæmic cases being the essence of the contract, one naturally gets under weigh with some of these measures of a general kind, before the exact bacteriological diagnosis has been established. In regard to no disease is there more difference in the personal equation of the doctor than in the treatment of a case of acute septicæmia. That is not only because the practitioner is apt to become enamoured of a particular mode of treatment of a case of this kind; it is because there are so many possible lines to take, so many possible things to do, so many available methods of treatment, and it is necessary for him to choose between these various things. He has to form a plan of campaign and follow it, and he must not introduce, with an air of panic, too many new remedies at any particular juncture.

Supposing I take as a common type an acute *Streptococcus pyogenes* case, and that the avenue of infection is a post-mortem wound of the finger, which is followed rapidly by cellulitis, lymphangitis, and a general infection. Perhaps it will be most convenient if I take these three general measures *seriatim* and deal briefly with them. The patient is being rested, and there is rest for the limb which has been infected. The question of fresh air has become, of late, more and more attractive in the treatment of all septic processes. Until recently the tuberculosis patient had almost a monopoly of fresh-air treatment. After him, those with other respiratory affections are treated with more fresh air than formerly: pneumonia, for instance. Now, perhaps, we are keener than ever before on fresh air in the treatment of infections in general. Some patients are alarmed at the idea of being treated in the open air, and it sometimes requires a good deal of persuasion to convince them that there will not be a chance of complications arising, especially of the respiratory sort, if full open-air treatment is adopted. But if that difficulty can be overcome, I think you will agree there is no doubt that, providing it can be done without transferring the patient any distance, he should be treated on a balcony, or if the season permits, in the open air altogether. I will return to that when I speak of the chronic septicæmic processes. No doubt sunlight is good, and I have included that in order to ask if anyone has experience of artificial sunlight in the treatment of pyogenic infections. Diet I have very little to say upon; but in my own experience I can testify to the good effects of a full diet, fluid of course during the height of the acute process. Those who have suffered from this disease will remember how much better they felt immediately after being fed, however frequently the food was given. Though the state of the stomach and abdomen has to be watched, the septicæmic patient in general requires a good many calories of food-stuff.

I have also put down "reassurance," and I mean by the term more than the mere word implies; I mean the general conduct of the patient's mind and his attitude towards his illness. Some septicæmic patients are very pessimistic, others are not. In any case, the treatment of the patient's mind becomes more and more important as the case progresses; the treatment in a long case becomes irksome, and there is often much coaxing required to get things done without upsetting the patient's equanimity.

With regard to hydrotherapy, this is something on which individual doctors do not differ so much, perhaps, as individual nations. In America, hydrotherapeutic measures are still favoured by many practitioners, not only in typhoid fever and scarlet fever, but in septicæmia. In this country we do not put our very sick people

into baths as is done in the United States; perhaps we might do more in the way of hydrotherapy while the patients are in bed. Elimination again is important, namely, attention to bowels, skin and kidneys. Sleep, of course, is important.

Reference to anodynes recalls the experience of Sir James Paget, who, after convalescence from his acute septicæmia, paid a high compliment to the value of opium, as to the use of which in many septicæmic cases there is no contra-indication. I think that in these days we may be a little too sparing of opium in acute febrile conditions which are prolonged and lead to pain and mental, as well as physical, discomfort.

I now come to the treatment of the site of infection, about which, no doubt, Mr. Rowlands will have a good deal to say. Drainage I have put down as rather a generic expression to denote the obvious indication to deal with the primary source of infection as efficiently as possible. And, in the type of case I have chosen, conference with one's surgical colleague is generally indicated; and I have raised the question as to how much surgery should be done. What should be done to the hand, the finger or the arm, according to the stage which the infection has reached before the case comes under treatment? It is a moot question, and the answer is, I suppose, that careful investigation of the individual case dictates how much. My own feeling on this point is a little towards conservatism; meaning that as little as need be is done, consistent with the relief of what one still calls, for lack of a better term, "tissue tension."

The choice of baths or fomentations again introduces the personal equation of the practitioner. The continuous bath has one difficulty, in that it interferes with the carrying out of what is in my next sub-heading, elevation of the part affected. That elevation should be as near to the vertical as possible. It is not a bad system to alternate the almost vertical elevation with the continuous bath, an hour or two at a time. But fomentations, providing the nursing is efficient, get over that difficulty, because hot fomentation and elevation can go on at one and the same time.

With regard to heat and the local use of antiseptics, my feeling is one of antipathy to anything in the form of strong antiseptics, which interfere with the vitality of the tissues, and I think this feeling is fairly general. One sometimes sees strong antiseptic fomentations or antiseptics put into the bath, with, I think, no very good result.

Intravenous infusions are very useful in the fulminating type of case; the more acute the case, the more helpful.

Blood transfusions, in three really bad cases of streptococcal septicæmia, were not promising, but others may have been more fortunate.

Lastly, with regard to drugs. These are useful, but they are to be taken in conjunction with these other general measures, and must not be relied upon independently. I am not now speaking of drugs which are claimed to possess specific virtues. Of the drugs which I have in mind, I will say a little about one or two of them.

I put arsenic in the first place. There are very few cases of septicæmia which do not, in my judgment, get some benefit from arsenic, administered intramuscularly for preference. My favourite preparation of it is cacodylate of soda, and I give it, generally, in conjunction with nucleic acid,—gr. ij cacodylate of sodium in a saturated solution of nucleic acid, \times twice in twenty-four hours intramuscularly throughout almost the whole of the acute stage, two or three weeks or even longer. There are few or no toxic effects, and a convenient method of knowing when the patient is fairly well saturated is the smell of the breath.

Next, I put opium, for the general help one gets out of drugs in the management of acutely septic cases. Quinine I do not use. One sees it used in the treatment of acute septicæmia more often than any other drug, but I do not think its reputation

is borne out by experience. And there are such tonic drugs as strychnine and digitalis, and hypnotics other than opium.

(II) I turn now to the second line of attack, by specific measures. They are of three sorts: bacterio-therapy, immuno-transfusion, and "specific" measures of chemo-therapy. I have already said that my own treatment of a case of septicæmia is much the same as it was ten years ago. In acute streptococcal septicæmia my general rule, if there is nothing to lead me to depart from it, is to give a very liberal dose of anti-streptococcus serum as soon as the patient comes under treatment, administering it intravenously. By a liberal dose I mean not less than 50 c.c. on the first day, repeated on the second day, and possibly on the third day also. I choose the univalent form of the remedy, provided the case is of the type I refer to, the pyogenes case, due to *Streptococcus hæmolyticus*. Therefore, as it seems to me, if one is using serum at all it should be the univalent serum rather than a multivalent one. The latter type is apt to give the practitioner false confidence that, whatever the nature of the particular streptococcus in the case, there is more chance of its acting specifically.

About the second or third day one has, in such a case as this, generally succeeded in isolating the causative organism, and an appropriate vaccine can be prepared. The question arises, what vaccine? There are now many vaccines, and some of them are not, perhaps, vaccines at all. There is the ordinary vaccine, and there is the sensitized vaccine, Besredka's type, which, according to some schools, is not different from, but only more dilute than, ordinary vaccine. In my experience sensitized vaccine has advantages over ordinary vaccine in these acute cases. As to dosage, my first dose, in the adult, is 100 million, my second, on the third day, 250 million, and on the fifth or sixth day my third dose is 500 million. The doses of ordinary vaccines are much smaller, $\frac{1}{2}$ million to 1 or 2 million, according to the severity of the case.

Of detoxicated vaccines I have practically no experience. "Immunogens" I have put down for consideration, because I think they are an improvement on the old "phylacogen" type of remedy. This is a type of antigen which differs from vaccines in that an attempt has been made to get into solution or suspension, in addition to the endotoxin of the organism, certain products of auto-digestion of the cell-body. There may be some here who have tried "streptococcus immunogen" in acute septicæmia and, if so, it will be interesting to hear with what result.

I mention combined methods, because my own practice is to combine methods; beginning with a serum and following it with a vaccine.

Chemo-therapy, it seems to me, for pyogenic infection, is very disappointing. There were, during the war, many glowing accounts of the effects of a sort of grand lavage of the blood-stream in septicæmia. War experiences in this class of case, like many others, have to be taken in relation to circumstances: the type of injury, the help given by the surgeon with regard to eliminating the primary focus, &c. However, the intravenous use of all three of the main forms of germicidal substance that were then tried seems to me to be very disappointing in civilian practice. I have used them all, and have seen others use them, and I do not feel that either chlorine derivatives, flavine, or the collosol metals have done much good.

The second acute form of septicæmia with which one is faced not infrequently is due to *Staphylococcus aureus*. This is more pyæmic in its features, when it is not fulminating and leading to such lethal conditions as infection of the heart, or to pericarditis, or to meningitis. If the focal abscess chanced to be situated where the surgeon can deal with it, then of course (inasmuch as pyæmia gives one time to act and septicæmia does not, relatively speaking) rather better results are seen in this form of infection. But against the favouring element of time we have to set the fact that there is nothing equivalent to anti-streptococcus serum in fighting cases of acute or chronic staphylococcal septicæmia. Outside the lists of wholesale chemists,

staphylococcal sera of any potency do not exist. Therefore, so far as bacterio-therapeutic measures are concerned, one falls back upon vaccines which, however valuable they may be in more chronic and localized forms of staphylococcal infection, are not so helpful in the acute and chronic generalized infections by *Staphylococcus aureus*.

Turning to chronic septicæmic processes, I would say that the present position with regard to the treatment of that most tiresome, yet perhaps increasingly common type of streptococcal septicæmia, ulcerative endocarditis, seems to be as disappointing to-day as it was ten years ago. Lately, however, I have been struck by the fact that the more one pursues these general measures of a non-specific kind the more hopeful does one become about the treatment of ulcerative endocarditis.

I saw last week a patient who, in a policy of despair, I sent to the South of France last October. She had then undergone a great deal of vaccine therapy; she had had immuno-transfusion; she had been given cacodylate of soda intravenously, one grain, two grains, three grains daily, twelve doses of each, a method of treatment which occasionally checks the advance of the disease. There seemed to be little left to do, and she was clamouring for sun and air. After making the position clear to the husband, we sent her to Mentone, where she lay out on the terrace of a hotel all day and all night. She had during that time no specific treatment, either bacterial or chemical. When I saw her the other day she was better than in October, not only as regards her temperature, but in appearance, in symptoms and in her weight. And there was this rather interesting thing about her, that she has the largest spleen I have ever observed in ulcerative endocarditis; it occupied the whole of the left side of the abdomen, resting in the left iliac fossa, and the inner margin of it came across the mesial line. It was so big that I thought she had developed leucæmia in the meantime. The blood-picture, however, showed that this was not the case. It suggested the possibility that some defensive mechanism had taken place in which the spleen was largely concerned.

I feel that in future the line of attack which is most hopeful in chronic streptococcal septicæmia, as illustrated by ulcerative endocarditis, will be a very thorough exploitation of non-specific measures.

MR. R. P. ROWLANDS (Section of Surgery)

What can a surgeon do for blood-poisoning? He can do his best to prevent or cure it, and I propose to discuss the subject under these two headings.

PROPHYLAXIS.

Here even more than anywhere prevention is far better and easier than cure. The surgeon aims at preventing local and systemic infection:—

(1) By taking every possible precaution against infection during operations. Thanks to the epoch-making discoveries of Pasteur and Lister, infection of a "clean case" is very rare, and it should become rarer still.

(2) By prompt excision of contaminated wounds such as those inflicted in war or, in civil practice, by accidents. The lessons learnt in the Great War have robbed these awful wounds of much of their danger, if only they can be treated in time.

(3) By prompt removal or treatment of local sources of systemic infection, such as acute appendicitis, the treatment of which is still too often delayed, with the result that nearly 3,000 people die in England and Wales every year from appendicitis; nearly all these deaths are from the septicæmia of peritonitis. This can be prevented by removing the appendix at the earliest possible moment. An operation undertaken within twelve hours of the onset of the attack has a mortality of under $\frac{1}{2}$ per cent. I have never lost a case operated upon within twenty-four hours.

More than 2,800 persons die every year in England and Wales from the perforation of gastric and duodenal ulcers. Practically two-thirds of these deaths are from the septicæmia of peritonitis. What has been said with regard to early operation in

appendicitis applies equally to perforation of peptic ulcers, as Grey Turner pointed out in his recent address before the Hunterian Society. The mortality is under 2 per cent. when the operation is carried out within six hours of the perforation, whereas it is over 50 per cent. when the operation is delayed until after twenty-four hours.

The same is more or less true of abscesses in other parts of the body, which should be opened without delay before they cause septicæmia. Many a life is lost from septicæmia by delaying amputation until it is too late, especially in such cases as acute, infective osteomyelitis of the long bones. Similarly, surgical anthrax is at first a local disease,—a mere papule,—the prompt excision of which saves the patient from a terrible death from anthrax septicæmia.

On opening an abscess, boil or carbuncle it is important not to trespass on and infect the surrounding healthy tissues. I have known septicæmia follow the too deep incision of a small boil in the auditory meatus.

(4) The universal use of rubber gloves at operations and especially during post-mortem examinations has saved a great many deaths from septicæmia as well as many patients from infection.

TREATMENT.

When septicæmia is established it is very much in the interests of the patient for the physician, the bacteriologist, the hæmatologist and the surgeon to collaborate, and it should be their constant endeavour to find, remove or treat the primary or other source of infection. They should never give up hope of discovering a collection of pus somewhere in the body. The opening and draining of such an abscess often lead to success in apparently hopeless cases. Repeated examinations of the blood for leucocytosis should be made, also systematic and complete examinations of the body for hidden collections of pus. For instance, pus at the roots of the teeth is only revealed by radiographic examination. Similarly the tonsil, the appendix or the gall-bladder may be the source of infection.

Sir Thomas Horder, in an obscure case of acute septicæmia which he kindly saw with me, diagnosed suppuration in a thyroid cyst which was low in the neck and mostly intrathoracic in position. Removal of this cyst, which was full of streptococcal pus, cured the patient. In another apparently hopeless, but more chronic case, in which pus had been aspirated from the knee on many occasions, Sir Thomas Horder diagnosed a collection of pus behind the knee, the draining of which was followed by recovery.

Many similar instances come to my mind: pus found in hidden places such as deep in the buttock, in the loin, under the diaphragm, in the liver, in the pleura, deep in the calf muscles, in the pelvis or prostate.

Whole-blood transfusion is valuable in selected cases, as pointed out by Stetson (*American Journal of Medical Sciences*, 1924, clxviii, p. 534). It will probably have a greater future than immuno-transfusion, particularly because it is more immediately available, and time is obviously of great importance in acute septicæmia. It combats the anæmia and raises the immunity. It is unfortunate that no chemicals introduced into the blood have yet proved to be of any value.

Exposure to sunshine, feeding up and careful nursing are very valuable accessories in the treatment of septicæmia.

Splenectomy.—This operation has been successful to some extent at the Mayo Clinic in subacute cases in which the spleen has been enlarged. It has also been undertaken there in cases of ulcerative endocarditis, but without any instance of recovery resulting.

Dr. L. COLEBROOK (Section of Pathology)

said he proposed to use the term septicæmia in its strict sense, meaning generalized infection with microbes free in the blood. He wished first to speak about some observations which his colleagues and he had been making at St. Mary's Hospital

under the supervision of Sir Almroth Wright; these had some bearing on the pathology rather than on treatment. It would be agreed, however, that to have the right pathology was half-way to getting the right treatment. It was necessary to ask oneself in connexion with the cases of septicæmia, what was the essential lesion which made these cases so fatal? Put another way, the question was—where did the mechanism of defence break down? There was not time for him to give all the experimental facts bearing on the genesis and progress of septicæmia which had been obtained, and he thought it would be better to confine himself to one point, namely, that in most septicæmias the breakdown of defence seemed to be associated with a diminution of the functional activity of the leucocytes. It would probably be in vain to look in the text-books for any reference to that; it would be stated there that leucocytes were either in normal numbers or increased numbers, and there the story would end. But the important point seemed to be, as was shown by Sir Almroth Wright [1] two or three years ago, that though present in normal numbers the leucocytes were often to all intents and purposes out of action; their function might be reduced from one-half to one-tenth of the normal. He thought that this reduction was largely responsible for the microbes being able to establish themselves in the blood-stream. This diminished function of the leucocytes could be easily demonstrated in two ways.

The first method was by a phagocytic test, the plan of which was shown in fig. 1.

TEST OF THE PHAGOCYTIC EFFICIENCY OF A PATIENT'S LEUCOCYTES.

Patient's washed blood-corpuscles	Normal man's washed blood-corpuscles
<div style="text-align: center;"> + Normal serum + Staphylococci <hr/> Cocci ingested: 100 per 100 leucocytes </div>	<div style="text-align: center;"> + Normal serum + Staphylococci <hr/> Cocci ingested: 300 per 100 leucocytes </div>
$\text{PATIENT'S LEUCOCYTIC EFFICIENCY} = \frac{100}{300} = 0.3$	

FIG. 1.

It was only necessary to take the blood from a septicæmic patient and a normal man, separate the corpuscles and wash them, just as for working out the opsonic index. One then made the two phagocytic mixtures described in fig. 1 and estimated the amount of phagocytosis occurring on incubation in the usual manner.

In the mixture containing the patient's leucocytes it would almost invariably be found that far fewer cocci had been ingested.

The second method of demonstration was illustrated by fig. 2.

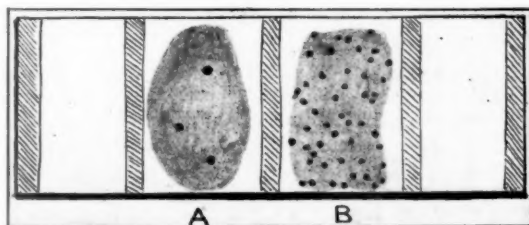


FIG. 2.—A slide-cell filled in with bloods derived from (A) a normal man, and (B) a septicæmic patient. Each blood was implanted with fifty-four staphylococci.

In the shallow "slide-cell" there depicted, a sample of the normal blood and of blood from a septicæmic patient had been incubated, after infection with the same dose of living staphylococci. Of the fifty-four cocci implanted it would be seen that only three had grown out into colonies in the normal blood, whereas there were something like fifty colonies in the patient's blood, showing that that blood had lost practically all its bactericidal value. The patient in question had died.

(In connexion with this experiment it ought to be explained that all the killing of staphylococci in such slide-cells was due to leucocytes—the serum had little or no direct bactericidal action on these microbes.)

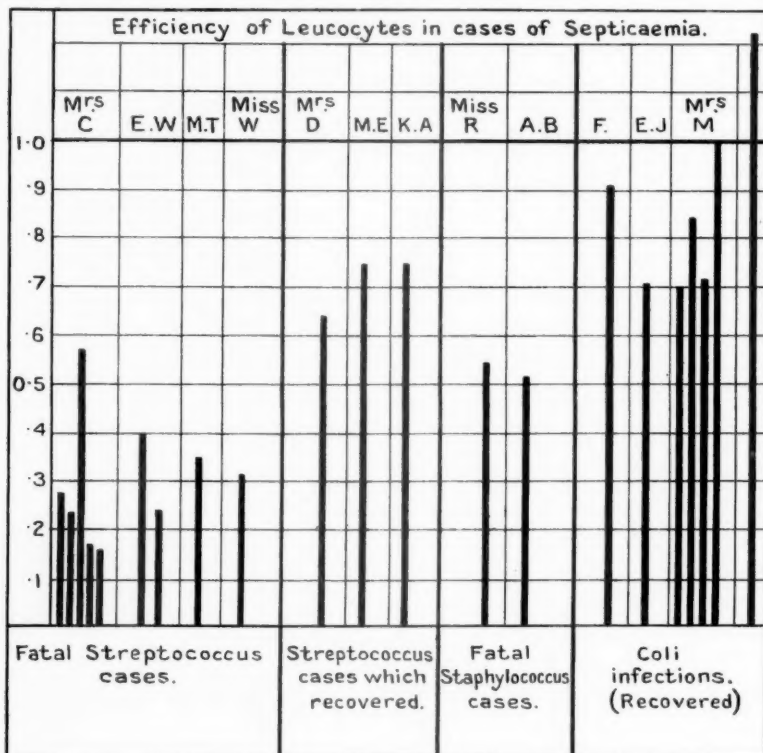


FIG. 3.

Summing up what had been learned about the reduction of leucocytic efficiency in disease, Dr. Colebrook made the following points: (1) It was almost certainly due to the action of microbial toxins produced in the local focus of infection, or in the bloodstream, or in both. A similar poisoning effect on leucocytes could be demonstrated by adding a filtered vaccine of sufficient strength to blood *in vitro*. (2) Although seen in its extreme form in septicæmia—especially in streptococcal septicæmia—it was not confined to that condition. Fig. 3 showed the leucocytic efficiency values obtained in a series of cases of septicæmia. It would serve to illustrate the importance of leucocytic efficiency for prognosis. (3) There was reason to think that efficient functioning of the leucocytes was not the only factor which restrained the

growth of microbes in the blood-stream in septicæmia. Even in the last stages of the disease one could seldom grow more than 1,000 or 2,000 colonies from one c.c. of the patient's blood, whereas the same blood, incubated in a test tube, would show a multiplication amounting to hundreds of thousands per c.c. The spleen was suspected of having something to do with this restraint.

He considered that these observations on the diminution of leucocytic function had a distinct bearing on treatment; they made it clear that what was necessary in order to save the patient was to neutralize the toxin which was destroying the leucocytes, or to do something which would directly kill off the microbes that were continually producing that toxin.

Turning to treatment proper, he intended to deal only with the three lines of specific treatment, namely, active immunization, passive immunization, and chemotherapy.

With regard to active immunization, he did not feel hopeful in cases of septicæmia. It must be remembered that when the leucocytes were much injured it was impossible to get any response from them by the addition of a vaccine. Treatment by vaccines was not likely, therefore, to be successful in grave cases, i.e., where the leucocytic function was reduced to below half the normal. The observations made by himself and his colleagues seemed to show that the body gave very little response in the way of elaboration of protective substances in septicæmic infection. In streptococcus and staphylococcus infections one could not demonstrate a rise in the bactericidal power of the serum as a rule, though when the patients were recovering, their leucocytes tended to function better than normal. With pneumococcal and *Bacillus coli* septicæmias it was not so uncommon to obtain a rise in protective substances in the blood. He believed most of these cases cured themselves in this way. Possibly by using a wisely adjusted dose of vaccine one might assist matters in those infections.

Speaking next of passive immunization, he explained that he meant the use of antisera and immuno-transfusion. When using antistreptococcus serum it was hoped that one was using an agent which would either kill microbes directly or would neutralize the toxins; but the evidence that any antistreptococcus serum did that when administered to human cases was somewhat slender. The tests which would give evidence of this action were so unsatisfactory that he understood many manufacturing houses had given up applying any tests. His colleague, Dr. Todd, had been doing some work with antistreptococcus serum, adding it to normal blood and testing its effect on the bactericidal power in slide-cells. The experiments appeared to show that the antisera (and also normal horse sera) would have some small effect if about a pint were introduced into the blood-stream of the patient, but that the doses used therapeutically would exert practically no antibacterial effect. In one experiment in which small additions were made to the blood of a septicæmic patient *in vitro* the only effect was to reduce the already low bactericidal power of the blood. He (the speaker) felt that, in view of the heavy price which patients had to pay in the way of serum sickness, &c., the use of antistreptococcus serum was not justified until there was better evidence that it was likely to achieve the desired result.

With regard to the programme of immuno-transfusion, probably the broad outlines of this were familiar to Fellows. It was introduced by Sir Almroth Wright [2], who suggested that there should be procured a non-specific immunization of the donor's blood before use. That was done by waiting from a few minutes to an hour after vaccinating the donor's blood outside the body, or by inoculating the donor before the blood was taken. The latter procedure had been usually employed by himself (the speaker). The donor was given a thousand millions of staphylococci subcutaneously; then one waited three or four hours and the blood was taken. Such blood could usually be shown to have acquired an increased bactericidal power, and its introduction into the patient usually determined a corresponding improvement in

his blood. He had treated about fifteen cases of septicæmia, mostly puerperal, in the last year or two, and in some measure he had been disappointed. The very grave cases, which he had at first hoped to save by this method, had not been saved, although temporary improvement had sometimes occurred. Quantitative blood cultures in these cases had, however, shown that there were often hundreds of streptococci per cubic centimetre of the patient's blood, and it could hardly be expected that the degree of immunization which we were able to effect at present in a donor's blood would be adequate to overcome such a heavy infection.

In some of the less severe cases very satisfactory results had been obtained, and he thought that the method should certainly be used for these and for border-line cases, i.e., those which were not strictly generalized infections. He hoped there would be discovered a more effective method of immunizing donors, and he did not think the end of that endeavour had been reached. Sir Almroth Wright had great hopes of being able to save these patients by a non-specific immunization of the blood, but he (the speaker) felt that more could be done if it was possible to secure a specific immunization of higher degree against the patient's microbes. That, however, was in the future. He thought immuno-transfusion a valuable method, though it should not be relied upon alone to save the gravest cases. Possibly in combination with chemo-therapy it would prove of great value, because if it did not itself furnish much in the way of protective substances, it yet gave the patient a supply of fresh leucocytes to carry on with.

With regard to chemo-therapy, that was a very difficult part of the subject. Unlike the two previous speakers, he felt that the future treatment of these cases lay along that road, though very little had yet been achieved. Before taking stock of the present drug treatment of septicæmia, he asked his hearers to consider what one ought to ask of a drug before consenting to use it as a chemo-therapeutic agent for septicæmia. Fig. 4 showed what he regarded as the most important credentials which should be required of any drug advocated for the treatment of septicæmia.

CREDENTIALS REQUIRED OF A CHEMO-THERAPEUTIC REMEDY FOR SEPTICÆMIA.

(1) That the dose administered does not still further diminish the activity of the patient's leucocytes (or destroy other essential tissues).

(2) That the patient's blood after such a dose can be shown to be bactericidal to the infecting microbe, and shall continue so for a sufficient period.

Or,

(3) That the patient's blood, after a dose, can be shown capable of neutralizing the microbic toxin which poisons the leucocytes.

FIG. 4.

With such a chart in front of one it was necessary to consider whether there was a drug at present in the field which fulfilled these requirements. Various drugs had been spoken of in the present discussion: eusol, chloride of mercury, quinine, flavine, and a German arsenic preparation, rivanol. It would be easy to show that none of those drugs fulfilled No. 2 requirement. Nobody had shown that after giving the patient these drugs there was the slightest increase of bactericidal power in the patient's serum. Neither had anyone shown that these drugs were capable of neutralizing microbic toxins.

With regard to the first requirement (fig. 4) Dr. Fleming's very instructive experiments would be recalled. He had shown that practically all the antiseptic substances in common use, when added to blood in sufficient concentration, spent their strength upon the leucocytes and red blood-cells, with the result that the microbes escaped, growing even better than in untreated blood owing to the injury done to the leucocytes.

The drugs recommended for the treatment of septicaemia, however, were—fortunately for the patient—usually given in doses much too small to have any appreciable effect upon his leucocytes. Much less would they affect the microbes.

On this showing the outlook seemed hopeless, but there was a group of drugs which made that outlook seem a little more hopeful to him (the speaker). This was the neosalvarsan group. On giving a drug of this group in the medicinal dose for curing syphilis, and taking the patient's blood after an hour or two, one found that the serum, which previously had no power of killing streptococci, had now considerable power, and usually the leucocytes—so far as one could ascertain—had not been harmed.

BLOOD EXAMINATIONS OF PATIENTS TREATED WITH "914" COMPOUNDS.

Mrs. G. Puerperal. 0.6 gr. neokharsivan

	Leucocytic efficiency
Blood before dose	0.7
½ hour after dose	0.7
4 hours after	0.41

Bactericidal Power of Serum for Streptococcus

Before dose serum killed less than 4,000 cocci per c.c.	
½ hour after " " 10 million	" "
4 " " " 1 million	" "
24 " " " 1,750,000	" "
60 " " " 4,200	" "

Mrs. B. Pneumonia in the Puerperium. 0.6 gr. N.A.B.

	Leucocytic efficiency
Blood before the dose	0.54
" 14 hours after	0.64

Bactericidal Power of the Serum for Streptococcus

Before the dose the serum killed 0 per c.c.	
14 hours after " " 320,000 per c.c.	

Mrs. C. Puerperal. 0.15 gr. sulpharsenol

Before dose the serum killed less than 560 cocci per c.c.	
24 hours after dose serum killed 70,000 per c.c.	

FIG. 5.

Fig. 5 showed the results obtained in three cases of puerperal infection—the last one a true septicaemia. The bactericidal values recorded in the first case were somewhat larger than usual and the improvement had lasted longer. Frequently it had not persisted so long as twenty-four hours. In view of the reduction of leucocytic efficiency shown by this case it appeared advisable to employ somewhat smaller doses, 0.1 to 0.2 grm. of novarsenobillon, repeated, if necessary, after twenty-four or forty-eight hours, but further work on this question was in progress.

Only the hæmolytic streptococci were very susceptible to these drugs. Staphylococci were slightly susceptible and the other streptococci very little so.

He would be asked what had been achieved in the way of clinical successes with this treatment. So far as he knew neosalvarsan had been used very little in England.

In America, Miller and Chalfant [3] had reported seven cases with streptococci in the blood, five of which had been saved after doses of neosalvarsan. In France an extended series had been reported by Joanny [4] and by Touchard [5], but the records as given were not of great value because no blood-cultures had been done, though the charts indicated that many of the cases were probably cases of septicaemia. Moreover, several other methods of treatment were applied to these cases. The

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authors claimed to have reduced their mortality from over 30 per cent. to below 1 per cent. He (the speaker) considered that these drugs were more worthy of a trial than anything else which was in the field at present.

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[2] WRIGHT, Sir A. E., *Lancet*, 1919, i, p. 489. [3] MILLER and CHALFANT, *Gynaecol. Trans.*, 1918, xliii, p. 469. [4] JOANNY, *Thesis, Univ. of Paris*, 1923, No. 376 (can be seen at the Royal Society of Medicine). [5] TOUCHARD, *idem*, No. 138 (can be seen at the Royal Society of Medicine).

Sir ALMROTH WRIGHT, K.B.E., F.R.S.,

remarked that he had himself done but little of this work spoken of by Dr. Colebrook; he had been very little more than an onlooker, though admittedly some of the suggestions were his own.

What Dr. Colebrook said about immuno-transfusion he would like the meeting to take to heart. Two years ago he (the speaker) came to this Society and stated the principles of immuno-transfusion, expressing the hope that some good would come of it. By inoculating a non-specific microbe he said there should be protection against the streptococcus. That was proved to be so. It was a quantitative matter. This method was quantitatively limited. If one could kill only a certain small number of microbes per cubic centimetre it was not worth trying where there were thousands of microbes per cubic centimetre to be killed. One could argue that a particular engine would draw a train up a particular bank; if it did not one could say the bank was higher than one wanted it. He did not want immuno-transfusion to be put down owing to failures from that point of view. These methods were tentative. It was not yet known what resistances would be obtained. Nor was it known what number of microbes one had to deal with in the blood. As Dr. Colebrook said, this method was good in the border-line cases in which the man wanted such help as he could not get for himself, and that help would sometimes tide him over. Still, it was no remedy for the severer cases.

With regard to drugs, it was useless to say, "I have given such-and-such a drug, and it is not fashionable to do this thing." That was not the way to approach this matter. The proper way was by a careful examination of the blood, under the conditions mentioned by Dr. Colebrook, care being taken not to spoil the white corpuscles more than they were spoilt already, because the killing power of the blood depended upon the integrity of those leucocytes. Even if one killed the microbes by the blood and left the white corpuscles unable to do anything, nothing would be gained by that procedure. It was necessary to demonstrate with every drug given that it did good, that it brought up the bactericidal power of the patient. The going, as the clinician did, to the remote results and paying no attention to the immediate results seemed to be the great fault in all medicine.

Mr. ZACHARY COPE

said he wished to speak as a surgeon and as a clinician. He reminded the meeting that all the pathology of the blood was not yet known, nor all the pathology of the tissues. There were factors which were known and which could be put under the microscope and demonstrated, but there were many more things still to be found out, and yet quite as essential in the problem of immunity to septicæmia. Was it known whether the leucocytes were the only things, or even the chief things, in immunity? Speaking from the clinical aspect, how could we explain why certain microbes selected certain tissues of the body for their activity, and those tissues only? The gonococcus grew well when it was invading mucous membranes or serous membranes; there was a good flow of pus if one put it into the conjunctival sac, and also if one

put it into the urethra. But if it was put into the connective tissues direct there was little formation of pus, and the only difference, so far as he could see, between one and the other was that there was a different cellular tissue surrounding it. What was the function of the connective and epithelial tissues of the body in resisting septicæmia? How could one dogmatize about other points until that was known? Why could not the clinician report what he found? General knowledge on the subject was imperfect. How was it that microbes of the most deadly virulence might be found on the epithelium of the tonsils and on the epithelium of the intestines and no harm result? What was the protection afforded to the body by epithelium apart from leucocytes? He had always been very willing to try methods emanating from the laboratory, such as immuno-transfusion, and to put his case of septicæmia into the hands of the pathologist, because he felt that the pathologist should know more of the scientific aspect of the case. He was willing to say to him, "Take this patient and save him." But in many cases the patients died, and the pathological methods spoken of often proved a failure in the treatment of these cases of septicæmia. Until more knowledge was available the clinician should certainly try the things which were put before him from the scientific side of the profession; he must also use his own judgment on the general lines of treatment to be adopted because he was face to face with the patients, and there were not enough skilled pathologists available to work out experimentally the problem in each case. He therefore contended that it was necessary for the practising physician and surgeon to utilize every likely method of treatment, especially as so many problems still remained unsolved about which it was unwise to dogmatize.

MR. HERBERT TILLEY

gave a few details of his personal experience of an attack of acute septicæmia from which he had suffered some fifteen years ago.

A day or so after his return from a six weeks' holiday, he received a needle-prick on the knuckle of the right index finger while operating on a very foul, chronic abscess of the maxillary antrum. Twenty-four hours later he experienced a stinging sensation over the knuckle, which disturbed sleep the same night. The following morning there was a slight blush in that situation which could have been covered with a threepenny-bit and, in addition, inflamed lymphatic vessels were visible in the skin of the forearm. He felt ill during the day, and at five o'clock, i.e., about fifty-two hours after infection, his temperature was 105°2' F. At six o'clock Dr. Teale gave him an injection into the loin of antistreptococci serum, and for the following three hours he felt practically well. But about 10 p.m. "shiverings" and intense malaise reappeared, and it was with difficulty he could get upstairs to bed. The temperature meantime had risen again to 104°8' F.; a sleepless night followed, and snatches of rest were only obtained by having the arm fixed to the bed-rail over the bed.

On the following morning the back of the hand as far as the wrist was red, tender and swollen—obviously a spreading cellulitis. This condition rapidly extended, and before a week had elapsed seventeen free incisions under general anaesthesia had been made into the hand, forearm, and lower end of the upper arm by his friend Mr. Wilfred Trotter. The cellulitic inflammation began to suppurate about this time.

During this week of spreading subcutaneous inflammation, immersion of the arm in a hot arm-bath every two hours during the day and occasionally at night gave much relief to pain. The baths were followed by the application of hot fomentations made from plain boiled water, because boracic fomentations caused unbearable irritation of the skin.

Sleeplessness proved a troublesome and serious symptom when, after the first few days, morphia and heroin lost their soporific but not their analgesic effects. It was then that large doses of a combination of tincture of opium, chloral and bromide induced refreshing sleep without any evil after-effects.

When the temperature was high a dose of 10 gr. of aspirin produced an extraordinarily profuse perspiration.

Such were a few of the facts which he remembered during a time in which many hours were passed in a semi-delirious condition,—the result of septic intoxication.

He thought that Sir Almroth Wright was pushing the scientific or laboratory aspect of septicæmia too far, and that it would be short-sighted if we ignored the experience and teaching of the great clinicians and observers of the past. The relationship, for instance, between malaria and mosquitoes had been recognized by army leaders many years before the scientific explanation of the fact was established.

Sections of Epidemiology, Comparative Medicine, and Disease in Children.

DISCUSSION ON THE CONTROL OF TUBERCULOSIS AND THE MILK SUPPLY.

Sir JOHN ROBERTSON, C.M.G., O.B.E., M.D.

(Medical Officer of Health, Birmingham.)

THIS discussion seems to me to be very opportune, for there appears to be a very divided scientific and medical opinion as to the part played by tuberculosis of bovine origin in the whole occurrence of human tuberculosis.

On the one hand we have it estimated that at least 3,000 human deaths are due annually to tuberculosis of bovine origin, and that, judged by notifications, probably tens of thousands of children suffer from other forms of tuberculosis which do not cause death, but produce crippling or glandular enlargement, &c., also of bovine origin.

On the other hand we have experienced physicians making the assertion that on the whole the mild bovine bacillus gives that amount of immunity which just saves enormous numbers of people from acute tuberculous disease of human origin. In effect, they say that the presence of a few living bacilli in milk is a good thing for the human race.

The whole subject is extraordinarily difficult and very involved. It is difficult to make a statement at present which will correlate these opposing views.

I have been in the somewhat fortunate position of having at one time of my life seen a very large number of post-mortem examinations on bovines and on human beings. Nobody who has had such an experience could, I think, fail to recognize the close clinical and pathological resemblances to that in man which exist between the lesions and the course of the disease in many bovines. So, too, one cannot help recognizing that the same factors operate as predisposing causes—hereditary predisposition, the absence of light and air, poor food, exhausting conditions, and so on.

Then I have had the opportunity of observing on several occasions the extraordinarily definite way in which infection spreads. The introduction of infection into a herd of cows previously free from tuberculosis, but which are housed under bad conditions, spreads the disease very rapidly. In the case of the human subject it is only occasionally that we get an opportunity of seeing that the introduction of infection rapidly produces the disease in a person otherwise in a suitable condition. With these experiences I early came to the conclusion that the evidence was sufficient to warrant expenditure in reducing the amount of tuberculosis in the milk supply. I recognize, of course, that there are many points that can be raised against such a procedure, but fortunately I have had a city council who are willing to assist.

We have in Birmingham attempted three methods:—

- (1) The testing of samples of milk for tuberculosis and the subsequent elimination of the cows giving tubercle bacilli in the milk.
- (2) Offering to farmers the free testing of their herds provided that they take the necessary trouble to eliminate reactors and thus get tubercle-free herds.
- (3) The correct pasteurization and sterilization of milk.

We have ascertained that about 8·7 per cent. of the churns of milk sent into

Birmingham during the past eighteen years contained living tubercle bacilli. The percentage varied from 4.7 in 1922 to 17.6 in 1917. After the discovery of tubercle bacilli in milk, we send the veterinary officer to the farm and find out the special cow or cows giving tubercle bacilli by the process of taking further samples of milk and inoculating guinea-pigs. We have never relied on microscopical examination alone. The result is that about two months elapse between taking the first sample and identifying the cow. During the interval the public have been supplied with infected milk. We did good by arranging for the visit of a veterinary officer to the farm, whose advice led to a good many wasters being eliminated. For these we paid a small amount of compensation as an inducement to slaughter.

This special milk legislation has done good by drawing attention to the probability of milk infection. The operation is far too slow. We have never been able to clear the herd. We still carry on the work. It is rendered expensive by the time occupied by a skilled veterinary officer and by his travelling, and the laboratory costs. But to the farmers the work has been educative to a high degree. It has prepared them for further steps to eliminate tuberculosis.

In 1906 I spent some time in Denmark with Professor Bang investigating the principles he enunciated for freeing herds from tuberculosis, and as a result my town council adopted the scheme of offering to any farmer, who regularly supplied all his milk to Birmingham, to test all his animals twice annually and to test newly-purchased cows during the interval, provided he took reasonable precautions to rid his herd of reacting animals.

Between the introduction of the scheme and the outbreak of the war we had twenty-eight herds free or practically free from tuberculosis. By 1914 a great many more farmers were making inquiries, and I should say that had the war not intervened there would have been at least 100 herds free by that time. The outbreak of war broke up every one of these herds and we have not yet got back to pre-war conditions.

What interested me was that a considerable number of farmers participated in, or wanted to participate in, our scheme because they believed they would have (a) fewer wasters from tuberculosis, (b) fewer cases of contagious abortion, and (c) fewer cases of Johne's disease. The attendance of a veterinary surgeon at the farm, and the advice he gave, appears to have helped farmers in a way which I did not anticipate.

To-day, I think we have thirty farms free or nearly free. It takes about five years to free a herd by the application of Bang's method. The farmer is not put to any serious expense unless he is stupid in carrying out his isolation and disinfection. He did not get a higher price for his milk in the early days—now, however, he can for his graded milk.

A point which I think is important consists in the results we have obtained from using the various tuberculin tests. If an experienced and careful veterinary officer applies the cutaneous test, our experience is that the results are very satisfactory and reliable. The large majority of officers in the early days obtained results which were difficult to interpret. I was very fortunate in having as a colleague the late John Malcolm, who seemed never to make a mistake. Of course in testing cattle a certain proportion of doubtful results are obtained, as happens in the application of all vital tests. When we had an opportunity of obtaining a post-mortem examination, I believe I may truly say that Mr. Malcolm proved that the test was always reliable when it gave a definite positive result in the class of cattle he was testing.

A great deal of discussion has taken place on the reliability of the tuberculin tests. Our experience is that in the hands of a careful man they are of such practical utility in testing dairy herds that the results can be trusted. On the first testing several herds were found entirely free from tuberculosis, while the rate of infection in several others was 100 per cent., the average being about 30 per cent.

Thirty herds of tubercle-free cows go a very little way towards supplying milk to the city. We are not eliminating infection quickly enough. I therefore went to America three years ago in order to study American methods of commercial pasteurization, with a view to rendering milk free from tubercle bacilli. Here, again, we are quibbling as to whether a pasteurization temperature of 145° F. to 150° F. for thirty minutes will kill all tubercle bacilli under all conceivable conditions, and further, as to whether such heating harms the milk in other aspects. I have, however, recommended our dairy trade to pasteurize and bottle all their milk.

So far as I can gather, the American experience is so good, both as regards destruction of the tubercle bacillus and the absence of damage, that I shall risk the death of one bacillus wrapped up in a heat-protecting casing even if this can occur. Our own tests indicate that commercial pasteurization is reliable if done by well-recognized modern methods. I have not yet found a living tubercle bacillus in milk which has been pasteurized in an efficient apparatus.

To-day 60 or 70 per cent. of the milk supplied to Birmingham has been heated sufficiently to kill tubercle bacilli, and I hope that in a few years no milk will be sold raw unless from tested herds. Indeed, I hesitate to advise raw milk from tubercle-free herds unless it is heated in the nursery. It is mainly a matter of time and amount of milk before raw Grade A milk will spread other infections (scarlet fever, enteric fever, &c.) in one district or another.

The American milk methods are so superior to ours, from a consumer's point of view, that I am convinced we shall adopt them sooner or later.

I think we need a reliable investigation into the merits of our present methods of pasteurization and sterilization. Such an investigation, if applied to ordinary commercial milk, would clear up many doubts which at present are widely prevalent. If they proved conclusively that proper pasteurization and bottling eliminated tubercle infection, then we have in our hands a method which can easily be applied.

I am still in favour of freeing our dairy herds from infection by tubercle, but this is a slow and difficult process. We should need a great addition to the present veterinary profession, we should need radical alterations being made to many cow-houses and we should require patient education of the agriculturist lasting over many years. In the meantime, my advice is to pasteurize commercially where this is possible. If ever the time comes when we can say that all dairy herds are free from tuberculosis, the farmer will undoubtedly be better off, so also will be the milk consumer. Even when that time does come I shall still think it wise to advocate the pasteurization and bottling of all milk for household use.

Unfortunately, it is impossible to give any results as to the diminution of cases of bovine infection among the people of Birmingham.

We have been very active in attempting to limit the spread of all kinds of tubercle infection in Birmingham. On the whole our results compare favourably with those of other large towns. We have about 1,000 fewer cases of pulmonary tuberculosis reported annually—I believe that this accurately reflects the diminution which has occurred recently.

The reported cases of non-pulmonary tuberculosis are unreliable. They, too, are fewer in number. Deaths from non-pulmonary tuberculosis in the city are now about one-half of the rate at which they occur in the whole of England and Wales. I am bold enough to believe that at least part of this reduction is due to our work in limiting infection of both human and bovine origin.

Mr. J. W. BRITTLEBANK, C.M.G. (Manchester).

In attempting to deal with a subject such as this, we cannot address ourselves merely to the immediate public health side of the problem. There is a serious economic problem affecting agriculture which is of enormous dimensions, and it

involves the question where the fight will have to be waged and upon whom most of the cost will fall. The total bovine population of Great Britain is somewhere in the region of $7\frac{1}{2}$ millions; and of these 3,000,000 (in round figures) are cows and heifers in milk or in calf, and I think it may be safely accepted that, on the average, one-third of these would be found to be tuberculous, as ascertained by the tuberculin test. This brings us, speaking again in round numbers, to the fact that 1,000,000 head of our dairy stock are tuberculous in some degree. We know also that many of these are so slightly affected that it makes no material difference to their well-being; indeed, it may be held that these encapsuled lesions are the source of protection to the animal concerned.

So far as the immediate problem is concerned, there is much to be gleaned from the work done by certain public bodies in their efforts to control tuberculosis in milk. Most authorities of any importance concerned with this branch of their public health work have taken advantage of similar powers to those possessed originally by Manchester and Liverpool. These powers confer the right, after samples from the source of production have been proved to contain the organism of tuberculous infection, to visit and examine the cattle with a view to ascertaining the source of infection, the presumption upon which the work is based being that the animals suffering from tuberculosis of the udder are those to be particularly eliminated, and, as far as my experience goes, I am of opinion that such a conclusion is correct. Tuberculosis of the udder is, however, for all practical purposes, invariably a condition secondary to lesions of a primary character situated either in the thorax or abdomen, and therefore, whether the bacilli found in milk are derived from a definitely infected udder, or merely at times escape by that path, it is clear that measures limited simply to removing the active producer of tuberculous milk will achieve little in the reduction of bovine tuberculosis and in freeing permanently the milk supply from this risk. The figures which I submit have been carefully compiled and relate to a continuous period of twenty-three years. They show that 9.88 per cent. of the mixed milk samples examined were found to cause tuberculosis, and a more detailed examination of these figures shows that the percentage containing tuberculous milk in 1923 was higher than in 1901, my first year in Manchester.

At first sight it might be concluded that the work done was of slight, if any, value. This, however, would be far from the truth, for on summarizing my results I find that on approximately 800 farms discovered to be sending tuberculous milk, 587 cows were found to be suffering from tuberculosis of the udder and were removed from the milk supply. So far as the remaining farms are concerned (200 or more in number), on which no cow was found producing tuberculous milk, it was almost invariably reported that certain cows had been disposed of preceding the visit of the inspecting officer, but subsequent to the taking of the initial mixed sample.

A large percentage of the cows were found producing considerable quantities of milk and many were at the height of their lactation period, so that if nothing else had been done, it was an act of some importance to have cut such an amount of infection out of supply.

It is certain that such piecemeal measures as it has been possible to utilize have done nothing to reduce the general incidence of bovine tuberculosis in our milking herds, but then I do not imagine that anyone thought the operation of the Milk Clauses would have any such effect. These Clauses can only have been regarded as preliminary, and it was hoped by far-seeing administrators that they would prove useful in demonstrating the true position of affairs, and that such special experience might provide a sound basis for further legislation of a progressive character.

Many of us hoped that we should be able to induce farmers to eliminate tuberculosis from their herds and so secure a gradually increasing quantity of milk from such tuberculosis-free sources, but it has to be admitted that here we signally failed.

On the other hand, the Milk Clauses may have been a positive hindrance to

general progressive measures, from the fact that the mere existence of such restricted powers as the Clauses provided tended to lull the health service generally into a false sense of security. For many years the dairy farmers strenuously resisted everything that was done to try to secure a reduction in the number of cows producing tuberculous milk. Generally speaking, they as a class have been unable to appreciate preventive medicine. They are accustomed to regard the losses they incur as being irrevocably an incubus on their calling. They ask what extra price they are going to receive for the extra work involved in carrying out the recommendations made to them, and it must be confessed that under the system which has grown up in this country where, generally speaking, all milk produced is regarded commercially as of one quality, it is difficult to convince a farmer that he is not right. We know that a large majority of the general public will only buy the cheapest milk which is in the market.

What then are we to suggest, in the light of our experience, as the correct course to pursue?

We have one school which demands that tuberculosis shall be eliminated by drastic measures, in which the wholesale testing of cows with tuberculin is to be the sheet anchor. In the earlier years of my experience I was an avowed advocate of this method, but let us see what we have to face. I have already pointed out that it is safe to estimate that not less than 1,000,000 of our dairy stock will react to tuberculin, and that they are all therefore potential producers of tuberculous milk.

First, it may be well to inquire briefly why one of three animals concerned is infected with tuberculosis. Bad housing, improper food, lack of sunlight, overwork, overcrowding and dirt, all render animals highly susceptible, and these, with infection ever present in either the food or the dwelling or both, may be cited as principal factors encouraging infection with the tubercle bacillus.

At what early stage in the milk-producing industry will it be possible to get rid of these conditions or, indeed, of any of them? I need only refer to housing to remind you of the magnitude of the elements which may be regarded as essential in any campaign of total eradication by such drastic measures as may be in the minds of some. But even supposing that the housing conditions were good and many of the other conditions I have mentioned were absent, I still believe that any such proposal is economically unsound, for almost inevitably the earliest animals to react to tuberculin are those which work hardest, or in other words, produce most milk, and cows which are of the utmost value for breeding would be condemned to slaughter. It is impossible to practise isolation on such a wholesale scale as would be demanded. The work done by Bang in this respect will repay close examination. Successful elimination has been done in a number of isolated instances, and these apply chiefly to single herds which, in most cases, had been selected as suitable or exceptionable. Therefore, those who have had intimate experience with work of this character will be able to contribute materially to the discussion on this point.

My own experience has been somewhat lengthy, and it may be briefly summed up as follows: given ample accommodation, plenty of money and the right individual, it is certain that reasonable success can be commanded. But it requires indefatigable industry and ceaseless vigilance on the part of the veterinary surgeons responsible, and even then many set-backs and rebuffs must be expected.

My most successful piece of work was done in connexion with a special farm selected to supply certain institutions in the City of Manchester. Without going into detail through the early stages of the work, it is sufficient to indicate that however thorough our methods were, no real success attended our efforts until complete disinfection of all the premises concerned had been carried out. In three years a herd had been established which could be described as being free from tuberculosis. But for some little time, at each re-testing, which was done every six months—I may add most thoroughly done—a few cows were found to react, and these were

mostly animals which had been bought in and had passed the test once. We then decided that the only way to real success appeared to be to dispense with the purchase of recruits from outside sources and to breed our own. This we proceeded to do and the work in itself provided a very interesting series of facts which are not, however, pertinent at the moment to the question. Nevertheless, we took the most elaborate care at every stage in our proceedings, and the net result was that in a few years the milking herd, averaging from 100 to 110 in number, became self-supporting. It is interesting to record that so thorough were our precautions (approaching as nearly as possible in fact to laboratory conditions) that we had no single case of tuberculosis in 257 calves reared, as ascertained by the tuberculin test. The following was the state of affairs in May, 1914:—

- (1) A tuberculosis-free herd had been secured.
- (2) The care and selection in breeding had been so successful as to raise the annual average output of milk per cow by nearly 200 gallons.
- (3) Much interesting information as to cost and feeding had been compiled.
- (4) There was an undoubted gain in the freedom from diseases such as pneumonia, and we were not troubled with contagious abortion.

In August, 1914, owing to the war, other spheres of activity were found for us, but before I left I had several talks with the farmer—a gentleman of exceptional intelligence and resource—as to the best methods of procedure during my absence, which I, of course, hoped would only be short. Unfortunately, however, he soon found himself without veterinary aid and was thrown upon his own resources.

He corresponded with me regularly and, knowing of his experience, I had no fear as to his being able to carry on. Tuberculin was supplied to him and, as far as he was able, he followed the same procedure as he had seen me carry out. Some time later, in 1916, I received a letter from the Medical Officer of Health informing me that three samples of mixed milk supplied from the farm had been found to produce tuberculosis in the guinea-pig. I obtained leave as soon as I could and proceeded to the farm. Here I found a most disastrous state of affairs. Clinical examination alone showed two cows suffering from tuberculosis of the udder, and the herd, except for a very few animals bred on the place, when tested with tuberculin, reacted to the extent of 37·5 per cent. With regard to the explanation of this disaster, it is believed that all went well till the end of 1915, when it was found impossible to exercise the same scrupulous care as hitherto. Some of the heifers were moved to another farm and it was found necessary to bring in a few animals from outside the stock—seven in all, to be precise. These were carefully tested by the farmer and selected as having passed the test. One of these recruits from outside sources I found at my examination to be suffering from tuberculosis of the udder and advanced pulmonary tuberculosis; and the other animal affected with tuberculosis of the udder was a young cow bred on the farm and barely three years old. Regular and systematic disinfection had gone by the board and much of the work done in this respect was unsatisfactory.

It will thus be seen that even a small amount of infection let loose among unprotected or non-immunized animals is disastrous. On the other hand, to those protagonists of clearance by drastic methods, I am tempted to read an extract from my Annual Report of 1909, and to ask what chance there is of getting rid of tuberculosis in conditions such as still prevail on many of the farms concerned in the inquiry. Their occupants do not send milk to Manchester, but their farms remain in exactly the same condition in which I found them in 1909, and they are by no means peculiar to the district concerned.

“It is only possible in a limited space such as is at my disposal to refer briefly to this piece of work, which was of considerable magnitude, and, having been carried out in the summer months, the general conditions prevailing at the farms were, or should have been, at their best; the results obtained are therefore all the more striking.

"The summarized results of this piece of work are as follows: The total number of farms visited was 136; of this number I found six to be clean, fifty-two were fairly clean, and seventy-eight were dirty.

"At five farms out of the gross total of 136 farms there was satisfactory provision for the storage of manure. At twelve other farms the provision made was of a less satisfactory character, but was nevertheless fairly good, while on the remaining 116 farms the manure stand was entirely absent or quite unsatisfactory.

"The total number of cowsheds inspected was 343; of these ten were clean, 147 fairly clean, and the remainder dirty.

"I should here state that the standard of cleanliness expected was not such as might obtain in a well-kept dwelling-house, but such as might reasonably be required in any farm in which a reasonable amount of supervision was exercised. The figures are then all the more striking.

"The lighting of seventy-eight cowsheds was satisfactory, the remaining 295 being either badly lighted or not lighted at all. Eighty-eight cowsheds were sufficiently ventilated, the remaining 255 being badly ventilated.

"The drainage of 151 cowsheds was satisfactory, while in 192 cowsheds it was not.

"In 201 cowsheds the cubic area per cow was sufficient; in 142 it was inadequate. This is somewhat contrary to usual inspection results, and while it was found that the housing was very bad there was little overcrowding.

"Finally, the total number of cows examined was 3,019. The total number of cows found to be suffering from tuberculosis of the udder was eleven. Of these, seven were proved by bacteriological examination. Two were 'dry' cows, in which the diagnosis was confirmed by post-mortem examination. An additional two cows were found to be suffering from undoubted tuberculosis of the udder, but no material being obtainable the diagnosis was not bacteriologically confirmed. In one of these cases a post-mortem examination was subsequently carried out, and ample evidence of marked tuberculosis was found.

"The number of cows suffering from tuberculosis of the udder found in this piece of inspection was comparatively small, but this is to a great extent accounted for by the fact that as soon as it became known that the farms were being visited, large numbers of suspected animals were sent into the local auction market every week and disposed of to individuals who evidently had no difficulty in disposing of this class of cattle.

"The chief purchasers of these emaciated animals were four individuals who were quite well known. Prices for actual 'wasters' ranged from 10s. to 35s.; in addition there was a brisk trade in worn-out old cows at prices ranging from £2 10s. to £5 each.

"On a certain Wednesday morning I watched the business carried on by one auctioneer, and took careful note of the first seventeen animals sold. Of these, fourteen were certainly suffering from advanced tuberculosis, nine of them having undoubted disease of the udder. A fortnight later, of the first fifteen animals sold eleven were undoubtedly tuberculous, six having marked tuberculosis of the udder. This is only what I saw in one auctioneer's section of the market. I should also say that what I have described took place at some considerable distance from Manchester, but it emphasizes one of the points in connexion with the suppression of the 'slink' trade, which is extremely difficult to control so long as such animals are admitted into markets and fairs without hindrance or comment. No hardship would be inflicted if such creatures, many of which can only be brought to auction by cart, and others arriving in a state of exhaustion, were peremptorily seized and condemned to slaughter by the nearest 'knacker.'

"There can be no doubt that if the inspection only resulted in the expulsion of the diseased animals from the herds, a certain amount of good would be done.

"Wholesale inspections of the above character should be carried out as far as is possible in the winter months, when the cattle are housed, and it is possible to see what are the actual conditions under which the cattle are living during those periods of the year when they are not at liberty. It would be possible then also to examine a much larger number in a day."

CONCLUSIONS.

(1) At the moment the incidence rate of bovine tuberculosis is too great to justify immediate eradication by tuberculin testing.

(2) Many cows that are never a serious danger to the milk supply react to tuberculin.

(3) Tuberculin testing applied on a wholesale scale will soon cease to be carried out carefully.

(4) Direct efforts must be made throughout the country to improve housing on inexpensive lines.

(5) Systematic disinfection of all cow-houses at reasonably short intervals in order to destroy much of the great amount of available infection.

(6) General quarterly inspection of all dairy herds by veterinary surgeons. N.B.—Such inspections to be in the hands of experienced men with special "refresher" training. The personnel of this service should consist largely of practitioners working in conjunction with experienced whole-time administrative and consultative veterinary officers.

(7) The re-introduction of the Tuberculosis Order, so amended as to exclude payment of compensation for emaciated animals. The keeping of such animals on a farm should be made an indictable offence.

Professor S. L. CUMMINS, C.B., C.M.G.,

pointed out that he had had no personal opportunity such as had been enjoyed by the previous speakers of carrying out work in connexion with the provision of tubercle-free milk. For this reason he was all the more interested and impressed by the reports which they had furnished.

Speaking as a student of tuberculosis, he said his opinion was that discussions on the questions of the infection of the human subject by tuberculous milk were somewhat dangerous if carried out in public, since the opinion of specialists was still divided on several important points. Whilst he was definitely convinced of the reality and importance of the partial immunization of human beings against pulmonary tuberculosis as a result of the ingestion of small numbers of bovine tubercle bacilli in milk, and whilst he regarded this as one of the causes underlying the favourable alteration in the clinical type and in the death-rate from human tuberculosis noted in recent years, he felt that these considerations, important though they might be, were not an adequate reply to the tragic fact that many children were suffering permanent crippledom and a deterioration of all their vital prospects as the direct result of a preventable infection arising from contaminated milk.

It appeared from what had been said that although tubercle-free herds were practically possible and had actually been brought into being on a relatively large scale, the fact remained that their freedom from tuberculous disease had only been attained as the result of a very considerable expenditure of money, skill and attention to detail. It had been clearly shown that the cattle in such herds were extremely liable to contract tuberculosis if exposed to infection through any breakdown in the elaborate arrangements made to isolate them from other stock. He doubted whether the solution of the milk problem in the case of a relatively poor and over-taxed nation would ever be found in the wholesale organization of tubercle-free herds.

He (Professor Cummins) said that while much more might be accomplished than at present by amended legislation and the awakening of interest in the matter amongst the farming classes and the middlemen, he felt obliged to express his opinion that the only safe solution of the question lay in pasteurization of milk. He laid considerable stress on the importance of quantitative factors in the spread of bovine infection to man, and held that the mixing of milks was a great safeguard so far as it diluted the tubercle bacilli in the infected milks and ensured that the number of bacilli in a given volume as supplied to the consumer was greatly reduced. He said that the experiments of many competent observers had shown that it required relatively large doses of virulent tubercle bacilli to bring about progressive infection through the alimentary tract.

In conclusion, he strongly advocated a closer co-operation between the medical

and veterinary professions, with a view to arriving at agreed facts on all these questions of bovine tuberculosis in its relation to man.

Dr. JAMES NIVEN.

(Late Medical Officer of Health for Manchester.)

For three consecutive years in Oldham, and after 1894 in Manchester, hand-bills were distributed to every house, with the advice *inter alia* that all milk should be boiled before consumption.

In 1894 and the following years a record was made of the farms and cowsheds within the Borough of Manchester, as then constituted, and the Veterinary Surgeon to the Corporation, Mr. James King, also made a report on these and on the cows contained in them, twenty-nine of which he condemned, all of this number being slaughtered. Acting along with the late Professor Delépine, I also examined the cows in a number of cowsheds outside the city, from which milk had been collected, and been shown by Professor Delépine to contain tubercle bacilli. Over 18 per cent. of the cows appeared to have tuberculosis of the udder. Following on these preparations, a number of cowsheds were suppressed, and plans for reconstruction of the others were prepared and gradually carried out.

In 1896 a number of Manchester gentlemen formed a company with a view to supplying the city with milk, on the lines laid down by Mr. G. Busck, of Copenhagen, and carried out by him successfully for a period of forty years in that city.

Mr. Busck had formed a company in 1878, with the approval and support of members of the University of Copenhagen and other eminent citizens, and also of the medical practitioners of the city. In 1888 this company had become so successful that Mr. Busck was able to build a large and fully equipped dairy outside Copenhagen. The company was bound not to declare a dividend above 5 per cent. of the share capital, but to utilize any excess profits to extend the concern. Great pains were taken in selecting farms; this careful selection was possible because of the price given by him for the milk. The farmers were bound to keep only first-class stock, and every precaution was taken to ensure that they did so. About this period the operations were described by Dr. Sims Woodhead and Mr. Stewart McGregor, British Vice-Consul in Copenhagen.

Seven competent veterinary officers were engaged to choose and supervise the care of the cattle, those which showed signs of disease being promptly removed. The condition of the fodder was watched by a special inspector, and the hygiene of milking and of the dairy by an inspecting dairymaid.

All milk had to be cooled down to 41° F. directly after milking, and the temperature kept below 50° F. until the milk reached the dairy. This was possible because farmers were able to collect ice, and kept it stored under non-conducting material, the requisite amount always in store being 30 lb. of ice for every 100 lb. of milk produced. The milk was kept in vessels surrounded with ice from which cream rose for ten hours, and it was then skimmed. The evening's milk was the ordinary daily supply, and a special children's milk obtained from carefully chosen cows, fed in winter on a prescribed special diet. In summer all cows had to be fed on the meadows. All these milks, the cream, the children's milk, the morning's skim milk, and the evening's milk, on arrival at the dairy were passed through four special filters, sterilized and cleaned daily. In summer the milks were conveyed in covered railway vans refrigerated with ice, and each form of cream and milk was conveyed in sealed, locked cans. The creams, children's milk, skim milk, and ordinary milk were all tested on arrival, and average samples of each form of milk were also sent to the professor of chemistry at the University to be tested for chemical quality. The cream and children's milk were at once filtered, passed into bottles, and sealed. The bottles were then placed in boxes and surrounded with ice. The other milks were kept in the cans in which they arrived, the latter being plunged into water mixed with ice.

Every precaution was taken to ensure that at no stage could the milk be tampered with. From the delivery cans it was drawn into hollow stems perforated at the sides and communicating with taps below. Owing to the special care used in feeding the cows, all milk had a very fragrant taste. Great pains were taken to ensure that all vessels and bottles were

thoroughly washed, cleaned, and sterilized, and that they were returned clean to the farmer, who was bound to wash out all dust with clean water.

All possible care was taken to ensure that no case of infectious disease in an employee at the farm or at the dairy, or in his family, passed unreported, and it was made to his interest to notify at once. It was also made to the interest of the farmer to see that no disease observed in any cow failed to be reported at once to the veterinary surgeon.

The milk from this dairy (about 4,000 gallons daily) was undoubtedly of the first quality, and it was not otherwise guarded against tuberculous infection, except by the vigilance of the veterinary surgeons employed, nor was it absolutely protected from the infection of other diseases.

In 1896 Alderman Bosdin Leach, Dr. Henry Ashby, and the Medical Officer of Health visited Copenhagen, and in 1899 the premises of the Manchester Company were opened. These consisted of an admirable dairy on the model of Mr. Busck's dairy; an ice-making plant was provided, and ice was supplied to the farmers; the best farms procurable were contracted with. Unfortunately, enough capital to see the enterprise through could not be raised, and the company was liquidated in 1901. It should be mentioned that arrangements were made for freeing the herds from contractors of tuberculosis after application of the tuberculin test, and that later on this was also done in the case of Mr. Busck's farms near Copenhagen.

In 1899, following the suggestion of the Commission on Tuberculosis of 1898, powers were applied for by the Manchester Sanitary Committee, similar to those enjoyed by Glasgow under a Local Police Act; powers were obtained, with the assistance of the Local Government Board, by Manchester and some other authorities, known afterwards as the Model Milk Clauses. Practically these were applied by taking samples of milk on arrival in the city, or production there. These samples were submitted to Professor Delépine, who, after an interval of about a month, determined whether they contained or did not contain the infection of tuberculosis. If the sample was tuberculous, the veterinary surgeon visited the farm supplying the milk, examined the cows, and took samples of milk from those udders which appeared to him suspicious, and these he submitted to Professor Delépine. When one of these samples was found to be infected, the cow was considered to be suffering from tuberculosis of the udder and had to be isolated. But the only useful procedure in these cases is to have the cow slaughtered, and the veterinary surgeon, Lieut.-Colonel J. W. Brittlebank, was very successful in getting this carried out.

Unquestionably this procedure was useful though it was never supposed that it would be effectual in eradicating tuberculosis. But it afforded an opportunity of educating the farmer and of improving conditions generally. Thus the veterinary surgeon was able to influence materially the choice of stock, the methods of feeding, cleanliness on the farm, the keeping of milk, the condition of the water supplies, and their protection from contamination, &c. In several instances he was able to get the premises reconstructed. That the results were good was shown by Professor Delépine's examination of samples for dirt, which for some years continued to diminish, while the number of tuberculous udders continued to decrease. That the diminution of dirt was real was shown by the diminished sepsis in guinea-pigs inoculated with the milks.

After a time, however, the improvement disappeared and Professor Delépine concluded that the dirt in milk was merely being concealed by filtration.

About 1910 the proportion of samples found tuberculous increased, and since that time has remained high. There was, however, still evidence that the Clauses were operative, as the tuberculous udders were not markedly tuberculous. But the competition for milk farms near London and Liverpool acted unfavourably on continued control, and the outbreak of war led to a general breakdown for a time of the results previously attained.

It will not be easy to regain the lost ground, chiefly on account of the deterioration of stock. But my opinion is that these Clauses are valuable, especially in connexion

with the effort to improve the conditions of dairying, as they give the veterinary surgeon an opportunity of carrying on quietly a process of education very much needed. Meanwhile the conditions of the milk reaching Manchester are bad as regards cleanliness, and the immediate question is, what is to be done? The Report of the Astor Committee on the Production and Distribution of the Milk Supply, 1920, shows that the quantity of milk consumed per head of the population is very low in this country as compared with other European countries and with the United States; the causes are traced and remedies proposed. The smallness of the amount consumed is undoubtedly due to the defective quality of the milk as regards cleanliness, and to the ignorance of the population in respect of the high nutritive value and the economy of milk as an article of food. But there is also ignorance as to the methods by which a dairy stock of high milking value may be reared, as to the best and most economic methods of feeding dairy cows, as to the most advantageous methods of utilizing the land so as to obtain a high food value for cattle, and as regards the measures required and practicable to secure a clean milk supply. Strenuous efforts are advised and are being brought into operation so as to remedy the defects. Were the advice given in this report carried out, there is no doubt that this country would take its proper place as a dairying country. But there are many accessory measures which are required. For example, a milk propaganda is needed in order to show the value and great benefit to the children of a good and clean supply, though such a propaganda must go side by side with the other measures required to raise the quality of the milking stock and to ensure cleanliness. In fact, the cultivation of cleanliness is necessary to ensure even a comparative degree of freedom from tuberculosis.

Different observers who have visited the United States testify to the admirable quality of the milk supply in the great cities. This was done by Dr. Eastwood in 1908, by Sir John Robertson in 1922, and Dr. Carnwath in 1923. Dr. Eastwood tells us that for many years great efforts have been put forth by the Live Stock Boards of the more Northern States to improve the quality of the dairy herds, and large sums have been expended in freeing the herds from tuberculosis. It is at best an expensive operation, and Dr. Eastwood is of opinion that it is bad policy to give compensation out of public money in respect of cattle suffering from advanced tuberculosis or from udder disease. Nevertheless, the energy displayed has been very great. One of the features of the efforts to free herds from tuberculosis is that the actual testing has been left to the farmers themselves. The records however are interpreted by experts. It is far from an easy matter to obtain tuberculosis-free herds, and a notable feature of the process is that, when the disease has been eradicated, if a tuberculous animal is introduced into the herd the disease is apt to spread rapidly and undo all the gain already achieved. This is especially liable to happen unless the utmost cleanliness and the most careful disinfection are practised in the tuberculosis-free cowshed.

Nevertheless, with sufficient care, herds may be maintained free, as Sir John Robertson has found in some thirty herds around Birmingham. In Manchester the hospitals were supplied for many years with a very rich milk obtained from a tuberculosis-free herd, maintained in that condition by the unremitting vigilance of Lieut.-Col. Brittlebank. He (Colonel Brittlebank) found, however, that this nutritious and untainted supply was only possible by the practice of most careful cleanliness and disinfection and by rearing a tuberculosis-free stock from calves.

The first point in the American campaign, then, is the great effort made to secure herds free from tuberculosis. America possesses many facilities for enabling the good quality of the milk supply to be maintained.

- (1) There is abundance of ice in winter in the Northern States, which can be stored and made available for cooling the milk during summer.
- (2) Local authorities have powers, independent of the Central Government, of regulating

and controlling their milk supplies both within the cities and outside at the place of production.

(3) In a large city like New York the milk is brought fresh from long distances extending to 500 miles. This is only made possible by carrying it in refrigerating vans, and the great cities have been able, not without an effort, to impose on railway companies the task of keeping the milk refrigerated during transport.

(4) The great attention given to the improvement of dairy stock.

(5) The education of producers both by way of assistance and coercion.

(6) Probably a large proportion of farmers own their own farms, which makes a great difference in the efforts put forth to improve farmsteads, stock, and methods of recording and feeding.

Perhaps the most important of these conditions is the power enjoyed by sanitary authorities of making regulations and of controlling the production, transport and sale of milk. It appears very desirable that local authorities in this country should enjoy similar powers, and when in possession of them to put a great deal of energy into carrying them out.

Milk in the great cities is said to be fresh, rich, cool, clean and free from disease. Its freshness is due to cooling from production to arrival. It is rich owing to the great amount of study given to the production of good milking strains of cows. It is customary to contract with farmers to provide milk yielding 3.5 per cent. of fat, with a proviso that for even $\frac{1}{10}$ per cent. of butter-fat over 3.5 per cent., extra will be paid to the farmer at a rate which makes it worth his while to endeavour to get the largest amount of fat possible. It is clean because the regulations provide that, on arrival at a pasteurizing station, milk must not contain more than 100,000 germs when sown on an appropriate soil.

Milk is graded according to the number of germs contained in one cubic centimetre on arrival.

(a) Grade A is milk obtained from herds guaranteed free from tuberculosis after the application of the tuberculin test; it must not contain more than 30,000 bacteria per cubic centimetre.

(b) Grade B is milk handled under sanitary conditions, cooled and transported so that it may reach the pasteurizer with not more than 100,000 bacteria per cubic centimetre. Practically all milk put on the market in New York for human consumption falls into these two grades. At least 90 per cent. of the milk supply in large cities falls under Grade B, and the greater part of the remainder under Grade A.

The local authority has to put forth a great effort to ensure that Grade B milk arrives at the pasteurizer with a temperature not exceeding 50° F., and with no more than the prescribed number of bacteria. It is necessary to prescribe a definite number so that the cleanliness of the milk can be controlled before pasteurization, and so that it is really good milk and not pus which is put into the pasteurizer.

Freedom from disease is secured by pasteurization, and it is now agreed that the pasteurization must be carried out by exposure to a temperature between 145° and 150° F. for not less than 30 minutes. Great diversity has existed in the method of pasteurization. But the Ministry of Health has prescribed in this country conditions which require for their fulfilment pasteurization in a "holding" pasteurizer of the newest type.

It appears certain that such a pasteurizer, if properly worked, does effectively destroy tubercle bacilli and other germs of the common diseases. But in order to ensure that the milk on arrival is of the requisite quality the cities supplied have, in the United States, to exercise an energetic control.

By a regulation for the City of New York, "no milk shall be received, held, kept, offered for sale, or delivered in the City of New York, without a permit from the Board of Health, and subject to the conditions thereof."

Section 53 of the Code prohibits the introduction into the city, or offering for sale within the city, of any milk which is "adulterated," and for the purposes of the Code milk is *adulterated* if it falls under one or more of nine specified definitions. For example: (a) milk drawn from animals fed on distillery water or any substance in a state of fermentation or putrefaction, or on any unwholesome food; (b) milk drawn from cows kept in a crowded or unhealthy condition; (c) milk the temperature of which is higher than 50° F. is *adulterated*.

Section 54 ordains that "any milk found to be *adulterated*, which has been brought into the City of New York, or is held or offered for sale in the said City, may be seized or destroyed by any inspector or other officer of the department authorized to inspect milk."

Every applicant for a permit to sell milk either in a shop or from a wagon, must inform

the department of the source from which he obtains his milk. This information is followed up by the department; the sources supplying the place from which the retailer obtains his milk are ascertained, and are traced back to the actual producers. As a condition to the issue of a permit, all places where such milk is produced or handled must be open to inspection by the employees of the department. Refusal to admit inspection would be followed by exclusion of the supply through withdrawal of the permit. Similarly, unless sanitary conditions are remedied, the same result follows. Special arrangements are made to ensure early notification of infectious disease in any person handling the milk.

The number of permits issued to stores in the city was between 12,000 and 13,000 in 1908. The average daily consumption was 1,800,000 quarts. If an inspector finds milk obviously adulterated, he is allowed to seize it without further parley, and pour it into a sewer, no compensation being given to the owner.

According to the Annual Reports of the Department of Health, 39,613 quarts of adulterated milk were destroyed in 1905 and 41,385 quarts in 1906; most of this under the clause which defined milk at a temperature over 50° F. as adulterated. In 1908 the department employed twenty-seven county inspectors in examining the creameries and milk supplying the city. The number of farms was estimated at over 40,000.

The farms supplying milk must come up to a certain standard of sanitary condition. To ensure this the inspectors at their visits record the conditions at the farms on score cards, which are filed. The farmers compete with each other in obtaining the highest number of marks, and the system produces a steady and marked improvement in the condition of the farms.

Behind all this lies the possibility that if the farm is not maintained under conditions satisfactory to the city authorities, the permit to supply the city may be withdrawn.

It will be seen that, in order to produce milk of quality equal to that sold in New York, great efforts would be required all round. Cities such as Manchester would need to be invested with powers to control their own milk supplies and would require to exercise them. In particular it would be necessary to appoint an adequate staff of veterinary inspectors to visit outside farms, to inspect the farms and cows, and to advise as to the measures required for the improvement of conditions, stock, feeding, &c., besides putting the model milk clauses into more complete operation. They would also require to instruct farmers in the methods required to produce a clean milk supply capable of remaining fresh up to the time of arrival at the city pasteurizers.

Would it be possible to obtain the requisite degree of improvement in the milk supply by good pasteurization alone? Organization for the provision of ice, and refrigeration at milking, and in transport after pasteurization, will no doubt be established in course of time. But as its establishment cannot be expected immediately, it is therefore the more necessary that farmers should be carefully instructed as to how to produce a clean milk supply capable of being kept pure until it reaches the city pasteurizers. Such a supply, if obtained within say 60 miles, under suitable arrangements with railway companies, properly pasteurized under an adequate control by samples and bacterial examination, might be sufficient for the present. But where milk is obtained from long distances, as in London, it would seem that the use of ice for refrigeration is almost indispensable.

If, again, we are to rely on pasteurization in order to obtain a proper supply of milk, it is evident that inspectors would be required who should possess the mechanical knowledge, and be sufficiently trained, to be able to keep an adequate control after every operation at the pasteurizing station.

Have we reached a point at which we could insist on the general pasteurization of fresh milk, and its sale in properly cleaned bottles, with the observance of conditions similar to those under which pasteurized (Grade B) milk is sold in New York? It is true that the conditions of good pasteurization have been rapidly advancing. But it is necessary that those newer conditions should be controlled and confirmed before it would be quite safe to rely entirely on pasteurization, or to compel the pasteurization of all milk reaching a city. Nor would it be advisable just yet to enforce regulations which would tend to exclude from consumption raw milk duly safeguarded.

[March 9, 1925.]

Dr. M. J. ROWLANDS.

At our last meeting on February 9, we heard from several speakers their views on the methods of preventing tubercle bacilli from being imbibed in the living state by human beings.

Colonel Brittlebank described the methods in the Manchester area, stating that they had been able to get a herd of cattle free from tubercle, but that they very soon reverted when there was the least neglect. What was the neglect?

Sir John Robertson, of Birmingham, also very ably gave us his views.

As a farmer with considerable experience in the feeding of stock, I have come to the conclusion that it is almost impossible to get a tubercle-free herd of cows under present-day conditions. Conditions of farming have changed enormously, and I should like, if possible, to learn whether any figures can be obtained which would enable one to come to a definite conclusion as to whether tuberculosis is on the increase or on the decrease amongst our dairy cattle. I am not referring to the last ten or twenty years, but I wish to know whether tuberculous disease was more common fifty or sixty years ago than it is to-day.

There is no doubt that hygiene, segregation and a testing of cattle will undoubtedly lead to a diminution, but that diminution will not free the herd from tuberculosis. We all know that if tuberculosis invades a tubercle-free herd it will be a very severe form, thus proving that the freedom from tuberculosis is not due to immunity, but due to freedom from infection.

Let us take the position on the farm to-day and the position many years ago.

The feeding of cattle previous to the manufacturing of cakes from such materials as palm kernel, cocoa-nut, linseed, soya bean, earth nut, &c., was carried on almost entirely by grass feeding during the summer, spring and autumn, and the feeding of hay with bran and oats as well as other wheat products during the winter. At the present day the chief method of feeding consists in giving artificial foods in the form of compressed cakes, given chiefly for their albuminoid, treacle and oil content. Here I think lies one of the chief causes of advancing tuberculosis among dairy herds. I cannot help feeling that the vitamin question plays an important part in the spread of tuberculosis amongst dairy cattle. In the old days the grass was rich in its three vitamins. The bran, wheat products and oats were also fairly rich in vitamins. Not so to-day.

We heard Colonel Brittlebank state his opinion that the cause of tuberculosis amongst dairy herds was bad feeding, bad housing, and overworking. This leads me to the second point of overworking. The cow was never meant to produce milk for human beings. Nature meant that cow to produce milk for her own calf, no more and no less. In the natural state, the cow would calve down in the spring when vitamins are sufficient in natural foods to supply it with its necessary quota. To-day 1,000 gallons of milk per year is the minimum you expect to receive from a cow. As much as 2,000 gallons is quite common, and higher numbers than this are now on record. Even Guernsey cows, which are not supposed to be heavy milkers, or at least were not considered to be heavy milkers, are now to be found in many herds giving 1,500 gallons of milk per year. I have just bought a bull calf whose dam gave 1,400 gallons. This excessive draining of fats and proteins must be replaced by fats and proteins, and the farmer has found out that these could be replaced to some extent by means of artificial cakes. But on testing these cakes I find that they are absolutely deficient in vitamin A content, and as you cannot get a large milk supply unless you supply concentrated food, sooner or latter your cow becomes deficient in her storage of vitamin A, with the result that when she next calves, that calf only receives very little vitamin A, and perhaps very little vitamin B. The result is a low immunity to tuberculosis and other disease (? abortion).

I will now give you my experience on my own farm.

I breed about 2,000 pigs per year, and as the pig is an animal which breeds rapidly and comes to maturity in a very quick time, we are able to notice the differences much more rapidly on a farm such as mine, than on a dairy farm where you have only one calf per year per cow. That calf does not come to its milking stage until it is at least three years old, whereas a sow up to this age will have had five litters or certainly four, with an average of eight or nine pigs per litter, and at four months old the majority of these pigs are killed and are fit for the market.

When I began pig keeping on a large scale some ten years ago, my greatest trouble was infection by tuberculosis, as shown by enlarged glands and pleurisy, as well as bone diseases. This troubled me greatly as my pigs were housed under the most hygienic conditions; the animals were all kept on the open-air system and they were fed on the recognized method of that time, namely, palm kernel meal, barley meal and middlings, with perhaps a little fish or meat meal. I made up my mind to watch every litter and see that every sow that bred a pig which suffered from enlarged glands or any tubercle infection was slaughtered. The boars were also very carefully watched in case two or three litters which had been sired by the same boar became affected. Even this segregation and the killing off of these dams did not result in reducing my anxiety. Whenever I purchased animals I was always asking for trouble. I can well remember purchasing twelve young pigs from a well-known breeder at fairly high prices; not one of these twelve pigs was I able to rear. They all had to be killed, as they showed signs of marked tuberculous disease, both ante mortem and post mortem.

The curious point is this, that immediately I began to feed my animals, taking into consideration the value of vitamin foods, my trouble soon came to an end, with the result that within one year the whole of the difficulty was over. Hardly ever was there a pig killed for the next four years that showed any signs of tuberculosis.

Now comes another interesting point. I sold this farm, and the animals which I had bred, to a company, and I had to re-start myself once more. All the animals had to be purchased from outside and out of 120 female pigs that I bought only forty of them proved to be healthy and free from disease, chiefly tuberculosis. Again, I did no segregation except to kill off any animals which were not thrifty, and examine their carcasses. My housing was exactly the same as I had always employed, namely, the open-air method; but by this time my knowledge of vitamins was much more extensive than when I began farming on a large scale, with the result that during this last year I have killed, since September last, an average of thirty pigs per week, and only about four carcasses have shown signs of pleurisy, with a few adhesions which were old. Not a single case of enlargement of the glands has been encountered, and as we kill all pigs on the farm and all carcasses are examined by myself, it has been possible to keep a careful look-out for signs of tuberculosis.

Now comes the whole question, what has been the cause of the diminution, or shall we say freedom, from tuberculosis in my herd? Certainly not housing, certainly not killing off the tuberculous pigs, and certainly not isolation, as I have never carried out any isolation, neither have I carried out disinfection to any great extent. The only possible reason that I can see is that I have bred a herd of pigs which are much stronger than the usual type. Owing to my supplying the three vitamins (A, B and C) to the pigs, I have raised their immunity to tuberculosis; and not only have I raised their immunity to tuberculosis, but I have increased it to other diseases. I am convinced from my own experience that neither cleanliness, nor sunlight, nor segregation and disinfection are going to prove of any value, good as their effect may be. They certainly will not eradicate tuberculosis amongst dairy herds.

Only a short while ago I visited, in my opinion, the best kept herd of cattle in

this country from a hygienic point of view. A large number of prizes had been won by this herd at all the leading shows—in fact the estates are one of the show places in this country; and yet almost every show animal reacted to the tuberculin test, whereas the rest of the milking herd, which were not forced on artificial foods, did not react. Yet the housing methods were exactly similar, in fact they were better amongst the prize-winning portion of the herd than amongst the ordinary dairy herd.

People who produce Grade A and certified milk are constantly telling me that they cannot keep it up owing to the number of cows that continually react, having been perfectly free, as shown by the test previously.

Now to sum up: in my opinion, under modern methods of feeding for high milk production on non-vitamin foods you will never solve the question of tuberculosis, neither will you free a herd from it. It is certain that we shall have to do something to the milk supply in the form of sterilization, and yet supplying the deficiency as the result of sterilization to the milk: the vitamin A in the form of cod-liver oil, and the B and C vitamins in the form of materials rich in these accessory factors. I confess that this is quite a new method of looking at the question of tuberculosis, but I ask the members of both the medical and veterinary professions to give this side a thought; there is certainly food for thought here.

A very strange coincidence is that tuberculosis is almost unknown, or is at least very rare in sheep and horses; also, I have not seen tuberculosis among fattening bullocks. This surely is due to the sheep and horse only giving milk for a short period; in fact this is what the cow would do were it allowed to follow its natural instinct. We are told the horse can be infected by milk. The pig, like the cow, but not quite to the same extent, is being very heavily milked. The cow is milked from some eight to ten months without a rest; in some instances longer. The sow has a rest for four months and is heavily milking once more. She will suckle two litters with only a four-months' rest. The wild pig, that is the sow only, farrows once in a year, and that in the spring, when there are plenty of natural foods containing the three vitamins. I have lately been examining the stools of my pigs, and it is very strange that my pigs, as compared with pigs on the estate of Dr. Chalmers Watson, pass just about one-third part of protein that his do. The same with fat, although I feed a much higher protein value than he does. My milking cows have a much higher protein value than most owners' cows; also, I see that this food is at all times rich in the A, B and C vitamins. I give chiefly animal protein and not vegetable protein; my pigs always receive animal protein, never vegetable protein. I find there is a conversional loss of about 25 per cent. in converting vegetable protein into heat or milk, as compared with animal protein. To see the conditions under which my brood sows live would astonish most of the audience: but not a cough; not a stiff pig, although up to their hocks in liquid mud! My cows are out wet or fine, yet no reactions. It is true I keep only five to six cows, but I keep 120 brood sows. My farm is 800 ft. above the sea-level on clay, and all my stock have to "rough it"; yet no tuberculosis. Why? Entirely a question of feeding; a proper diet, rich in the essentials, carbohydrate, protein, &c., as well as mineral salts and vitamins. By all means do away with all tuberculous cows, but do let us prevent further infection. Dr. Williams told me once at Reading there was only one way to sterilize a cowshed—cover the tubercle bacillus with whitewash so that it cannot get out.

Dr. A. STANLEY GRIFFITH.

My contribution to this discussion is a summary of investigations undertaken to ascertain the incidence of bovine infection on human beings in this country.

The investigations with which I will deal have been carried out by one group of workers, namely, Drs. L. Cobbett, A. Eastwood, F. Griffith, and myself. The total

number of cases of human tuberculosis examined bacteriologically exceeds 1,450. Of these nearly 150 were apparently cured, since the material did not infect guinea-pigs or yield cultures. The series comprises all the chief clinical varieties of tuberculosis and includes a large number of post-mortem cases.

The results obtained by the Royal Commission on Tuberculosis, published in the year 1911, disproved Koch's assertion that bovine tuberculosis was not a serious danger to human beings. The bovine tubercle bacillus was found by the Commission in cases of cervical gland tuberculosis, general tuberculosis originating in the alimentary tract, lupus and phthisis pulmonalis.

Subsequent investigations, which were undertaken mainly to define the extent of the danger arising from the consumption of infected milk, by the examination of unselected material, have fully confirmed the results obtained by the Commission.

The investigations are summarized in the following tables, which give the proportion of human to bovine infections at various age-periods in the different groups of cases examined.

CERVICAL GLAND TUBERCULOSIS.

Age periods	Number of cases	Human	Bovine	T.B. dead	Percentage bovine
0-5 years ...	20	3	17	9	85.0
5-10 years ...	34	13	21	11	61.8
10-15 years ...	28	17	11	6	39.3
15-20 years ...	21	14	7	12	38.3
20 and upwards ...	23	18	4		18.2
All ages ...	125	65	60	38*	48.0

* 3, age not stated.

The percentage of bovine infections is seen to be highest in children under 5 years of age. As age advances there is a progressive diminution in the incidence of bovine infections, but even in adult life 18.2 per cent. of the cases were of bovine origin.

BONE AND JOINT TUBERCULOSIS.

Age periods	Number of cases	Human	Bovine	Percentage of bovine
0-5 years ...	96	67	29	30.2
5-10 years ...	217	166	51	23.5
10-16 years ...	133	118	15	11.3
16 and upwards	68	64	4	5.9
All ages ...	514	415	99	19.2

This table shows that in this country bone and joint tuberculosis is more frequently caused by human than by bovine tubercle bacilli. In a small series of cases from Scotland there was a higher percentage of bovine infections than in the English series, though not equal to Fraser's.

SCROFULODERMA.

Age periods	Number of cases	Human	Bovine	Percentage of bovine
0-5 years ...	12	5	7	58.3
5-16 years ...	27	15	12	44.4
16 and upwards	13	13	1	7.7
All ages ...	52	32	20	38.4

These were cases with tuberculous abscesses in the skin, and were nearly all supplied to me by Dr. H. A. Ellis, when he was acting tuberculosis officer in Middlesbrough.

LUPUS.

Age periods	Number of cases	Human	Bovine	Percentage of bovine
0-5 years ...	50	17	33	66.0
5-10 years ...	53	29	24	45.3
10-16 years ...	20	8	12	60.0
16 and upwards	17	14	3	17.6
All ages ...	140	68	72	51.4

The ages given in the tables for scrofuloderma and lupus are the ages when the disease was stated to have begun. About 70 per cent. of the lupus cultures of each type showed some degree of attenuation. The investigations of lupus have extended

over many years, and it is interesting to compare the relative proportions of bovine to human infections at different periods. Of the cases which contracted the disease before 1910, 50 per cent. were bovine. Of those in which lupus first appeared in the decade 1910-1920, 55 per cent. were bovine.

Attention may here be called to the fact that in very few of the cases of lupus and scrofuloderma, or of bone and joint tuberculosis of bovine origin, was there any clinical evidence of tuberculosis in the neck or abdomen. The absence of evidence of tuberculosis of the alimentary tract in these cases indicates that the persons affected had in all probability been invaded by a relatively small number of bovine tubercle bacilli.

GENITO-URINARY TUBERCULOSIS.				
Age periods	Number of cases	Human	Bovine	Percentage of bovine
All ages ...	21	17	4	19.0

Three of the "bovine" cases were affections of the kidney in persons aged respectively 19, 20 or 25 years.

PULMONARY TUBERCULOSIS.				
Age periods	Number of cases	Human	Bovine	Atypical
All ages ...	275	267	3	5
				Percentage bovine
				1.1

In only three cases of ulcerative pulmonary tuberculosis has the disease been proved to be due to bovine tubercle bacilli.

TUBERCULOUS MENINGITIS.				
Age periods	Number of cases	Human	Bovine	Percentage of bovine
5-10 years ...	3	1	2	66.6
10-16 years ...	5	5	—	—
16 and upwards ...	4	4	—	—
All ages ...	12	10	2	16.6

The original material in this series was cerebro-spinal fluid withdrawn during life. In a series of forty-eight children under 12, who died from tuberculous meningitis, in which the cultures were isolated not from the meninges, but from some other part of the body, the percentage of bovine infections was 14.6.

POST-MORTEM MATERIAL FROM CHILDREN—LOCAL GOVERNMENT BOARD SERIES.

Age periods	Number of cases	Human	Bovine	Percentage of bovine
0-5 years ...	61	48	13	21.3
5-12 years ...	52	46*	6	11.5
All ages ...	113	94	19	16.8

* Including one dysgonic human strain.

ROYAL COMMISSION ON TUBERCULOSIS SERIES.

Age periods	Number of cases	Human	Bovine	Percentage of bovine
0-5 years ...	32	17	15	46.9
5-15 years ...	6	5	1	16.6
All ages ...	38	22	16	42.1

These two series are exhibited separately because the Commission's cases were, to some extent, selected.

I have analysed the post-mortem cases and have classified them according to the probable portal of entry of the bacilli. The classification is based on the anatomical distribution of the primary lesions.

POST-MORTEM CASES IN CHILDREN, UNDER 12 YEARS OF AGE, CLASSIFIED ACCORDING TO THE ANATOMICAL DISTRIBUTION OF THE PRIMARY LESIONS.

Portal of entry	Number of cases	Human	Bovine	Percentage of bovine
Alimentary tract ...	35	7	28	80.0
Respiratory tract ...	77	76	1	1.3
Double portal (respiratory and alimentary) ...	35	34	1	2.8
Uncertain ...	4	4	—	—

The analysis has shown that whenever the bronchial glands are severely affected, alone or in conjunction with the glands of the alimentary tract, the infection is almost invariably human. When on the other hand the mesenteric glands are

severely affected and the bronchial glands not at all, or only slightly and more recently, the infection is predominantly bovine. Another point brought out by this analysis is that primary abdominal tuberculosis due to the human type of bacillus is relatively infrequent. These facts, taken in conjunction with the rarity of bovine infections in phthisis, disprove in my opinion Calmette's contention that the bacilli which cause phthisis generally invade the body from the alimentary tract.

In this connexion it should be stated that there is no evidence that bovine tubercle bacilli are changed into human tubercle bacilli during long residence in human tissues.

SUMMARY.

The bovine tubercle bacillus plays a part in the causation of all the chief varieties of human tuberculosis and can produce lesions in every tissue of the body indistinguishable from tuberculosis of human origin.

The proportion of bovine to human infections is highest in children under five years of age and in those forms of tuberculosis which primarily affect the cervical glands and the intestines and mesenteric glands. In these forms of tuberculosis the percentage of bovine infections ranges from 80 to 85.

In cervical gland tuberculosis and lupus at all ages, infection of bovine origin accounts for about half the cases, whilst in bone and joint tuberculosis, genito-urinary tuberculosis and tuberculous meningitis, diseases caused by bacilli which have been distributed over the body by the blood-stream, the proportion of bovine infections ranges from about one-fifth to one-sixth of the total cases examined. These figures prove conclusively that a considerable amount of the tuberculosis of human beings, especially in childhood, is caused in this country by the bacillus of bovine tuberculosis.

Mr. GEORGE P. MALE.

The position as revealed by the addresses given at the last meeting is very unsatisfactory, and the counsels given were mostly those of despair.

Although the present position is bad there are many hopeful features, the chief of which, in my opinion, is the rapid increase in the number of certified and Grade A tuberculin-tested herds. Although the scheme for their promotion has only been in operation a short time, there are at the present moment seventy-four farms producing certified milk, sixty-six farms producing Grade A tuberculin-tested milk and a large number of Grade A herds.

During the last twelve months the two first grades have so increased that they now nearly equal the whole of the three grades in existence at the end of 1923. If institutions similar to the National Institute for Research in Dairying, over which my friend Dr. Stenhouse Williams presides, could be established in every county of England, there would, in a few years, be such a diminution in the amount of tubercle bacilli in the milk, and such an improvement in the quality and cleanliness, that there would be little need for anxiety with regard to the milk supply of this country. Records of the early tests showed that considerably over 50 per cent. reacted, whereas now it would be under 5 per cent. I believe I may say that there are very few herds in that district which have not been tested.

The buildings and general hygienic conditions have been immensely improved, and consequently the general health of the cows; but it was not until Dr. Williams came to Reading that an extensive campaign was instituted. He demonstrated the importance of sterilization of all vessels containing milk, and what is more important, he went round to the farms and instructed not only the farmers but also the workers exactly as to how this sterilization should be carried out, and with striking results. Clean milk competitions were instituted, prizes were given to the workers and demonstrations were held until the workers took a pride and interest in the work.

Dr. Stenhouse Williams proved that clean milk could be produced in old buildings with little structural alterations, and he revolutionized current ideas on the subject. But what is more important, the farmers are satisfied that the eradication of tuberculosis is a sound economical proposition, as many have testified. Many of them are getting about £10 more per cow annually for their milk.

There are two main objections raised against this scheme: (1) That it increases the cost of milk to the consumer, and (2) that the reacting cows are only distributed to other centres. I will deal briefly with these two points. (1) It will be admitted that the production of the 1st grade, i.e., certified milk, entails considerable expense, and that owing to this we can hope to have only a certain number of such herds which will supply milk for invalids and others where the cost is not considered. But there is no reason why Grade A tubercle tested milk should not be used by all—if all the medical practitioners would advise it for their patients and institutions there would be such a demand that the scheme would spread all over the country. It has been proved that the producer can obtain a profit if he sells it wholesale at threepence a gallon above the ordinary price and the retailer if he sells it at one penny a quart above the usual price. If the keeping properties are considered it will be found cheaper for the consumer to pay this small additional amount as there is less souring, less waste and the milk is more appetizing and nutritious. (2) As to the second objection I admit that at present there are grounds for dissatisfaction; for although in many cases the cows are isolated and fattened, and in other instances they are isolated at another farm, nevertheless, many are sold in the open markets. But they are usually only bought by people who are *not* going in for tested herds and who either know or have a shrewd suspicion that they are reacting cows. So that gradually they are collected together into tuberculous herds and the medical officer of health will know where they are. He will also know where he should be especially careful to take frequent samples of the milk for bacteriological examination. When the Tuberculosis Order comes into force many of these cows can be dealt with. It may be possible in the course of time entirely to eliminate all reacting cows from such a district by slaughter and compensation as is done in America, where about a third of the United States cows are free from tuberculosis, though of course the methods employed there would not be applicable to this country. At any rate, a few accredited areas, as they are called, may in time be formed where people wanting tubercle-free cows could come and replenish their stock. In fact, now there are several people who sell annually about 100 heifers, down calving, which have been tested once, twice or three times and are free from tuberculosis, and there is a great demand for these animals.

It would be a great help if the Ministry of Agriculture would give a certificate in these cases to such accredited herds. We must also insist that a Tuberculosis Order shall be re-introduced without further delay; not only has this been asked for repeatedly by the veterinary and medical professions, but even by the farmers themselves and local authorities throughout the Kingdom.

The disease, from my own observations, is far more contagious among bovines than among human beings. One or two cows can infect a whole herd very quickly and very few cows appear to have any immunity from the disease. Only last week I found one tuberculous cow introduced into a herd had infected every other cow in it, though six months previously they all passed the test. I have found as many as 98 per cent. in a herd affected kept under the best possible conditions, and in one herd chiefly of young cows, numbering about 150, 87 per cent. were affected. Also owing to the short life of the cow I am convinced that very few recover. The loss from carcasses of cattle and pigs condemned for tuberculosis must amount to many thousands of pounds annually. In Edinburgh alone it amounts to £5,000 per annum and the saving under this head would be very considerable.

The cost of enforcing this Order would be trivial compared with its benefits.

should like to propose a resolution from this meeting calling upon the Government to re-introduce at once a tuberculosis order.

During the short period, about twelve months, throughout which the 1913 Tuberculosis Order was in operation 8,073 diseased animals were dealt with in Great Britain and of these 16 per cent. of the English cases and 29 per cent. of the Scotch cases were disseminating tubercle bacilli in the milk, i.e., about 1,500 tuberculous milk samples were traced to their source and the cows slaughtered—this is a great record. Also, nearly 8,000 premises were visited and the cows inspected—very beneficial from an educational standpoint. I was one of those who helped to carry out the Order and I can testify to its utility so much so that I should like to see all cows inspected at regular intervals by a veterinary surgeon not only in order to find clinical cases of advanced tuberculosis but also to report on conditions likely to affect the milk injuriously. This could easily be arranged for under Orders in connexion with the Milk and Dairies Act.

One must educate the man on the spot and show him not only how to obtain a pure milk supply but how he can do it profitably to himself. The British farmer will not be coerced, but he can be led.

Advances have also been made in other directions. The Ministry of Agriculture has established a scheme for immunizing bovines on the lines suggested by Calmette and others by inoculating them as calves and then annually with attenuated living cultures of the tubercle bacillus. Dr. Nathan Raw is also working with dead cultures. Time will not permit of my discussing these methods, which at present are in the experimental stages, but they offer great prospects of so increasing the resisting power of bovines that they may in time eliminate the disease. *The great objection* to some of these methods is that the animals then react to the tuberculin test, so that it will be very difficult to say whether they are naturally infected or not.

The Milk and Dairies Act, when it comes into operation, will be of great assistance, as it will enable more samples to be taken of the milk, so that infected milk may be traced to its source and the diseased animal dealt with. In this work there must be *close co-operation* between the Medical Officer of Health and the Veterinary Inspector; each have very important work to do and it is only by the help and hearty goodwill of both that the working of the Act will be rendered successful.

To sum up I would urge:

- (1) The encouragement in every way possible of the production of one of the special grades of milk—particularly the Grade A tuberculin tested milk.
- (2) The regular inspection of all milch cows at least every three months.
- (3) The immediate re-introduction of the Tuberculosis Order.

If these are carried out, it will, I earnestly believe, go a long way towards the solution of this very pressing and difficult problem.

DR. STENHOUSE WILLIAMS

said that that afternoon he had been taking a visitor round to see how milk was being produced in Berkshire. On going into the cowsheds they were seen to be quite clean, ordinary cowsheds with no white tiles; but they were provided with light, water and steam, which were intelligently used. The cows were brought in and washed, then the men went away and changed their coats and washed their hands, came back and milked the cows. The stools upon which they sat were clean, and the milk was put into sterilized, covered buckets. It was then transferred to a container which had been sterilized, and from the container it passed over a cooler—which had also been sterilized—into a sterile 10-gallon churn, unventilated; this was sealed, marked, and sent away to the retailer. The retailer took the milk from the churn and put it into a sterilized receiver and over a sterilized cooler if it had travelled any distance. The milk was then put into sterilized bottles, which were capped and covered with the Government seal, "Grade A Milk (Tuberculin Tested)."

For all that work he (Dr. Stenhouse Williams) paid 1d. a quart more than did the ordinary person, and for it he obtained milk from cows which were tested every six months, and it was guaranteed to be within a certain bacteriological standard. He received his supply of milk once a day, namely, at seven in the morning, and it was always the previous day's milk. In three years he had never had a sour sample. Could the farmer do anything better for the people in this country?

Until 1913 it was nobody's business in Britain to sit down and study the farmer's problem from the farmer's point of view. But when, in 1913, one farmer found what could be done, he took the matter up, though it did not promise much financial reward. He cleared his herds of tuberculous cows, carried out tuberculin tests, put in steam, and proceeded to learn the best methods. He then offered his milk to the local hospital, at the same rate as ordinary milk, and his contract was turned down. He asked his hearers to consider what that meant as a reflection on the profession. Then the farmer tried to sell this milk to a dealer, but the dealer would not have it either. So in the end the farmer, who was not rich, had to put his hand into his pocket once more and start a dairy business on his own account so as to get his milk on the market. Within two years of getting it on the market at Reading, that milk formed one-sixth of the total milk supply of the town, and always at the extra price; and the poor man bought as much of it as the rich man. Last March there were already 600 gallons a day of such milk going into Newcastle. There had been great trouble in getting that milk introduced into London, for reasons he would not now enter into, but one was that the doctors had not given the movement the support they might have done. But St. Bartholomew's Hospital was being supplied with this milk, and in Bermondsey, where the medical officer of health was interested in the matter, the consumption of this special milk was increasing. The question was, did the doctors want it?

What was the alternative to accepting such a farmer's offer? That this country should follow in the steps of New York. In New York, after twenty years' work, 2,750,000 American quarts of milk were received daily, but only 50,000 quarts of that was fresh milk, the whole of the remainder being pasteurized, either Grade A pasteurized, 500,000 quarts, or Grade B pasteurized, 2,200,000 quarts. The results gradually emerging in America made one pause before thinking of committing this country to such a scheme. Hess, for instance, was asking himself whether the latent scurvy among their children was not due to an excessive consumption of pasteurized milk. Recently there appeared in the *Journal of Biological Chemistry*, a paper the point of which was that milk pasteurized by the "holding system" when fed under control conditions in welfare centres, did not give the increased weight which was to be expected; it was found better to give fresh milk which had been boiled. In Denmark there was the increase in xerophthalmia coincidentally with the diminution in the consumption of butter-fat by the inhabitants. These things made one wonder whether it was wise to tell the people of England that the future of the country lay between certified milk at a price the people could not afford to pay, or, as an alternative, pasteurized milk. It was surely better to foster the consumption of milk free from tuberculosis at a reasonable price, delivered fresh. The problem in this country was much simpler than it was in the United States, where the distances over which the milk was transported, and consequently the haulage time, were much greater than here; the extremes of temperature were also much greater in the United States. Further, in America the Public Health control was not so stabilized as in this country owing to the fact that the people are continually moving into new districts over which it is very difficult to exercise adequate control. The typhoid death-rate was six times higher than in this country. Obviously it was typhoid that Dr. Parkes feared in reference to the New York milk supply. Moreover, in this country the farmers were rising to the requirement, and were producing a milk

which was good for them and for their workmen. He appealed to his hearers to see that the milk he had described was supplied to them.

Sir LAYTON BLENKINSOP

said that many members of the veterinary profession were somewhat disappointed at some of the remarks made at the last meeting; members of the medical profession had declared they were satisfied with pasteurized milk.

He would make a few remarks on the eradication of disease among Army animals, as this might throw some light on the removal of disease from farm herds. Dr. Rowlands' remarks, however, expressed much of what he had intended to say, to the effect that if animals were placed in hygienic conditions and under proper control by practical veterinarians, disease would be eradicated from them with little difficulty. Dr. Rowlands seemed to attribute rather too much importance to vitamins in the diet. The efficient veterinary control of animals was a matter to which the public did not attach sufficient importance. The best way to help the farmer was to place at his disposal veterinary advice free from bias. No body of men in England were more keen to follow good advice than farmers, especially if they could be assured of its value. In 1912 he had had under his supervision a small herd supplying milk to a localized area. He wished to have the herd tested, but he received the usual answer: that pasteurized milk was good enough. It did not seem to matter whether the disease was got rid of or not; still, by mere supervision of that herd, tuberculosis disappeared from it. The employment of tuberculin was often disparaged. Many of the unfortunate results attributed to the use of tuberculin were due to the fact that men able to interpret the symptoms manifested were not employed in its administration. He was aware that was a serious statement to make, but he was prepared to be questioned on it. The same applied to the case of glanders in horses, for there were a number of horses which were doubtful reactors to mallein. If cattle were placed under proper hygienic conditions he was sure they would acquire a special immunity from disease. Perhaps this was not practicable at the present time. The farmer deserved all the help these two professions could give him in the shape of sound advice. It was unlikely that a Bill could be got through Parliament legalizing wholesale slaughter of cattle, nor did he think it was necessary at the present time; but he fully agreed with Mr. Male that the re-introduction of the Tuberculosis Order was absolutely necessary.

Dr. DAVID NABARRO

said that he would revert a little to the attitude taken up on this question by Professor Cummins at the last meeting. In connexion with such a subject as this, with so many interests concerned, it was desirable to move cautiously, and certainly it was necessary to make sure of the ground before expressing definite opinions upon which the authorities might take action.

The first interest to be considered was that of the farmer, whose diseased cattle it was proposed to slaughter, and who would doubtless require compensation. Secondly, there was the interest of the taxpayer, who had eventually to pay the compensation; and, thirdly, there was that of the milk-drinking community, particularly the infants, and children generally.

It seemed clear from the discussion that herds could be freed from tuberculosis, and perhaps kept free from that disease for a time. But once a herd was free from the disease its immunity to it would be lowered. Possibly the method of feeding pursued by Dr. Rowlands helped in greatly raising that immunity. The advent of a strange milch cow to the neighbourhood could easily start the disease again in a herd which had been free.

Therefore, though it was possible to rid a herd of tuberculosis, was it desirable

to do so? He was not convinced that it was. The frequent taking of small doses of killed tubercle possibly conferred an immunity, and that was what was constantly being done by the people. Unless it were so, many more people would contract the disease acutely, and die from it. Boiling of milk would kill the bacilli, or if any were left after that they would be too few to cause harm. Of course it was realized that a cow which was suffering from gross tuberculous disease of the udder should not be allowed to furnish milk for mankind. But were all reacting cows to be condemned? In the present state of knowledge such a conclusion was going too far.

The dosage of bacilli had an important bearing on the likelihood of spreading the disease; therefore, as Professor Cummins pointed out, the dilution of milks must be a factor in reducing the number of bacilli consumed. When children were fed with the milk of one or two tuberculous cows, those children were more likely to develop the disease than were those receiving mixed milks.

He agreed with all the speakers as to the need for a clean milk, and that could be ensured by the exercise of a little care. There should be no gross pollution with faecal matter, or with pus from the udder.

The relation of bovine to human tuberculosis was an extremely difficult problem. Dr. Stanley Griffiths' screen tables gave a vivid demonstration of the fact that children under five years of age were much more susceptible to bovine tuberculosis than were adults, and that as age advanced the human bacillus was more in evidence. That fact was well known, and it was capable of one or two explanations. The first, the one Dr. Griffiths believed in, was that the child was more susceptible to the bovine bacillus than was the adult. The second, which had not been entirely disproved, was that in the adult the human type was chiefly found because it had become converted into that variety. He (the speaker) believed in the mutability of organisms; when a bacillus had been living in the human body, and had been taking its pabulum and sustenance from that body, for years, it was quite conceivable that it might change its characters accordingly. That was why, in phthisis pulmonalis, there were only three cases of the bovine form to 200 of the human form. This question of the conversion of one bacillus into another was so important that it deserved close attention. In 1921 Professor Kolle and two collaborators published a research into acid-fast bacilli other than those of tuberculosis, eight strains of which were known to occur in milk, cream, and other milk products. Though, in the ordinary way, these bacilli were not pathogenic for animals, by passing them through guinea-pigs their virulence was raised, and those organisms then acquired the cultural properties of the true tubercle bacillus. These acid-fast bacilli grew rapidly in culture media. He thought those experiments ought to be repeated on account of their importance. Sanfelice, an Italian, worked at the same subject and found the same thing happened. This should be either confirmed or refuted by further work. Twenty years ago he (the speaker) did some experiments with acid-fast organisms in butter, for the Local Government Board. In only a few cases did he find tubercle bacilli, but he isolated a series of twelve of these acid-fast bacilli which looked like those of tubercle; some of them could stand decolorization with 25 per cent. sulphuric acid, and could produce lesions in the guinea-pig when injected with butter. If inoculated into a second series of guinea-pigs there were no lesions if they were mere acid-fast bacilli, but if they were those of tubercle there occurred tuberculous lesions in the second animal. Some were morphologically identical with tubercle bacilli, but they grew in a day or two, instead of in three or four weeks. In all probability these organisms were found in milk and sometimes called tubercle bacilli. Beattie said he had found milk to contain numerous acid-fast bacilli which were practically indistinguishable from those of tubercle, and in the cows yielding the milk clinical evidence of udder disease. He inoculated the

bacilli into guinea-pigs, but the animals gave no evidence of tuberculosis. In other cases Beattie found bacilli similar to those present in butter.

There were still so many unsolved problems associated with this subject that he (Dr. Nabarro) considered more work should be done, perhaps by the Medical Research Council, or by the Ministry of Health, before recommending that enormous numbers of cattle should be slaughtered, except in cases in which gross tuberculous lesions were present.

Dr. NATHAN RAW.

The provision of a pure milk supply is one of the most difficult problems of our time. It is surrounded with controversy and conflicting opinions, whilst the cost of producing a pure milk for consumption for the masses is quite prohibitive.

Tuberculosis is the greatest scourge in the country to-day, and the loss to the farmer and breeder is enormous every year, whilst the untold misery to the human race cannot be estimated. We are discussing to-night the relation of the milk supply to tuberculosis.

It is now generally accepted that a very large amount of tuberculosis is directly conveyed to children and young adults by milk infected with tubercle bacilli. I myself hold the view, based on clinical experience, that nearly all surgical tuberculosis is bovine in origin and has no relation whatever to pulmonary tuberculosis. It is estimated that 10,000 children are lost annually in the British Isles from this cause alone. It has also been stated that a million dairy cows are at present tuberculous, and of that number about 5 per cent. are infected in the udder, rendering the milk highly dangerous. We know the nature and magnitude of the problem, but we seem powerless to apply the remedy.

It would obviously be no solution to the problem were we to slaughter all the infected animals, as a new generation is being infected all the time. From the point of view of reducing this number of diseased cows the only hope seems to be in directing our attention to the immunization of calves as they are born, with a view to their being able to resist the ordinary infection to which they are exposed.

Until this can be effected, I am strongly in favour of compulsory sterilization or even boiling of all non-certified milk exposed for sale.

Personally I have never seen any ill-effects on nutrition or otherwise in children from drinking boiled or pasteurized milk, and it is infinitely preferable to milk containing living tubercle bacilli. I am strongly in favour of the Tuberculosis Order being put into force as soon as possible, as it is bound to have a good effect, and I hope this meeting will pass a resolution to that effect.

Dr. H. SCURFIELD

said he had been carefully over the experiences of the towns in which the Milk Clauses were in operation, and there was no evidence that the weeding-out of cows which were suffering from tuberculosis of the udder had had any effect in diminishing the amount of bovine tuberculosis. Professor Penberthy read a paper last December before the Farmers' Club in which he said tuberculosis was as prevalent now among bovines as it was twenty years ago. In Sheffield the experience was that, with the combined veterinary inspection of the cows and the examination of the milk, in stalls for 100 cows, there would be one cow with a tuberculous udder every year. The cows might be in the stalls only nine months, so that would not give quite one per 100 cows. Apparently tuberculous infection had a great tendency to creep into herds which had been made tubercle-free, and that had been the experience in Birmingham and Denmark. It was useless to attempt to maintain a tubercle-free herd unless breeding was carried on within the herd. Those farmers who purchased fresh cows had reactors every six months. Did that mean that at the time of the importation the infection was not established enough to give a reaction, but that it subsequently

advanced to that stage? Or did it mean that the cow in question had practically been "doped" by the tuberculin given some time previously for testing? It had been notorious for thirty years that, in certain cases in which cows were tested with tuberculin, if they were tested a second time within a few weeks they might not react. In 1896, at the Sanitary Institute, he proposed that the Government be asked to protect the tuberculin test by standardizing the tuberculin and enacting that it should only be applied by a competent veterinary surgeon, and that every cow which reacted should be "tagged." This was unanimously carried. "Tagging" did not mean that the cow should be slaughtered, but ensured that it was not passed on to another herd as a sound animal; it meant also a more careful inspection at the abattoir, or wherever it was slaughtered.

In Denmark, since about 1890, the Government had assisted a scheme for clearing herds of tubercle, and in that country matters were not progressing very quickly. Dr. Rowlands' results supplied some of the most interesting facts which had emerged from this discussion; and it was well worthy of consideration whether, having obtained a herd free from tuberculosis, feeding it with foods rich in vitamins was not the best means of maintaining that high standard of health. Dr. Stenhouse Williams also had shown wonderful results, and there should be many Reading Institutes throughout the country.

With regard to the increase of xerophthalmia in Denmark, when too much butter was exported and the inhabitants had to eat vegetable margarine, he believed that all the Danish butter was made from pasteurized milk; therefore this argument supported the value of pasteurized milk.

He believed that whenever a sample of milk produced tuberculosis in the guinea-pig, if its source was traced quickly before the cows had been got rid of, it would be found that there was nearly always a tuberculous udder to explain the sample. He mentioned this because a prominent representative of the Ministry of Agriculture, at one of the recent milk conferences, seemed to be under the impression that the faeces of the cow were the chief source of tubercle bacilli found in the milk. He thought veterinary surgeons skilled in detecting udder disease should be engaged in this work. He did not believe that veterinary control and the adoption of hygienic measures could eradicate tuberculosis from cattle, or that skilled veterinary control could easily, under present conditions, maintain herds tubercle-free, after the reactors had been eliminated.

Professor F. HOBDAY

said that the Society had heard authorities on each side of the question: from the medical side, from the veterinary side, and from men whose life-work had been devoted to the matter. He wished to remark on something which Dr. Scurfield said, namely, the question of "doping" cattle. He called attention to a resolution which he had brought forward and which was carried at the meeting of the Council of the Federated and Allied Medical Societies a short time ago, to the effect that it should only be possible for tuberculin to be obtained by qualified medical men and veterinarians. The reason was that it was universally known that if a man chose to be unscrupulous he had only to buy a syringe and get some tuberculin through his local chemist or elsewhere, and to give an injection within a month of the official testing of the herd, to prevent a positive reaction in his infected cows. That subject had been hotly debated in several districts of Great Britain, where this sort of practice was stated to have been carried out.

Dr. Nathan Raw's idea of immunizing calves when they were born, by means of dead bacilli, had the advantage that animals so injected could be used for Grade A milk, because if the result was satisfactory they did not afterwards react to the tuberculin test. This method had been tried for the past two and a half years, and the results up to the present time had been very encouraging. The Ministry of

Agriculture had also under consideration Professor Calmette's method. If stock owners could give immunity to the calves as they were born there was a reasonable prospect that in twenty years at the most the herds of this country would be free from tuberculosis.

Mr. HENRY GRAY

said it had not been stated what was done with the reactors. He believed that the tuberculin testing now practised was spreading the disease all over the country. Farmers sent reactors to the open market to be sold, other farmers bought them and incorporated them into their milking herds, and in a few months many of them died of the disease. He advocated total eradication by slaughter. Therefore he considered that the cattle in the country should be placed under veterinary control, and those which reacted to tuberculin should be at least branded, isolated and fattened if not killed at once. While the twenty years mentioned by Professor Hobday were elapsing, children would be dying of the disease, and many more have it in a crippling form. By keeping it from children, 3,000 to 10,000 deaths a year would be prevented. Slaughter of diseased animals would also be the cheapest method in the long run. The loss due to tuberculous cattle must amount to some millions every year, and if his plans were adopted, in ten years the money would be regained. It was not doing the proper part by the nation to continue the present, or to carry out the proposed, tinkering methods. The medical profession must do its duty in the matter of purity of food, and the question of cost must be settled by Parliament and the public. Tuberculous milk from cows was often given to pigs, with the result that they, too, contracted the disease. If tuberculosis were eradicated from cattle, he thought it would soon disappear from other animals as well. The more pedigree cattle free from tuberculosis there were exported, the greater would be the proportion of diseased animals left. In America, where all the cattle were being tested, the reactors were killed, and the farmers compensated. Three or four tests were applied before the herd was passed as "accredited" or free from tuberculosis. Vaccination, if proved to be a certain preventive, was all very well, but even its adoption would only be tinkering with the subject.

Sir WILLIAM HAMER

said that the discussion on this subject had been one of the most instructive held by the Society for some time.

One speaker said there were two sides to be considered, but he (Sir William) agreed with the view that there was only one side. He recalled the fact that Power, Shirley Murphy, and Bulstrode, always maintained that the way to deal with tuberculosis was by those very methods which were now being advocated by Dr. Rowlands and Dr. Stenhouse Williams who attached far less value to immunizations and segregations than to what they regarded as so much more important, viz., proper feeding and housing. The benefit of improvement in those matters had been seen in the case of human beings, and it would be very curious if the methods which had been so successful in man, did not also prove successful in the case of cows and pigs. Personally, he wished Dr. Rowlands was not quite so keen on "vitamins"; he, the speaker, thought the all-important points were the adequacy of the diets and the appropriate selection of food-stuffs. But admittedly it was owing to the skill, knowledge, and businesslike methods of Dr. Rowlands that he had been so successful with his animals.

Sir LEONARD ROGERS, F.R.S.

said the meeting agreed as to the results which should be aimed at, namely, providing as much as possible of the Grade A milk. He advised the use of that milk for

all children. It was also agreed that it would be a long time before Grade A milk could be procured in sufficient quantity for the whole population. Therefore it was necessary to consider what other methods there were for preventing infection by tuberculosis, a disease which caused so much suffering and loss of life, especially among children. The pasteurization of the milk seemed the next best step; so that if as much Grade A milk as possible was produced and the remainder of the milk supplied was pasteurized, these would bring about a much better state of things until the measures taken were successful in eradicating the disease from cattle. It had been said that pasteurized milk was deficient in substances which were necessary for the health of the children. In India there was practically no tuberculosis in the cattle, and the milk did not need to be sterilized or otherwise treated to prevent tuberculosis arising from it. Still, except in one or two places, there was no such thing as pure milk in India. The natives diluted their milk with water from the nearest tank, and it was carried about in open cans, to which dirty straw was added to prevent the milk from spilling over the side. And before the roads were watered sweepers stirred up every particle of dust there was, a good quantity of which must enter the milk cans. As cholera, typhoid, dysentery, and such diseases were rampant in India, one could not think of drinking milk until it had been boiled, and the natives did not drink unboiled milk. But in that country one did not find the deleterious results which had been attributed to pasteurization by some preceding speakers.

Dr. J. C. McVAIL (Chairman)

said he quite agreed as to the immense importance of the subject under discussion, and it had been most interesting to hear the medical and veterinary sides put forward. But he did not think these two aspects could be regarded as in any way opposed to each other; for instance, pasteurization and the prosecution of the methods advocated by veterinarians were really complementary. A health officer was bound to be impressed by results attributed to pasteurization in Chicago. If it could be shown that pasteurization did more harm than good, it should not be adopted; but the almost universal opinion was that it was of great value. It was a kind of "truce of God," which allowed other measures for the eradication of tuberculosis to go on while the milk meanwhile being consumed was rendered harmless. The effect of the discussion had been to point to a unity of aim on the part of the medical and the veterinary professions.

Mr. A. PHILP MITCHELL.¹

I have much pleasure in complying with the request of the Royal Society of Medicine to contribute to this discussion on the incidence of tuberculosis of bovine origin in human beings.

One of the most important public health questions to-day is the part played by the bovine bacillus in the causation of tuberculosis in man. Whilst the majority of investigators in the same or different countries admit the possibility of such infection their findings as to the actual extent of the disease are widely divergent.

When considering the results of all such investigations, it is important to bear in mind that special conditions prevail in one country and not in another. The consideration of these in relation to the Edinburgh results I shall deal with later, but what I wish to emphasize at this point is: (1) the possibility of different results being obtained according as the tuberculous material under investigation has been removed at autopsies, or during life; (2) that the importance of the relative frequency of the bovine and human types of infection at the different age-periods has not been sufficiently recognized. Numerous observations on the prevalence of the two types have been made, but these mainly concerned tuberculosis in adults. When the disease has been investigated in children, the tuberculous material has usually been

¹ This communication was taken as read.

obtained from autopsies, and very seldom removed by surgical operation. The only way, therefore, to obtain definite statistical evidence as to the relative frequency of the bovine and human types of infection is to examine material obtained not only from autopsies but also removed by surgical operation from a large series of non-selected cases. When I commenced such an inquiry in 1911 it was originally my plan to occupy myself only with tuberculosis of the cervical lymph-glands and the faucial tonsils. In consequence however of the appointments I subsequently held at the Sick Children's Hospital I was given an exceptional opportunity of investigating cases of abdominal tuberculosis, together with a considerable number of autopsies. I also investigated the relationship of the milk supply to the disease. I was thus enabled to undertake a systematic and broader study of the entire subject.

That surgical tuberculosis among children of all ages is common in Edinburgh and the district is unfortunately evident to all who visit our Sick Children's Hospital. To state the extent of this prevalence in exact figures is by no means easy, but I think the following statistics and clinical data clearly show that the disease is sufficiently prevalent to constitute a real danger to the community and to emphasize the great necessity for some comprehensive and systematic attempt at prevention and cure. Of new cases treated in the surgical out-patient department of the hospital fully eleven per cent. are children affected by one or other form of surgical tuberculosis (glands, bones and joints). These manifestations of tuberculous disease also constitute a very considerable percentage of all cases treated in the surgical wards of the hospital. It is not uncommon to find at any one time that no less than 50 per cent. of the in-patients under treatment are cases of tuberculosis. These figures are submitted with a full realization of the inevitable disabilities attending the purely clinical statistics of any hospital. Such statistics must, however, be accepted as broadly demonstrating the prevalence of the various types of this disease treated and in a general way it is possible to arrive at certain conclusions.

RESULTS OF INVESTIGATIONS.

In the present contribution I propose to discuss the results of my investigations in regard to the relative frequency of the bovine and human types of infection (1) in material removed during life, (2) in material obtained from autopsies in children.

(1) *Surgical Material.*—The cases of tuberculosis now to be considered are those in which the invasion has taken place either through the mouth or throat into the cervical lymph-glands, or through the intestinal mucosa into the mesenteric glands.

First, I wish to call attention briefly to the subject of tuberculous cervical glands in children—a very frequent disease in Edinburgh and district. It must be stated how much one is struck with the numerous instances of tuberculosis of the glands in the upper part of the neck met with in the large number of children, coming for examination, at the surgical out-patient department of the Sick Children's Hospital. Fully 7 per cent. of new cases are children with tuberculous cervical glands. These children as a rule show no clinical signs of tuberculosis elsewhere, and, except for the local condition, are apparently healthy. It is not at all rare for a single member of an otherwise healthy family to exhibit the disease. The high incidence of the disease in children from 2 or 3 years of age is a striking fact. The influence of infectious diseases, notably measles, whooping-cough and scarlet fever in apparently favouring the dissemination of the disease in the glands, is also noteworthy. This fact indicates the great need for children being placed under the best hygienic conditions possible during convalescence from these infectious diseases.

Eighty consecutive cases of tuberculous cervical adenitis were investigated; of these forty-two resided in Edinburgh, and thirty-eight came from neighbouring country districts (within a radius of thirty miles). The ages of the patients ranged from 3 months to 12 years, the average being $4\frac{1}{2}$ years and 50 per cent. of the cases under 3 years of age. Age is a most important factor in such experimental inquiries. Of

the eighty cases, the bovine bacillus was present in seventy-one instances (88 per cent.) and the human bacillus in nine (12 per cent.).

Without exception the cases were those of children of 12 years of age and under. The maximum incidence occurred during the second year of life. This is not surprising when it is recalled that the large majority of children of this age are nourished in whole or in part on cow's milk. I found that, in my series of cases, 84 per cent. of the children 2 years and under had been fed with unsterilized cow's milk since birth. Whatever may be the case in other countries, the mode of feeding children in Britain, especially Scotland, is such as to favour bovine infection. That a large proportion of Scottish babies are bottle-fed, and that it is the exception rather than the rule to sterilize the milk, are factors the significance and importance of which have not been fully recognized by many investigators in other countries. With such frequent localization of the disease in well-defined glandular groups it is natural to consider it as a first stage in the spread of tuberculosis and also to inquire as to the sources and channels of infection.

How do tubercle bacilli reach the glands in the upper part of the neck? Invasion takes place either through the faucial tonsils or the adenoid tissue behind the nose. The position of the tonsils at the entrance to the digestive tract favours infection more with food than with inspired air, a fact of great consequence in considering the frequency with which cervical glands in children harbour the bovine type of tubercle bacillus. Furthermore, the very direct and short pathway between the tonsils and the upper deep cervical glands explains the frequency with which well-marked tuberculosis of these glands may be secondary to a small focus in a tonsil. The faucial tonsils from 100 consecutive cases of children suffering from tuberculous disease of the upper deep cervical glands were investigated. Thirty-eight (38 per cent.) showed histological evidence of tuberculosis in the tonsils. Experimental inoculation of guinea-pigs and rabbits with pieces of tonsillar tissue yielded positive results in 21 per cent., and of these the bovine bacillus was present in sixteen cases and the human in four cases. It should be noted that in the inoculation test the percentage of positive results is somewhat lower than that obtained by microscopical examination of the tonsils. This is explained by the fact that a large number of inoculated animals die from rapid septic infection before tuberculous changes have had time to develop.

Hypertrophied tonsils removed from 100 children with no clinical evidence of tuberculosis of the cervical lymph-nodes or other parts were also investigated. Nine (9 per cent.) of the hypertrophied tonsils examined gave histological evidence of tuberculous disease. The bovine bacillus was present in four cases and the human in two cases. In the remaining three cases the type of bacillus was not determined owing to contamination of the culture tubes.

Let us next consider cases of abdominal tuberculosis in children. I have included only those cases in which the diagnosis is tuberculous peritonitis or tuberculous mesenteric gland disease, and not cases in which the abdominal affection is part of a generalized tuberculosis. Out of eight cases observed, seven proved to be of bovine origin, and one of human origin. All the children were under twelve years of age, and had been fed on raw milk. It is interesting to note that in the human infection the father of the patient suffered from chronic pulmonary tuberculosis.

It is evident from the results which I have briefly outlined that the bovine type of tubercle bacillus plays a significant part in the causation of tuberculosis of the faucial tonsils, cervical glands and mesenteric glands amongst children residing in Edinburgh and district.

In a series of cases of glandular tuberculosis in children yielding such a high percentage of bovine infection it seemed to me very desirable that an exhaustive inquiry should be made as to the prevalence of tubercle bacilli in the Edinburgh milk supply, particularly that portion derived from country districts. As in all large

towns, Edinburgh derives its chief supply of milk from country byres, of which there is practically no veterinary inspection under the existing legislation. The general public know little more about milk than that it is or should be a natural product of the cow, and that it is one of the most important foods for children. The quality of milk (that is, percentage of fat) has generally been the most prominent question with the consumer and the local authorities, so that the more vital subject of infected milk has not received due attention.

The results of such an inquiry which I carried through showed that of 406 samples of milk collected from the same number of milk shops, 82 samples (20 per cent.) contained tubercle bacilli. Practically all milk imported from the country byres reaches the consumers through the milk shops, which number fully 400, whereas the small amount of milk produced in the city byres is conveyed direct by van to the consumers. In testing each sample of milk two guinea-pigs were inoculated.

Children being large consumers of milk, and milk, according to some, forming such an essential part of their diet, it is quite clear how contaminated milk may be a frequent source of infection to them. Small as is the danger in individual cases, the multiplicity of opportunities gives it considerable importance for children. It may also be confidently stated that the reason why many children do not present any clinical evidence of tuberculosis, in spite of the fact that they are frequently drinking infected milk, is to be found in a consideration of the results of certain experiments. These have shown that while inhalation of tubercle bacilli is a certain method of infection even when small doses of bacilli are employed, very much larger doses of bacilli are required to produce the disease by way of the alimentary canal. It is perfectly plain therefore that when the milk is obtained from a single cow with udder tuberculosis or with advanced generalized tuberculosis, or from the mixed milk from a small herd containing one or more tuberculous cows, a child may receive the infection in concentrated form.

(II) *Autopsy material.*—In the first place let us briefly consider (a) the incidence of tuberculosis in the various groups of lymph glands as determined from a series of autopsies in children dying from all causes, and (b) the bacteriological characters of the tubercle bacilli which were found.

The total number of cases examined was twenty-nine, all being children under twelve years of age.

At each autopsy particular attention was directed to the three chief groups of lymph glands—the cervical, bronchial, and mesenteric. Occasionally the tonsils were also examined.

The investigation consisted of (1) gross examination of the glands; (2) inoculations of guinea-pigs with emulsion of glands; (3) microscopic examination of glands.

It is not my intention to state the experimental data in detail.

Summary of Results.—Cultures were isolated from twelve of the twenty-nine cases investigated and tested as to their cultural characters and virulence for rabbits. Eight cases yielded cultures of human tubercle bacilli; in four cases they were of the bovine type.

(A) *Cases Yielding Tubercle Bacilli of the Bovine Type.*—Bovine tubercle bacilli have been isolated from four of the children. Three of these died from tuberculosis, the immediate cause of death being tuberculous meningitis. The other child died from intraperitoneal hæmorrhage, the tuberculous lesions being found at the autopsy. In the four bovine cases there was more extensive and advanced caseation of the mesenteric glands than of any other group of glands.

(B) *Cases Yielding Tubercle Bacilli of the Human Type.*—Human tubercle bacilli were isolated from seven tuberculous children and one not apparently tuberculous child. The tuberculous cases only are considered here.

In each case cultures were derived mainly from the cervical, bronchial, and mesenteric glands. Occasionally, however, cultures were also obtained from other

parts of the body, and the different strains isolated were compared and found identical in cultural characters and virulence for rabbits. Four of the children died from miliary tuberculosis of the lungs and three from tuberculous meningitis. In six of the children the tuberculosis was most advanced in the thorax, the bronchial glands showing the more extensive disease; in the remaining case the disease was equally severe in the bronchial and mesenteric glands, possibly a case of simultaneous infection in the thorax and abdomen.

(C) *Cases with no Visible Lesions of Tuberculosis.*—The bronchial and mesenteric glands of eighteen children who presented at autopsy no visible lesions of tuberculosis were tested on guinea-pigs for the presence of tubercle bacilli. In thirteen of these cases the cervical glands, and in two the tonsils, were also tested.

Tubercle bacilli were demonstrated to be present in a single case in the bronchial glands of a child aged four years; the bacilli were of the human type.

The results of inoculations with autopsy material, on account of the small number of cases investigated, cannot be considered conclusive as to the relative frequency of the bovine and human types of infection. The investigation was interrupted by war service. The results, such as they are, indicate the seriousness of the mortality in childhood directly attributable to tuberculosis. Further, besides showing the greater importance of the human type of bacillus as a contributor to the mortality rate in children, they illustrate also the danger of cow's milk as a source of tuberculosis in children.

PRACTICAL CONSIDERATIONS.

(1) Tuberculosis of the upper deep cervical glands develops from a primary focus in the faucial tonsils much more frequently than is generally supposed. Treatment of tuberculous cervical adenitis by radical measures may justly be claimed to rank amongst the many triumphs of modern surgery. Tonsillectomy is essential in all cases of tuberculosis of the cervical glands in children.

(2) Having demonstrated that a high percentage of the surgical tuberculosis affecting children in Edinburgh and district is of bovine origin, more particularly that which affects the faucial tonsils, mesenteric and cervical glands; that the milk supply of the same area is frequently infected with bovine tubercle bacilli; and having demonstrated that a certain number of deaths occur from this bacillus, I am of opinion that bovine tuberculosis should be considered an important factor in respect to the spread of surgical tuberculosis amongst children.

(3) The establishment of convalescent homes in the country for the adequate treatment of children affected by active tuberculosis is a much-felt want.

(4) For the welfare of any community, large or small, the provision of a pure milk supply is of the utmost importance—clean milk of good quality from healthy cows and protected from contamination. The powers at present vested in our local authorities are inadequate, and it seems to me to be our special duty to prevent this large and entirely preventable amount of tuberculosis amongst children.

What is most wanted is a well-informed public opinion which will demand new and more drastic powers for the authorities, and insist upon these powers being exercised to their full extent.

(5) To suppress bovine tuberculosis it will be necessary to have the co-operation, either compulsory or voluntary, of those who produce milk, and to establish, so to speak, centres of tubercle-free cows, and slowly increase such territories.

The following resolution was proposed by Mr. GEORGE P. MALE, seconded by Dr. NATHAN RAW, and carried unanimously:—

“That this meeting of the Sections of Epidemiology and State Medicine, Comparative Medicine, and Diseases in Children, strongly urges the Council of the Society to press the Government to re-enact the Tuberculosis Order as soon as possible, with a view to reducing the incidence and mortality from tuberculosis in children, and to the provision of a pure milk supply.”

